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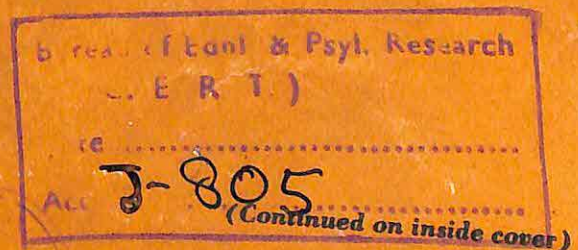
# Contemporary Psychology

A JOURNAL OF REVIEWS

January 1963

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Published by THE AMERICAN PSYCHOLOGICAL ASSOCIATION



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*Contemporary Psychology: A Journal of Reviews* is published monthly. The subscription per year is \$10.00, foreign \$10.50, single number \$1.00.

Published by the American Psychological Association at 2210 W. Dallas, Houston, Texas and 1333 Sixteenth St., N.W., Washington 6, D. C.

Second-class postage paid at Houston, Texas and at additional mailing offices. Accepted for mailing at special postage rate stated in section 132.122, Postal Manual.

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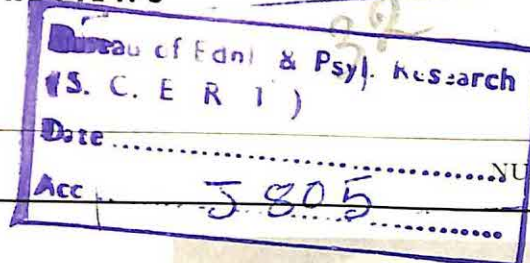


# Contemporary Psychology

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## Beyond Game Theory

Anatol Rapoport

*Fights, Games, and Debates.* Ann Arbor: University of Michigan Press, 1960. Pp. vii + 400. \$6.95.

Reviewed by FRANK RESTLE

Anatol Rapoport, the author, is a mathematical biologist who came to this country from Russia in the early 1900's, received his PhD from the University of Chicago, and, since 1955 has been Professor of Mathematical Biology and Senior Research Mathematical Biologist at the University of Michigan's Mental Health Research Institute. His earlier books include *Science and the Goals of Man* (1953). The reviewer, Frank Restle, is, with only very minor changes, the same Frank Restle who reviewed *Suppes and Atkinson's Markov Learning Models for Multi-Person Interactions* (1960) CP June 1962, 7, 237) and who authored *Psychology of Judgment and Choice* (1961) CP, Sep. 1962 7, 318). is now Professor of Psychology at Indiana University and continues to be a theoretical and mathematical psychologist. He has worked on concept formation, discrimination learning, judgment and choice, individual and group problem-solving, and on the experimental study of open and closed belief systems. His main working hypothesis is that clear and definite theory can be written on many topics to answer a variety of questions, and is not the prerogative of S-R, mechanical, or

any other particular conceptual standpoint; nor is it limited to measurements of threshold nor to the phenomena and principles of "simple learning."

IN twenty years, Game Theory has developed from a mathematical curiosity to a technical specialty, and, as a modern theory of competition, Game Theory has an almost official status among the elite of the Western world. It is the "scientific" theory of conflict.

This book studies conflict in a broader context. The title is apt, for Rapoport's thesis is that conflicts may be of three kinds: "fights" intended to destroy the enemy; "games," which end when the opponent, outplayed within a system of rules, concedes defeat; and "debates" which terminate in a meeting of the minds, when the antagonist is convinced and becomes an ally. These concepts of conflict bring to mind (after some gentle prodding by the author) the demand for "unconditional surrender" which made World War II a fight; the maneuvering of threats, brinkmanship, counterforce- and countervalue-strategies of the militaristic aspects of



ANATOL RAPOPORT

the Cold War (concepts openly based on Game Theory); and what is hopefully called "The War for Men's Minds," the current debate which, as of this writing, has not dissolved in thermonuclear explosion.

In Part I of the book, the theory of fights is exemplified by Richardson's mathematical models of the 1940's, which describe an arms race (antagonists stimulating one another to new efforts, until war erupts), the epidemic spread of war fever, and various models of classical economic competition, ecological competition, symbiosis and parasitism. Rapoport properly criticizes these theories of "Social Physics" for using a deterministic "differential equation" approach, and because they give only the external shape of things without plumbing to the decisions which underlie conflict.



Part II of the book deals with Game Theory in the conceptual non-mathematical fashion of Luce and Raiffa. The discussion and critique are beautifully done, and Rapoport points with a sure finger to the collapse of strategic thinking when applied to situations which involve an element of cooperation along with competition. Rapoport states a strong thesis; that game theory does not quite make sense in non-zero games and in N-person games, specifically because no such games involve cooperation whereas the basis of game theory is pure competition. This position is harder to defend than the more diffuse criticisms of Luce and Raiffa, but Rapoport makes a convincing case.

Part III of the book is, so far as I know, original, for it is a systematic analysis of conflicts in which the participants try to convert one another.

THE theory begins in psychology. Learning and perception, says Rapoport, involve selecting certain aspects of a situation as important. Here Gibson and Gibson, and Donald Campbell might have been cited, and orienting and observing responses, hypothesis and expectancy theories, might have been brought to bear; Rapoport satisfied himself with more clinical sources, especially Carl Rogers. Conflict arises when opposing groups select different aspects as salient in their views of the important issues of the day. Commitment to an ideology involves, in Rapoport's view, blindness to the categories and values of the opposition.

If my opponent is blind to my values, I have a few recourses. If he is in my power (a prisoner or a patient) I can "brainwash" or use psychotherapy. The different terms reflect the social context more than they do the procedure. If he tends to believe me but is not in my power, I can try to explain away his cognitive selections by pointing to their origins, as does a psychoanalyst (appealing to the id) or in the Marxist manner (appealing to class values). But what if he is not in my power and tends to disbelieve me? The question is apt, for certainly East and West are in about this position.

The problem of conflict under these

conditions is what Rapoport calls debate. His proposals cannot be summarized in a few lines, but they include (1) "Conveying to the opponent that he has been heard and understood," (2) "Delineating the region of validity of the opponent's stand," and (3) "Inducing the assumption of similarity." This last, which is crucial, is the step of getting the opponent to enter the spirit of the debate by listening to our side and delineating the region in which our arguments are valid.

Step 1 has a clear criterion—I state my opponent's case so well that he must capitulate and agree that I have stated it well.

Step 2 depends upon the assumption, nicely illustrated, that any sentence in natural language (or, for that matter, in mathematics) which is not a clear-cut formal contradiction, has a "region of validity," which is to say, a set of conditions within which it is true. As Rapoport points out, our purpose may be to show where the opponent's position is *invalid*, but it is logically equivalent and psychologically preferable to show where it is valid.

For Step 3 Rapoport provides, in the last two chapters of his book, a beautiful illustration by giving "The Case for Collectivism" and "The Case for Individualism," employing his own principles with unusual if imperfect success. At the end there is a bare suggestion of how or why such debate should solve anything. By studying conflict, he says, we may change ourselves.

FIGHTS give way to games when the protagonists take account of the values of their opponents and thereby gain a partial control and prediction of the opponent's actions. This advantage makes game theory somewhat more moral and considerably more effective than the fighting orientation. Games give way to debates when the protagonists both can gain or both can lose (as in the arms race now; both East and West can gain by throwing their industrial might to useful purposes, and both can be destroyed). It is certainly moral to seek a meeting of the mind; but is Rapoport's method of debate effective? If I choose to

game, my opponent can either game or lose. He is forced to game. But if I choose to debate, can I drag my reluctant opponent into the debate?

Certainly the exercise of Rapoport's steps 1 and 2, stating the opponent's view and finding its region of validity increases my own breadth of cognition and helps in finding common ground. But steps 1 and 2 are also a blueprint for a deadly subversion. The opponent cannot refuse to listen to his own position well stated and defended, but my statement of it threatens to undermine his morale and political support. He must enter the debate. Also, it is to his advantage to enter the debate. Recall the experiment, in the Siegal and Fouraker monograph on bargaining, in which participant A knows the payoffs of B for the several possible deals that might be made, but B does not know the payoffs of A. In a competitive sense, A is at a terrible disadvantage for he cannot obtain agreement from obstinant B for an equitable deal; but it is even more likely that no deal will be made at all, to the disadvantage of both. A should tell B the facts, and it is prudent for B to listen and learn. Would they, then, debate?

Rapoport's concept of an ethical debate makes sense. Unlike most current thinkers about conflict, he combines a profound analysis of the problem with a well-defined course of action. The book is a guide for makers of policy and leaders of opinion who would work for a peaceful world. For psychologists, it is a rich source of ideas. For significant research on the problems of our time.

## Statistics and Spice

R. S. Rodger

*Statistical Reasoning in Psychology: An Introduction and Guide.* London: University Tutorial Press 1961. Pp. viii + 204. 15s.

Reviewed by ROBERT PERLOFF

The author, R. S. Rodger, received his



MA from the University of Edinburgh in 1956 and his PhD from Queen's University in Belfast in 1961. He has been a lecturer at Queen's since 1956 but in September, 1962, he moved to Australia to take up the post of Senior Lecturer in Psychology at the University of Sydney. The reviewer, Robert Perloff, received his PhD from Ohio State in 1951 and soon went to work as a research psychologist in the Department of the Army, serving as Chief of the Statistical Research and Consultation Unit of the Army Personnel Office. Then for four years he served as Director of Research and Development for Science Research Associates in Chicago but in 1959 he retreated from the harsh world of government and industry to take up an associate professorship at the department of psychology at Purdue University, where, the chances are, he is working harder than he ever has in all of his life.

ANY ONE seeking a low calorie but high protein acquaintance with the fundamentals of modern statistics should examine Rodger's lean though generally solid treatment of much of the "reasoning underlying the simpler basic statistical techniques."

While modest in length (166 pages, exclusive of bibliography, exercises, tables, and index), this Lilliputian book (approximately 5x7 inches) is remarkably Odyssean in its trustworthy reconstruction of the conceptual bricks and mortar of psychological statistics.

For instance, the constraints of brevity notwithstanding, the author treats creditably 1) the properties of scales of measurement; 2) the nature of graphic methods, with a digest of rules for constructing graphs; 3) the nature of and differences among the more frequently encountered statistical distributions; 4) randomness and probability; and 5) statistical hypotheses, together with their decision errors. Indeed, Rodger's succinct treatment of psychological probability vs. statistical probability, unbiased and biased estimators, and one-tailed vs. two-tailed tests, may well communicate more effectively to bemused students than heretofore more elaborate accounts of these concepts.

Criticisms of the Rodger book must be tempered with a reiteration of his objective to "encourage understanding and guide further reading rather than to develop a rule-of-thumb technique." Still, one might regret with some justification his failure to discuss such essentials as the simple analysis of variance, nonparametric statistics, (per) centiles, or correlational techniques beyond the Pearson product-moment. The author does not deny the importance of these topics; rather, he ignores them, for they are beyond the scope of the book. Unfortunately, they are not beyond the scope of the typical reader's statistical needs.

The aforementioned topics, and more, are covered variously by other introductory statistics books, viz., those by Blommers and Lindquist, Downie and Heath, Edwards, Guilford, Hoel, McNemar, and Walker and Lev. These books are generally capable of standing by themselves, insofar as the "blood-and-guts" of statistics are concerned, the brand of elementary statistics enabling an individual to face his data and, with efficiency, do something with them, descriptively, inferentially, and interpretatively. Rodger's book, lamentably, is not adequate for this kind of operation. It must be followed, or accompanied, by one of the foregoing basic texts, which concentrate on *how* to analyze and interpret data.

(Incidentally, the answers to exercise 14 in the back of the book are incorrect, although this will be noted in an erratum slip in future copies of the book. And curiously, Rodger's otherwise current bibliography includes Guilford's 1936 edition of *Psychometric Methods*, rather than the second (1954) edition.)

*Statistical Reasoning in Psychology* tends to grow on you. At first, this reviewer's superficial appraisal was negative to lukewarm. Only after recognizing the author's objectives, accepting rather than fighting them, and acknowledging a place on the psychologist's shelf of a compact 'what-and-why' along with the more conventional 'how' books, did he begin to appreciate the value of Rodger's efforts. To many psychologists the judicious brevity of this book may be a blessing, not a handicap, especially if they share

Thoreau's sentiment that "It is life near the bone where it is sweetest."

## See Appendix C

Richard A. Beaumont and James W. Tower

***Executive Retirement and Effective Management.*** (Industrial Relations Monograph, No. 20) New York: Industrial Relations Counselors, 1961. Pp. viii + 248. \$7.50.

Reviewed by ALLYN M. MUNGER

Both authors, Richard Beaumont and James Tower, are members of the staff of the firm Industrial Relations Counselors. Neither is a psychologist. The reviewer, Allyn Munger is a psychologist, one who took his PhD at Tulane University in 1951 and moved into industrial psychology. He worked first as a research associate with Richardson, Bellows and Henry and later in a similar capacity for Esso Standard Oil, in South America and for the Standard Oil Company of New Jersey. Now he is a staff psychologist with The Psychological Corporation.

THE PRIMARY purpose of this book is an attempt to focus on the problems faced by corporations in their retiring of executives. It tries to examine the practices and problems and stems from a mail survey of 750 companies. Its impact as a research study is reduced considerably in that only 228 companies supplied answers full enough to be used. However, it can be estimated that no real harm has been done, since few conclusions are drawn from the study, and most of the text is based on personal interviews carried out in 46 companies following the questionnaire.

According to the authors, there seem to be two major approaches to retirement: those companies with a flexible retirement age for executives, and those with a mandatory retirement age. There are advantages and disadvantages, as could be expected, to either approach.



# Linguistic House of Many Mansions

Sol Saporta (Ed.), Prepared with the assistance of Jarvis R. Bastian  
*Psycholinguistics: A Book of Readings*. New York: Holt, Rinehart and  
Winston, 1961. Pp. v + 551. \$7.50.

Reviewed by CAROLYN K. AND ARTHUR W. STAATS

*Sol Saporta, editor of the present volume, is Associate Professor of Romance Linguistics at the University of Washington who has had an interest in psycholinguistics since his 1953 participation in the SSRC seminar mentioned in the review. Dr. Saporta was helped in this present project by Jarvis Bastian, a psychologist who took his PhD at the University of Minnesota and is now at the Haskins Laboratory in New York. The reviewers, Carolyn K. and Arthur W. Staats, both received their doctoral training at UCLA and both are now at the Arizona State University, Carolyn K. as an associate professor of sociology and psychology, Arthur W. as a professor of psychology. The former is an experimentally inclined social psychologist, with a special interest in communication and attitude formation. The latter is a student of the principles of operant and respondent conditioning as they apply to complex behavior, especially to the development and function of language. The two Staats are now collaborating on 1) a book in the area where their interests meet and 2) a research program in language that presently focuses on the operant conditioning of reading in four-year-old children.*

IN 1953, sponsored by a Committee on Linguistics and Psychology of the Social Science Research Council, there occurred a research seminar to which distinguished scholars from both disciplines contributed. The report of the seminar proposed formulation of the field of psycholinguistics in terms of learning theory, linguistics and in-

formation theory. This seminar, now nine years past, led to the question, at least in the minds of the present reviewers, of whether the seminar would finally yield a vigorous hybrid fruit, or whether the two species might separate, unaltered by any genetic flow.

Now, one of the graduate student participants in that first research seminar has brought us a collection of readings which illuminates where the disciplines are unified, how they may be mutually stimulating, as well as the extent to which they separately approach problems of language. Many of the original members of the Committee or Seminar are represented in this new attempt at interdisciplinary communication; the list includes Osgood, Carroll, Jenkins, Greenberg, G. A. Miller, Leopold, Lotz, and Lenneberg. Many other distinguished contributors are also included; Skinner, Hockett, Chomsky, Quine, Jakobson, Weinreich, and Brown are here, to name only a few.

If the list of contributors were not enough to demonstrate it, a cursory examination of the table of contents will indicate that psycholinguistics is indeed a field of great breadth, encompassing widely different areas of human psychology—from acoustics to child development—as well as various phases of descriptive linguistics. Saporta solves the formidable task of organizing such a field by ordering his 43 selections into the following eight main parts: the nature and function of language; approaches to the study of language; speech perception; the sequential organization of linguistic events; the semantic aspects of linguistic events;

language acquisition, bilingualism and language change; pathologies of linguistic behavior; and linguistic relativity and the relation of linguistic processes to perception and cognition. Through this organization the editor clearly makes the point that similar phenomena are studied within the different disciplines.

Nevertheless, even when the subject matter is the same, diversity in approaches is the most dominant theme of the book. Each part except the first cuts across the two primary disciplines, providing some theory or formulation, as well as reports of empirical research, the latter either in the form of experimental articles or excerpts from reviews of experimental studies. The great diversity and breadth of these selections may be illustrated by referring to the last two sections. Material in the part on pathologies, for example, ranges from an experimental analysis of stuttering in terms of the principles and procedures of operant conditioning (by Flanagan, Goldiamond and Azrin), to a theoretical article by Jakobson which analyzes aphasia in terms of linguistic units. The section on linguistic relativity demonstrates another existing continuum of research methodology, ranging from interpretation based upon naturalistic evidence on the one hand to complex experimental designs in laboratory research on the other. Thus, the topic is introduced with Whorf's persuasive philosophical arguments that as a function of language not all observers are led by the same physical evidence to the same picture of the universe; and the section ends with an experiment by Herman, Lawless and Marshall on the effect of language on subjects' reproduction of visually presented forms—an experimental design with four interaction mean squares.

From the unilateral approaches of many selections, and the lack of integration of the basic principles of the two fields, one might gain the impression that union may be more easily accomplished in name than in practice. Indeed, this suspicion might have been intensified if Saporta had included such selections as Chomsky's (1959) attempt to annihilate in both principle and content Skinner's approach to language.



HOWEVER, lest we overstate the diversity and conflict in the field, we must point to the number of selections that successfully integrate principles and methods from the separate disciplines. Hockett's review of Shannon and Weaver's *Mathematical Theory of Communication* is an outstanding example of potential compatibility and cooperation because it represents a linguist's analysis of the theory's possible contribution to linguistics. Interdisciplinary research of an empirical nature is also represented, e.g., in Berko's paper, *The Child's Learning of English Morphology*, and in Liberman's *Some Results of Research on Speech Perception*. While these two contributions are very different, they both illustrate additional avenues for research combining the structural categories of descriptive linguistics with certain methods and concepts of experimental psychology. (Too recently to be included in the readings, Bachrach and Ross (1961) have also outlined an approach which attempts an integration of descriptive linguistic categories and operant conditioning procedures.)

It was in looking for further seed to cross-fertilize that these reviewers were somewhat surprised to find that there is very little mention in *Psycholinguistics* of the work in verbal learning by Underwood, Russell and Jenkins, Cofer, Bousfield, Noble, etc., although it seems relevant to understanding sequences of language behavior; for example such work seems to have a distinct bearing on the Miller and Selfridge study of verbal context and dependent probabilities, and might clarify as well some phenomena of grammatical order. Furthermore, research in mediated generalization is hardly mentioned although it was a topic of great importance in the early behavioristic approaches to language.

Like most readings, it is unlikely that *Psycholinguistics* was meant to stand alone as an introduction to the study of language. As the text in a course, the Readings would require a basic background in the two sciences involved, or would depend upon that *rara avis*, an instructor who has acquired a mastery of the two disciplines. However, the Readings would make an excellent

supplement in many courses dealing with language.

In conclusion, *Psycholinguistics* appears to be a most important set of studies for the student and scholar of language. As Osgood and Sebeok said some time ago, "the development of any new interdisciplinary field must ultimately depend upon young scholars who maintain in a single nervous system the habits of both sciences" (1954, p. vii). This book may well be instrumental in producing such young scholars for it will provide them not only with some of the most significant products of the two disciplines, but in addition, because of its insightful organization, *Psycholinguistics* should help define the nature of this new field to the end that in-

creasing interdisciplinary communication and research will be stimulated.

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# For Theoretical Psychometricians

G. Rasch

*Probabilistic Models for Some Intelligence and Attainment Tests.* (Studies in Mathematical Psychology I.) Copenhagen: Danish Institute for Educational Research, 1960. Pp. xiv + 184.

Reviewed by ERIC F. GARDNER

The author, Georg Rasch is a Dane who received his doctorate in mathematics in 1930 and who thereafter went to London to study with R. A. Fisher. Since 1937 he has been a lecturer in biological and mathematical statistics at the University of Copenhagen and, since 1944, he has been teaching statistics to students of psychology. The reviewer, Eric F. Gardner, studied psychological measurement and statistics at Harvard University where he took his doctorate with Truman L. Kelley. He has been deeply involved ever since, but not exclusively so, with matters of psychological measurement. In the area of measurement he wrote the chapter on statistical methods in the 1957 Annual Review of Psychology, was co-author of the 1953 revision of the Stanford Achievement Test and was co-author of the Syracuse Scale of Social Relations. He is now one of the five ex-

aminers who advise the College Entrance Examination Board on policy and research operations connected with the scholastic aptitude test. In areas not so intimately related to measurement he co-authored with George Thompson *Social Relations and Morale in Small Groups* (1956) and, with Thompson & Francis DiVesta, *Educational Psychology* (1959). In addition to a continuing pattern of editing and consulting, he serves as Chairman of the Department of Psychology at Syracuse University.

FOR the past fifty years test theorists have been concerned with a series of problems related to the interpretation of test scores and to the description of item characteristics. These problems have been studied under the rubrics of item analysis, scaling, equating, and norming.



One is inclined to believe that once having decided one way or the other, the company surrounds itself with reasons why its particular practice is correct.

I do not believe that this study adds anything that psychologists should read. It is one of a number of studies done on "what other companies do." I feel that while this type of study may be useful to those in the field who are called on to provide material on practice, it is no substitute for good thinking or writing on what should be done. The mandatory retirement age allows the company to be impersonal in that the poor and inefficient are forced out along with the most effective. From the point of view of the corporation, this is an extremely inefficient practice, since it tends to "throw out the baby with the bath water," as H. M. Johnson loved to say. The flexible approach, which seems to be followed mostly by family governed companies and those with a number of senior citizens already among their executives when policy was decided, offers the opportunity of retaining the most effective. In practice, from the authors' report, this approach seems to have resulted in a number of cases of retaining the dominant but frequently less effective—a case of throwing out the baby and retaining the bath water, if such can be conceived. From the corporate point of view, a solution utilizing physiological, psychological and performance measures and administered impartially by a committee with the corporate interest at heart would seem to offer a marked advantage.

If there is a significant contribution in this book, it is contained in Appendix C, where two careful lists of questions to be asked and answered have been provided: one set is to be used to determine whether an individual should be retained beyond normal retirement age; the other set is to be used to determine whether an individual should be retired earlier than the normal retirement age. I would commend this Appendix to those who are concerned with these problems.

## A Mathematical and Humanistic Look at Conflict

Kenneth E. Boulding

*Conflict and Defense: A General Theory.* New York: Harper, 1962. Pp. v + 349. \$7.00.

Reviewed by RICHARD S. LAZARUS

*The author, Kenneth E. Boulding, is well known, primarily as an economist, and is further identified in the review. The reviewer, Richard S. Lazarus, received his psychological education at CCNY and the University of Pittsburgh, obtaining his doctorate at the latter. Since 1957 he has been at the University of California in Berkeley where he is now a professor of psychology. He has done systematic research on psychological stress and personality and he guesses that he was asked to review this book on the presumption that conflict and defense meant intrapsychic conflict and ego-defense, not inter-individual, inter-group conflict. He is the author (with G. W. Shaffer) of Fundamental Concepts in Clinical Psychology (McGraw-Hill, 1952), and of Adjustment and Personality (McGraw-Hill, 1961) (CP, Nov. 1962, 7, 411). He is currently writing a theory of psychological stress, which he hopes will be an integrative statement about the field, and is editing for Prentice-Hall a series of introductory paperback volumes representing the whole of psychology. He reports that he himself is writing the volume on personality and adjustment for that series; he does not say who will edit that one.*

IT came as a surprise to the reviewer to discover that Boulding employed the term conflict to refer, not to intrapsychic matters, but to inter-individual, inter-group, inter-institutional and international struggles. Similarly surprising is the use of the term defense to refer not to ego-defense, but to national de-

fense. Even so, there was much gratification in the experience of reading this literate piece dealing with matters of great concern to both social scientists and laymen.

Boulding is well-known as the author of a widely used textbook of economics, and as an econometrician. A professor at Michigan, he is also actively involved in the *Journal of Conflict Resolution*, the Center for Research in Conflict Resolution, and is a contributor to the Society for General Systems Research, an organization that encourages the development of general theoretical systems which might be applicable across fields regardless of their specific content.

*Conflict and Defense* reflects two main lines of interest. One is the analysis, by means of mathematical models, of social processes involving conflict. The other is the passionate effort to solve in a practical way the potentially disastrous problems of social conflict besetting our age, the most crucial of which is war and international tension. To these tasks Boulding brings the traditional background of the economist who views the human being as a rational creature attempting to maximize his utility. But Boulding also is a humanist, one who has published a volume of sonnets and who has an abhorrence of war as a solution to international conflict. He believes passionately that the current conditions of international tension produce a desperate plight because no nation is any longer "unconditionally viable" in a world of missiles and atomic weapons. Thus, the values of the rational, mathematically oriented





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economists are tempered by the values of the poet who is aware of limitations in the strictly economic view of man. Boulding writes, "The major contribution of the psychologist to the economist's view of behavior is perhaps to point out that there may be negative goals that repel the individual as well as positive goals that attract him." Further evidence on Boulding's orientation can be found in his earlier book, *The Image* (1956); in that volume he presents a phenomenologically oriented statement on the person's unique interpretation of himself and the world in which he lives.

Boulding's literary interest and skill are seen in this book's highly poetic passages reflecting both sensitivity and articulateness in dealing with human problems. For example, in a stirring passage on the need to attain a world perspective liberated from a narrow egocentric and ethnocentric bias, he also recognizes the difficulty in attaining it by noting, "I must confess, however, that even though I know the brilliance with which my own children shine in the sky of my attention is only an illusion of perspective, my personal concern and activity is heavily weighted in the direction of the near and dear." And a bit later he writes on the same point, "We are still left, however, with

a serious ethical dilemma that has become overwhelmingly acute in the modern world—that of reconciling the universal ethic that both science and high religion imply with the particularistic loyalty to existing institutions and responsibilities." And, commenting bitterly on the mentality of the war psychology that led to the controversial report of the Rand Corporation in its study of non-military defense, he writes, "By making what seem to me fantastically optimistic assumptions, this report concludes that, by taking adequate precautions, the United States might not lose more than from 5 to 85 million people, and the economy would recover almost to previous levels, assuming a gigantic effort, in 25 years. What the report does not say is that the purpose of all this misery and sacrifice is so that the next generation can go through it all again; that is, the purpose of national defense is to re-establish the system that gave rise to the catastrophe."

Thus, *Conflict and Defense*, has these seemingly contradictory qualities of being, in part, a cold effort at mathematical and topological analysis of social conflict, and at the same time a warm, humanistic effort, on the part of a literate citizen who feels deeply the urgency of the problems of conflict, to find solutions to them.

But what of the first effort, the attempt to fashion mathematical models for conflict behavior, portrayed as these are by Boulding through the use of elaborate Lewin-like topological diagrams? This effort seems to the reviewer to fall far short of the brave goals. It involves first the making of a limited number of assumptions about the mechanisms of behavior in conflict situations, then the quantitative derivation of resultant behavior patterns in various inter-personal and inter-group contexts. (There is analysis, for example, of economic conflict between business firms, of hostility conflict between persons, and of political and military conflict between nations.) One trouble is that the assumptions are terribly over-simple. As is true of all mathematical models, actual behavior does not necessarily work so simply. Boulding recognizes that other

assumptions are needed, and during the discussions of various forms of conflict, he introduces, willy-nilly, many interesting postulates about groups and individuals as parties to conflict; but none of his postulates is systematic or capable of verification because the issues of measurement and validation are begged.

It seems to this reviewer that the weakness of mathematical analyses lies not in any essential defects of the mathematical tools, but rather in the fact that essential ingredients are still missing with which to construct the necessary formulas. That is to say, social scientists have not yet provided the necessary rules of the game to make the process of analysis deterministic. Boulding appears to think he has enunciated a theory, but it cannot really be found in his book. What there is are many fascinating, and often profound assumptions at many levels of analysis, scattered unsystematically throughout the book, and brought up in an *ad hoc* fashion when it is useful to do so, or when it suits the author's taste. Throughout, Boulding seems to recognize that the mechanisms of behavior are simply not well enough understood to permit the system to be fully elaborated and deterministic. And after all, it is a matter of taste how long to postpone mathematically oriented model building in the absence of well established behavioral principles on which to build. His psychology is often naive, but he seems at the same time to know it.

In spite of these limitations, which are, after all, limitations of the social sciences rather than of Boulding, the effort is wonderfully worth reading, even if reading it highlights the herculean task still required in theory and measurement in the social sciences before mathematical analysis can attain its ultimate promise. One cannot read the book seriously without developing a sense of admiration for the intelligence and the humanity of the man who is working so hard to provide the conceptual basis for the unification of the social sciences, and tries so mightily to find effective solutions to the problems posed by inevitable and pervasive human conflict.



The common practice has been to administer tests to samples from well defined populations and to study the responses made by these subjects to the stimuli. The proportion (or a function of the proportion) of these subjects who make correct responses to an item has been defined as the difficulty index of the item for that group. The index representing the ability of the subject as measured by the test has been obtained initially either by the summation of his errors or summation of correct responses. Various types of transformations have then been applied to distributions of raw scores to incorporate in them normative properties. In addition, numerous equating studies have been performed to establish the comparability of scores among various test forms. For example the various forms of the Scholastic Aptitude Test of the College Entrance Examination Board have been linked together and anchored to the scale scores established from the 1943 normative population. The precise role of the examinee population in theoretical psychometrics has been challenged many times and is still a controversial topic.

Dr. Rasch attempts to set up and illustrate the use of models which take into account two very old issues associated with the role of the examinee population. They are 1) the extent to which most statistics, being group-centered, are appropriate or adequate for answering questions about individuals and 2) the desirability of being able to make comparisons between individuals independent of the particular instruments used and, symmetrically, the desirability of being able to compare stimuli measuring the same thing independent of which particular individuals were used in the comparison. Stochastic models based upon the probability that a subject responds incorrectly are developed for several types of tests in the application of which the role of the populations are eliminated. Each model implies two types of parameters, a "difficulty" for each test (or item) and an "ability" for each person.

**T**HE simplest model presented is one designed for an oral reading test where

the score is the number of errors made. By means of certain (as the author says) "bold" assumptions the problem may be formulated in terms of a Poisson distribution as follows:

If the probability of a pupil making an error in reading a certain text is  $\theta$  and he reads  $N$  words, the probability that he will make "a" errors is given approximately by

$$p\{a/N\} = \frac{e^{-\lambda} \lambda^a}{a!}$$

where the parameter

$$\lambda = N\theta$$

is the mean number of errors. This relationship follows from the well-known fact that the Poisson distribution will closely approximate the binomial if  $\lambda$  is small.

Dr. Rasch assumes that the probability of making an error on a test is the product of two factors, one  $\xi_n$  pertaining to the pupil, and the other  $\delta_i$  to the test such that

$$\theta_{ni} = \frac{\delta_i}{\xi_n}$$

Hence  $\delta$  is conceived as measuring a degree of difficulty of the test and  $\xi$  as a measure of the ability of the person.

By using two tests with known number of errors on each one Dr. Rasch shows that the conditional probability of the event that a person has a specific number of errors  $a_{n1}$  on the first test, knowing that he has a total number  $a_n$ , on both tests is independent of  $\xi_n$  the personal factor. Hence it is possible to estimate this particular difficulty parameter of the test independently of the particular subjects involved.

In a similar fashion the author sets up a probabilistic model for measuring speed in oral reading and arrives at the same type of distribution function for number of words read in a given time as for the number of errors in a text of a given length.

**A** THIRD major topic is the presentation of a structural model for scaling items of a test. The approach is somewhat similar to Guttman's work on scaling (1941) and to Ebel's "test scores having content-meaning" (1962).

Although there are a number of shortcomings of these models, including the extent to which data in most practical situations would meet the necessary

assumptions, the approach is very interesting and provocative. However in defense of Dr. Rasch some of the assumptions appear to be no more violent than some currently used by psychometricians, e.g. the assumption that the distribution of errors of measurement of a number of individuals estimated from scores based on odd-even responses can be used as an approximation for the distribution of errors of measurement of a particular individual on an infinite number of comparable forms of a test.

Although the empirical data presented conform well to the models, it would be premature to assume that we now have available new techniques to replace the currently used procedures. The success of the model even in the special cases cited seems to me to be due in no small measure to the apparently extremely careful work which must have been done to obtain the comparable forms of the instruments used. As Dr. Rasch says, "Psychologists wishing to use the methods in practice will miss a careful discussion of how to do so. I consider, however, that it is as yet a little early to go right ahead to practice." This volume, presenting an interesting and somewhat different approach, deserves careful study by theoretical psychometricians.

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*There are found some minds given to an extreme admiration of antiquity, others to an extreme love and appetite for novelty; but few so duly tempered that they can hold the mean, neither carping at what has been well laid down by the ancients, nor despising what is well introduced by the moderns.*

—FRANCIS BACON







Because of reasons he hopes are obvious, CP's editor puts a damper on his own communicative impulses and yields here to Dr. Joyce M. Hoffman of the Veterans Administration Hospital in Brockton, Mass., who now has the floor.

#### A NOTE ON CURRENT PAPERBACKS

IN THE last few years paperback books have gone from the western and murder mystery circuit right on to the shelf of the student. Twenty-four European and American universities are now issuing English Language paperback books of scholarly import. Several large publishing houses are issuing paperback series of serious works as well as their hardbound editions. Finally, many publishers have come into existence solely to publish serious paperback books, and they are flourishing.

As opposed to the earlier paperbacks which tended with awful rapidity to become loose-leaf editions of their former selves, the current lot are tougher, better constructed, and more durable. They tend to be more expensive than their antecedents, but still very much less expensive than their hardbound brethren, whose prices soar ever higher and rapidly pass from view.

To the student and the younger professional still recovering from the debts of the graduate school, the paperbacks offer the chance to have books which would otherwise be out of reach monetarily. For the established professional who is loaded with money, the paperbacks offer the chance to add to the library books which have been out of print and unobtainable for years, and are now being issued in paperback editions. To the teacher, the existence of useful paperbacks means that his students can own and read at leisure books

which otherwise would have to be used in the library, if at all.

Though there are several indices of paperback books, none provides a usable list of books in psychology. They tend to list under psychology some books which might better be listed under fiction. Meanwhile, books of import to the psychologist are listed under numerous other headings. A work list of paperbacks relevant to psychology may be useful.

Of the 16,600 paperbacks in print as of September 1962, approximately 330 of them are of use in psychology. They are listed here alphabetically by last name of author or editor. This is followed by the title, price, letter and/or number of the book, if it has one, and finally the publisher. (A separate page appears on the back of the list to identify relevant publications by WHO and UNESCO.)

Listing publishers gets a little complex, inasmuch as one principal publisher may have several subdivisions, some with the same address as the "parent" press, and a few with different ones. For this reason, a list of publishers and their divisions appears at the back of this list.

- ART and BELLAK (Ed). *Projective Psychology*. \$2.95. E169 Evergreen.  
 ADAMS, D. *Anatomy of Personality*. \$.85. 11-PP Random House.  
 ADLER, A. *Individual Psychology*. \$1.75. 209 Littlefield, Adams.  
 ———. *Understanding Human Nature*. \$.50. D52 Premier Books.  
 ———. *What Life Should Mean to You*. \$1.25. 3 Capricorn Books.  
 ———. *The Problem Child*. \$1.35. 228 Capricorn Books.  
 ———. *Essays in Individual Psychology*. \$2.95. E180 Evergreen.  
 AICHORN, A. *Wayward Youth*. \$1.25. M5 Meridian.  
 ALEXANDER and ROSS (Ed). *The Impact of Freudian Psychiatry*. \$1.75. 62-PP Phoenix.  
 ALLPORT, G. *The Nature of Prejudice*. \$1.45. A149 Anchor.  
 ———. *Becoming*. \$1.25. Y20 Yale University Press.  
 ——— and GILLESPIE. *Youths Outlook on the Future*. \$.85. 15-PP Random House.  
 ARKIN and COLTON. *Tables for Statisticians*. \$1.25. Barnes and Noble.

- AUSUBEL, D. *Drug Addiction: Physiological, Psychological and Sociological Aspects*. \$.95. 17-PP Random House.  
 BACHRACH, A. *Psychological Research*. \$1.35. 21-PP Random House.  
 BARBU, Z. *Problems of Historical Psychology*. \$1.95. E310 Evergreen.  
 BARTLETT, M. S. *Stochastic Processes*. \$2.95. Cambridge University Press.  
 BELLAK (Ed). *Contemporary European Psychiatry*. \$3.95. E285 Evergreen.  
 BENEDICT, R. *Patterns of Culture*. \$.50. MD89NAL New American Library.  
 BERNE, E. *Transactional Analysis in Psychotherapy*. \$3.95. E300 Evergreen.  
 BERNSTEIN, A. *On the Nature of Psychotherapy*. \$.85. 12-PP Random House.  
 BETTELHEIM, B. *Paul and Mary: Two Case Histories*. \$1.45. A237 Anchor.  
 ———. *Symbolic Wounds: Puberty Rites and the Envious Male*. \$.95. AS153 Collier Books.  
 BEUKENKAMP, C. *Fortunate Strangers: An Experience in Group Psychotherapy*. \$1.95. E137 Evergreen.  
 BIJOY, S. and BAER. *Child Development, Vol. I, A Systematic and Empirical Theory*. \$1.25. Goldentree Books.  
 BIRNEY and TEEVAN. *Instinct*. \$1.45. 2-1B Insight Books.  
 ———. *Reinforcement*. \$1.45. 3 Insight Books.  
 ———. *Measuring Human Motivation*. \$1.45. 6-1B Insight Books.  
 BISCHOF, L. *Intelligence: Statistical Concepts of Its Nature*. \$.85. 5-PP Random House.  
 BONAPARTE, M. *Female Sexuality*. \$1.95. E321 Evergreen.  
 BRENNER, C. *Elementary Textbook of Psychoanalysis*. \$.95. A102 Anchor.  
 BREUER and FREUD. *Studies in Hysteria*. \$1.45. BP 59 Beacon Press.  
 BRILL, A. *Basic Principles of Psychoanalysis*. \$.60. W641 Washington Square Press.  
 ———. *Lectures on Psychoanalytic Psychiatry*. \$1.25. V11 Vintage Books.  
 BROMBERG, W. *Mind of Man: A History of Psychotherapy and Psychoanalysis*. \$1.95. TB 1003 Harper Torchbooks.  
 BROWN, J. A. C. *Freud and the Post-Freudians*. \$.95. A522 Penguin.  
 BROWN, R., GALANTER, HESS, and MANDLER. *New Directions in Psychology*. \$2.50. HRW Holt, Rinehart and Winston.  
 BRUSSEL, J. *Understanding Psychiatry*. \$1.50. Barnes and Noble.  
 ———. *The Layman's Guide to Psychiatry*. \$1.50. EH220 B & N.  
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 COHEN, E. *Human Behavior in the Concentration Camp*. \$1.65. 100UL Universal Library.  
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 COTTRELL, W. *Energy and Society*. \$2.95. 13209 McGraw-Hill.  
 COVILLE, ROURKE, and COSTELLO. *Abnormal Psychology*. \$1.75. Barnes & Noble.  
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- FLUGEL, J. *Man, Morals, and Society*. \$1.65. C93 Compass Books.
- FORD AND BEACH. *Patterns of Sexual Behavior*. \$75. K128S Ace Books.
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JOYCE M. HOFFMAN,  
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The reader will be confused by the different meanings of "deviation," reflecting the different interests of the symposium participants. The title of the volume counterposes deviation with its usual social psychological complement, thus equating it with independence of social pressure. In most of the papers it is used in this sense. As one gets into the volume, however, two other distinctly different usages appear. In one usage deviation is pathological behavior, where its clinical complement would be "normal" or mentally healthy. The third meaning is that of an atypical response where its complement would be "typical," "average," or "modal." The editors of the volume do not prepare the reader for this "forced fit" of diverse viewpoints. We shall first consider those chapters which are on the theoretical side and then those that are primarily reports of recent research.

The first three chapters, which overlap each other considerably, all attempt to integrate or summarize the literature on situational and personality factors which predispose the individual either to conformity or to independence in the face of social pressures. Blake and Mouton, in their adequate summary, distinguish between conformity, which may be only overt, and conversion, which is an internalized acceptance of the opinion advocated by the group. In the second chapter Bass, under the formidable title of "Conformity, Deviation, and a General Theory of Interpersonal Behavior," presents thirty-eight propositions which have to do with conformity. This agglomeration includes such diverse and complicated statements as: "If the group's difficulties are too great, members may deviate further from the norms of the group rather than increase in conformity." "The self-oriented member conforms to the extent that doing so meets his own personal needs irrespective of whether it is conducive to the task or interaction effectiveness of the group." "The importance of the group, the situation, and the individual are relative matters. We can increase or decrease the significance of each at will." A set of 38 such sentences, which have different foci and are stated at different levels of discourse, can obviously not qualify as theory, at least for this

## What and Who Is a Deviate?

Irwin A Berg and Bernard M. Bass (Eds.)

*Conformity and Deviation.* New York: Harper, 1961. Pp. v + 449. \$6.50.

Reviewed by HAROLD B. GERARD

Both Irwin Berg and Bernard Bass, editors and contributors to the present volume, were located in the Department of Psychology at Louisiana State University, where the present volume went to press. Berg chaired and Bass professed. Since then Dr. Bass has moved to the University of Pittsburgh. Dr. Berg received his PhD at Michigan, longer ago than he likes to recall, and has had an active and varied career as teacher, researcher, author, editor and administrator. Of his various accomplishments he is perhaps proudest of his role as one of the founders of the Confederate Psychological Association. Bernard Bass received his graduate education at Ohio State University, came to Louisiana State in 1949 and except for a year as visiting professor in Berkeley, has been there until his recent move. His publications have dealt principally with the phenomena of leadership, his most recent production being the book, co-edited with Luigi Petrullo,

*Leadership and Interpersonal Behavior* (1961) (CP, Aug. 1962, 7, 303). The reviewer, Dr. Harold Gerard, received his doctorate from the University of Michigan in 1952 and while doing his graduate work served as a research associate in the Research Center for Group Dynamics. After teaching at New York University and the University of Buffalo, he moved in 1956 to the Bell Telephone Laboratories as a member of their technical staff and in September 1962 he went back to academia in the University of California at Riverside. Within the social area he has been especially concerned with problems of social influence.

SOCIAL conformity, a distinguishing feature of society, has long been the focus for considerable research and discussion. The present volume contains papers on the subject delivered at a symposium held in 1960 at Louisiana State University.



reader. Campbell's chapter attempts to reconcile behaviorist and cognitive approaches to conformity and independence, thus dealing with issues of very broad significance. The individual, according to Campbell, enters a social situation with a set of dispositions built up through learning and cognizing. When confronted by a judgmental discrepancy with some social agent, his response results from the composite of prior dispositions and the disposition induced by the social agent. There are no specifications as to how these dispositions interact. Instead Campbell lapses into listing individual and situational factors disposing the individual toward either independence or conformity. For the most part, Campbell's "propositions" describe empirical relationships observed in research on social influence.

Two pioneers, Sherif and Asch, have papers in the symposium. Sherif's paper ranges broadly, discussing past and current work by himself and his students on normative processes within groups and especially those that emerge due to intergroup conflict. Asch's paper gives a very brief historical account of research in conformity suggesting, as he has elsewhere, that the behavior can only be understood by examining the cognitive functioning of the person.

Rokeach, in his paper, discusses conformity within the context of authoritarianism, claiming that the sharp distinction between the reliance upon either reason or authority is false. Rather we should distinguish between the open reliance upon authority for information and closed, blind, slavish dependence. Among the papers reporting new research, the one by Matarazzo and Saslow concerns an investigation of clinical deviation. They examined the tendency for patients in different diagnostic categories to maintain smooth-flowing conversation with an interviewer. This research represents a noteworthy attempt to study psychopathology *in vivo*. They suggest that an individual who exhibits conversational aberrations (i.e., deviation) in one context may behave in a normal fashion in another. Berg, on the other hand, in his paper,

presents evidence for what he calls "the deviation hypothesis," which maintains that a deviate is a deviate regardless of the context. The gauntlet, represented by the obvious conflict of viewpoints between Berg and Matarazzo and Saslow, is never picked up. Here was fuel for interesting controversy which a symposium, where participants meet face to face, could provide.

IN THEIR contribution, Bachrach, Candland, and Gibson describe some inconclusive research on verbal reinforcement, a matter of undemonstrated relevance for conformity or deviation. A second chapter by Blake and Mouton describes a field experiment involving social pressures in which the original focus of the research is not entirely clear.

Pauline Pepinski examines the relationship between conformity and creativity, where the creative individual is seen as a nonconformer (another meaning of deviation?). The experiment, which grew out of her "abstruse" theory based on the analysis of several field studies, is entirely unrelated to the original problem. She finds that if you reward a person for conforming, he will conform, whereas if you reward him for not conforming, he will not conform. What this has to do with creativity eludes this reviewer.

This reviewer also is puzzled over the relevance to this symposium of the paper by David Wechsler in which he is concerned with the injustices committed in the name of being well-born, focussing primarily upon the atrocities of the Nazis and their super-race myth.

Was such a volume necessary? Why should this assortment of papers be dignified by a hard cover binding and a flashy title? No issues are attacked in any concerted way, no controversies are aired, few if any common threads appear, little carefully controlled *new* research is presented which bears on issues which are significant and central to the symposium topic, and there is in some of the papers repetition of positions previously stated. One wonders what was the purpose of the symposium. Some self-control by psychologists as well as some quality control by pub-

lishers would seem to be required. Indeed, one begins to suspect that some symposia (this is not the only one) may be published to enable some of the participants to circumvent the rigorous standards to which our journals aspire.

## Shamefully Naked Experience?

Haim G. Ginott

*Group Psychotherapy with Children: the Theory and Practice of Play-Therapy.* New York: McGraw-Hill, 1961. Pp. xiv + 208. \$5.95.

Reviewed by PAUL E. EISERER

*The author, Haim Ginott, obtained his EdD degree from Teachers College, Columbia University in 1952, and since then has amassed a wealth of experience in clinical work with children. He now is holding down three jobs in New York; he is Adjunct Associate Professor and Supervisor of Child Psychotherapy at New York University, is Associate Clinical Professor at Adelphi College and is Senior Clinical Psychologist at the Institute for Crippled and Disabled. Paul E. Eiserer, the reviewer, received his PhD degree in 1948 at the University of Chicago. After two years at the University of Oregon, he came east to Columbia University, where he is now a professor in the Department of Psychological Foundations and Services. Though primarily a clinical psychologist, recently he has also been so caught up in school psychology that he is now director of the Teachers College training program in that speciality.*

STUDENTS in clinical psychology, or experienced psychologists without background in child therapy, are often at a loss for a guiding orientation in work with children. Familiarity with adult verbal symbols offers little comfort when confronted by the nonverbal communication efforts or the partially



articulated formulations of troubled children in a free play situation. The experienced supervisor frequently observes the intellectually sophisticated student's bewilderment when the words which seemingly have such comforting power in work with adults fall flat in the playroom. To those in search of guidance Ginott offers solace, for his book is directed to their needs. Although "theory" appears in the subtitle and "rationale" is often used in the book, the aim and achievement of the author lie in the area of practicality. Selection of toys, limits, meaning of play activities, and other common practical issues are given serious attention. As a child therapist with wide experience, Ginott is a sure footed guide.

Whatever one's view of the state of certified knowledge about psychotherapy it can be said with confidence that the fields of play and group therapy are less well developed with respect to theory and supporting research. Yet Ginott has the audacity to write about both fields.

Some writers about treatment of children (Klein, A. Freud, and Allen) offer more "theory," less practical guidance; others such as Axline offer the coziness of reductionistic simplicity. In contrast Ginott reveals theoretical pretensions, but they fail to obscure his eminently practical bent. In a chapter on research he comments, "for the most part, this field has been fallow and the yield meager." The author believes the land to be arable.

It is hoped that the term "group psychotherapy" in the title will not limit the sale of the book for the contents, as the author admits, "apply equally well to individual therapy." A more accurate reflection of the author's contribution might have been a book on play therapy with several chapters about group therapy. Although topics common to books on group therapy—rationale, selection criteria, composition—are discussed in early chapters, considerations of group therapy are missing in later chapters.

THE LITERATURE of group therapy during the past decade shows a tendency to use constructs from social psychology, and to deal extensively with

interaction phenomena, but there is little evidence of such interest in this book. It is not clear whether the author considers such trends irrelevant to child behavior or whether he prefers the more traditional psychoanalytic views of Slavson, to whom he acknowledges indebtedness. From the chapter on theoretical framework for group play therapy the reviewer would surmise that the latter is the case.

The novice in play therapy is often at sea amidst the conflicting claims of different theoretical orientations. Shall he be interpretive, nondirective, permissive, or relationship oriented? He yearns for the truth that will set him free—to do therapy. Ginott might well answer that the fault lies not in the theoretical stars, but within. In a perceptive discussion of the qualifications of the child therapist the reader is brought face to face with personal issues that must be resolved as the price of admission to the inner world of children.

As evidence of the author's unwillingness to be bound, even by the title of a book, two appended chapters ("Group Screening: Pre-intake Selection of Motivated Applicants" and "Parent Guidance Groups") are included, and they are stimulating indeed. They offer a jarring reminder of the depth of the rut into which practice in many clinics has often descended. Instead of making the usual lament about waiting lists, Ginott, with energy and inventiveness, set out to do something about the situation in the Child Guidance Clinic in Jacksonville, Florida, where he was Chief Clinical Psychologist. The procedures employed to provide immediate service to applicants, "to eliminate potential non-attenders" and to enhance the selection of clients appropriate to the clinic's resources are described in sufficient detail to invite emulation.

The doctrine held by many clinics, that the price for accepting a child in treatment must be therapy for the parent, usually the mother, is also challenged as an overgeneralization about parental need. Differentiated use of therapy, counseling, and guidance groups is presented as a more fruitful approach for a clinic that serves a heterogeneous population.

Essentially the book may be viewed as the report of an experienced child therapist offering what he has learned from his life with troubled youngsters. He is at his best when writing out of his experience in the playroom and it is good enough. His uses of "theory" are revealing. Often he says "I believe;" not so often "theory requires" or "theory suggests." As is true of so many "theory and practice" books the garment of theory hangs loosely on the body of practice. Shame on naked experience!

## The Heavenly Cities of the Neo-Freudians

Martin Birnbach

*Neo-Freudian Social Philosophy.* Stanford, Calif.: Stanford University Press, 1961. Pp. v + 238. \$6.00.

Reviewed by GUY E. SWANSON

Martin Birnbach, the author, took an MSc in economics from the University of London and a PhD in political science from Rutgers University. His interests have long focused on political theory and psychology. At present he is visiting lecturer in the College of Social Studies at the University of Puerto Rico. Guy Swanson, the reviewer, is a professor of sociology at the University of Michigan and works to keep personality related to social organization in Michigan's doctoral program in social psychology. He is the author of *The Birth of the Gods: The Origin of Primitive Beliefs* (1961) (CP, Jan. 1962, 7, 22f.) Also, with Daniel Miller, he wrote both *Inner Conflict and Defense* (1960) (CP, Mar. 1961, 6, 89f) and *The Changing American Parent* (1958) (CP, Oct., 1959, 4, 318).

SOCIAL and political philosophies offer special contributions to our interpretations of behavior. They afford a whol-



istic view of man in society that can serve to guide the evaluation of more specialized perspectives. They also provide an array of concepts that help us to formulate systematic comparisons. In recent years Philip Rieff, Sebastian de Grazia, and Herbert Marcuse have employed these resources in notable appraisals of orthodox psychoanalysis. Now Martin Birnbach takes the same approach in assessing the neo-Freudians: Sullivan, Kardiner, Alexander, Horney, Fromm, and Lasswell. His principal concern is with their vision of social interaction and the social order, leaving to other commentators their work on the structure of personality.

Birnbach encountered difficulties from the beginning. There seems an important exception to almost any generalization one might hope to establish about these six writers. Lasswell, at least in his more extended treatments of personality, is quite the orthodox Freudian, sharing the master's skepticism concerning human progress, giving relatively little heed to ego processes or man's capacity for psychic growth and change, and refusing to elevate the value of love above that given health or safety or sexuality in the psychic economy. Fromm, like Lasswell but unlike the other four, presents an explicit and detailed picture of the connections between personalities and social relations, but Birnbach comes to doubt that this picture owes much to Freud—hence can hardly be called neo-Freudian. Birnbach concludes that "his system is essentially a sociological, or perhaps a philosophical one, and not a psychological one" (p. 204).

Because the four who remain provide only fragmentary statements concerning the encounters of individuals with each other and with institutions, Birnbach cannot employ most of the analytic tools with which he is equipped. By a considerable extrapolation from their works he does manage to place them "firmly in the humanistic, individualistic, libertarian tradition of Western thought" (p. 218). There is no discussion of Marcuse's judgment that the neo-Freudians have seriously compromised Freud's outlook which Marcuse held to be in that tradition. One may guess, however, that the

data are too slight to permit a conclusive answer.

IF BIRNBACH'S careful survey documents any point it is that Sullivan, Horney, Kardiner, and Alexander give little place in their work to a technical analysis that would relate personality to a social context. What they do, in very general terms, is to stress that human experience is at least the equal in importance to human biology in the formation and development of personality. Each illustrates the point by citing social conditions that induce pathology, promote psychic health, or structure enduring features of personal organization. None develops a thorough and systematic treatment of the issues they jointly have raised.

It is a particular merit of Birnbach's work that social psychologists and social psychiatrists who wish to pursue the tasks marked out by the neo-Freudians now have available a thorough review of their social thought. Birnbach does more than bring together their scattered and shifting ideas. He understands that much of the meaning and coherence of a man's work is to be found in the career from which it grows, and he provides for each writer a sufficient intellectual biography to help us understand the problems each set for himself and the successive steps through which his thought proceeded. This is not an effort to substitute biography for logic in appraising a man's work, but to lead us closer to the objectives and premises, often only implicit, which that work contains and which must become explicit before more formal evaluations can be made. Thus the enormous corpus of Lasswell's writing makes little sense until we see how the grave political crises of the 20's and 30's brought him to search in Freud for the roots of irrationality and the means, not for their elimination, but their amelioration. Without the perception of Fromm seeking the common ground between psychoanalysis and Marxism and then trying to overcome the inadequacies of both, his work can appear to be only a series of interesting but diverse speculations about personality and social history. Until we understand that Sullivan, for all his stress on ego processes, saw

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little promise in rational decision-making through debate and argument, his view of the pathogenic character of our society seems almost hysterical.

Some readers will not share Birnbaum's judgment that the neo-Freudians, Fromm often excepted, founded their

judgments on adequate evidence. Others will be surprised to find a political scientist arguing that social institutions are functions of psychic needs. Whatever one's tastes in such matters, the book as a whole is a work of substantial and thorough scholarship.

## Clients (and Counselors) with Problems

James F. Adams

*Problems in Counseling: A Case Study Approach.* New York: Macmillan, 1962. Pp. 164. \$2.25.

Reviewed by EARL A. KOILE

James F. Adams, the author, took an EdM in counseling and guidance in 1951 from Temple University and a PhD in psychology in 1959 at Washington State University. He is now Assistant Professor at Temple. He has published widely in educational, psychological, and personnel journals. The reviewer, Earl A. Koile, took two graduate degrees in the School of Education at Harvard University where, he reports, he learned about the intellectual side of counseling, and then, in the States of Vermont and Texas, he learned about the emotional aspects of the same thing. Since 1958 he has been at the University of Texas, where, while teaching in the Department of Educational Psychology and serving as coordinator of counseling at the University's Testing and Counseling Center, he deals with students in the counseling process and also with counselors who deal with students in the counseling process. His continuing interest in personality development, as such development is facilitated both by the therapeutic climate and by other contexts for learning, can be seen in his publications and in his recent co-editorship of *Personality Factors on the College Campus* (1962).

FOR students and professors who want case studies and counselor incidents to discuss and to carp about, this small book fills the bill in a big way—particularly if the accent is on carping. There are no typescripts of interviews by Carl Rogers or Theodore Reik here. Instead there are cases that reveal the foibles and frustrations of the novice who has assumed the mantle of the counselor but who has not yet acquired the requisite sensitivity and skill. There are other cases that suggest the counselor's desire but also his inability to offer a helping relationship. And there are skillfully reported cases that suggest effective counseling. Portrayal of both good and bad counseling apparently is the author's aim; in this he is successful.

Twenty-five counseling cases represent a variety of problem situations. The array includes problems of vocational choice, job and academic adjustment, premarital pregnancy, family, sexual, and other emotional conflicts. Some of the cases are complex and difficult; most seem run of the mill in that they are precisely the kinds of cases counselors face frequently in their work-a-day world.

Clients in the 25 cases range in age from seven to forty-six years. Two are married men; three are divorced women; others are children and youth of both sexes. Two-thirds of the clients are in the junior-senior high school setting; some are in elementary school, college, and state school; others are in the community guidance center, vocational guidance-placement service, and private practice.

Counselors range in training from the college psychologist and the psychotherapist in private practice to the teacher-counselor without formal training. Except for two typescripts of interviews, the counselor presenting a case describes the problem and background, comments on what he does, and poses questions about the case and his counseling behavior.

The author's expectation is that discussion and critiques of the cases will add to the intellectual and emotional experiences of the prospective counselor. Among the incidents likely to provoke lively discussion are these:

—A counselor gets a thirteen-year-old boy to the state hospital for a neuro-psychiatric evaluation by telling him that he is to participate in a children's program there.

—A seventeen-year-old girl writes a 'love note' to her young unmarried counselor who tells her that they can be 'only friends.' He later suggests that such relationship problems could be avoided if the school only had a woman counselor.

—A sixteen-year-old girl confides to her teacher-counselor that her father is forcing her to have sex relations, whereupon the teacher informs the principal and the local chief of police.

—A counselor insists that he must not counsel a client about problems related to religious beliefs because his beliefs are quite different from those of the client.

This volume vividly illustrates the need for referrals and for cooperation among the different treatment agencies. It communicates very little about the actual process of counseling or about the behavior of a perceptive and able counselor. In only one instance (Chap-



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ter 21) is a case reported in sufficient detail and depth for discernment of what the counselor is doing to facilitate client self-exploration, discovery, and change.

The author identifies himself with a broadly conceived client-centered orientation to counseling. With two or three exceptions, there is no evidence in the cases to suggest any theoretical or experiential rationale for the counseling process or for client behavior change, other than what is expected through advice giving, the offering of a friendly ear, and a hope for the best.

This reviewer found himself wishing that more cases by highly skilled and sensitive counselors had been included and that counselor behavior had been related explicitly to different views of the counseling process and to assumptions about how client behavior changes. Models of interviewing and case reporting by the expert may overwhelm the student and heighten his sense of inadequacy, but they also should provide him with a basis for a deeper understanding of counseling and should help him find criteria for judging his own professional growth. Moreover, relating counselor behavior to theoretical rationales for counseling and for behavior change should help the trainee counselor to understand what he is doing and what the effects may be, thereby enabling him to gain new knowledge and to formulate new hypotheses about the counseling process.

Despite its dearth of good case reports and counseling, this publication can be a useful resource for the sophisticated counselor who teaches a course in counseling. The issues that the instructor and perceptive student will want to discuss may not be those posed by counselors who present cases, but issues by the dozen will be conspicuous.



No scientist can observe directly nor experiment upon every event that belongs to his field of investigation. But in no case should the objects of such remote knowledge be regarded as different from those directly observed.

—KANTOR



## Gimu in the Work Place

Arthur M. Whitehill, Jr. and Shin-ichi Takezawa

*Cultural Values in Management-Worker Relations. Japan: "Gimu" in Transition.* (Research paper 5.) Chapel Hill: University of North Carolina, School of Business Administration, 1961. Pp. xiv + 113. \$2.00.

Reviewed by ARNOLD S. TANNENBAUM

The first of the two authors, Arthur Whitehill, Jr., is Reynolds Professor of Human Relations in Industry at the University of North Carolina School of Business Administration. In the past he served as a Fulbright Lecturer at Keio University in Japan and as a research scholar in Japan under the auspices of American Philosophical Society. The second author, Shin-ichi Takezawa, received an LLB degree from Tokyo Imperial University and followed it up with graduate work at the Universities of Minnesota and Chicago. Presently he is Professor at the School of Social Relations, Rikkyo (St. Paul's) University, Industrial Relations Center, Tokyo. Arnold Tannenbaum, the reviewer, started out to be an electrical engineer but after receiving a Purdue degree in that field moved to Syracuse University where his interest ran to social psychology and where he received his doctorate. His most recent specialization concerns organizational behavior and he is now directing a series of studies concerning the conceptualization, measurement and the practical applications of processes of control in organizations. He is executing these studies in the Institute for Social Research in the University of Michigan. Combined with his research activities is a lectureship in psychology at the University of Michigan. With R. L. Kahn he co-authored the *Participation in Union Locals* (1958).

THE LITERATURE of anthropology illustrates important differences in social structure and custom among peoples in different lands. Increasingly, psychologists have come to apply their

own tools of measurement to the study of these differences. Intelligence tests, Rorschachs and TATs, F scales and comparable attitudinal measures have been administered in a number of countries. Laboratory experiments and field studies have also been conducted cross-culturally to investigate a broad range of phenomena of interest to the psychologist: processes of communication and decision-making, problems of juvenile delinquency, voting behavior, and of adjustment, morale, and motivation of workers in industrial organizations.

These studies have not met with equal success, but they represent the best of scientific motivations—curiosity, the desire to explore, to discover and to broaden the base of scientific generalization. Cross-cultural research provides a special challenge to the student of human behavior who is endeavoring to learn more about the conditions under which his hypotheses may or may not hold and who is hoping to develop principles that transcend the limits of national boundaries.

The present monograph is concerned with the impact of cultural values on the motivation of workers. It reports the results of a study conducted among 283 workers in five Tokyo companies, and 60 members of the Japan Management School. It is the first in a series of studies which will include comparable research in the United States and other countries.

The authors initiated this work under the premise that "good-management" and "good-worker" roles vary from one culture to another because of different, culturally acquired expectations. Of particular importance in the motiva-



### EXPLORATIONS IN COGNITIVE DISSONANCE

By JACK W. BREHM, *Duke University*, and ARTHUR R. COHEN, *New York University*. Describes and evaluates *dissonance theory*—which deals with the conditions under which people react to unpleasant situations. Includes extensions of theory into new areas of psychology; compares it to other theoretical positions; indicates some applications of the theory to social problems. Another important feature of the book is its presentation of the authors' experimental studies. 1962. 334 pages. \$7.95.

### BIRTH TO MATURITY

#### A Study in Psychological Development

By JEROME KAGAN, *Fels Research Institute*, and HOWARD A. MOSS, *National Institutes of Health*. This book is based on the Fels Research Institute's longitudinal study of the personal and intellectual growth of 71 individuals. Each subject was studied from birth to 14 years. As an adult each was given intensive tests and interviews. The book shows that the relationship between an adult's personality, occupational choice, and interests is a function of his childhood characteristics. 1962. 381 pages. \$8.50.

### CONCEPT LEARNING

#### An Information Processing Problem

By EARL B. HUNT, *University of California, Los Angeles*. Drawing upon findings in three different scientific fields—psychology, logic, and cybernetics—this book examines the problem of how intelligences, human or artificial, learn to assign objects, situations or persons, to pre-existing classes. 1962. 286 pages. \$7.50.

### GROWING UP IN RIVER CITY

By ROBERT J. HAVIGHURST, PAUL H. BOWMAN, GORDON P. LIDDLE, CHARLES V. MATTHEWS, and JAMES V. PIERCE, *all of the University of Chicago*. A longitudinal study of an entire public school age group from ages 11-20. Compares those who are successful against those who fail. A non-sentimental and non-prejudicial treatment of social class and social adjustment as it relates to the process of growing up. 1962. 189 pages. \$4.50.

### PERSONALITY DYNAMICS AND DEVELOPMENT

By IRVING SARNOFF, *New York University*. This book offers a systematic presentation and evaluation of the psychoanalytic theory of personality, relating psychoanalytic concepts to those derived from developmental psychology, sociology, and anthropology. 1962. 552 pages. \$7.75.

### INTERGROUP RELATIONS AND LEADERSHIP

#### Approaches and Research in Industrial, Ethnic, Cultural, and Political Areas.

Edited by MUZAHER SHERIF, *The University of Oklahoma*. A detailed study of important problems of intergroup relations. Special attention is given to the role of leadership as it contributes to, and is itself affected by these relations. 1962. 284 pages. \$5.95.

### FRUSTRATION AND CONFLICT

By AUBREY J. YATES, *University of Western Australia*. Surveys experimental work in the field, covering the relationship between frustration and fixation, aggressions, regression and conflict, learning theory and behavior. 1962. 246 pages. \$5.00.

### SOCIETY, CULTURE, AND DRINKING PATTERNS

Edited by DAVID J. PITTMAN, *Washington University*, and CHARLES R. SNYDER, *Southern Illinois University*. A wide selection of the best current social science research on drinking patterns, normal and pathological. 1962. 616 pages. \$9.75.

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tions of Japanese production workers is the notion of *gimu*, or feelings of shared obligations. The average Japanese worker traditionally feels a deep commitment to his company which goes beyond the simple matter of earning a livelihood. Management, in turn, feels a primary responsibility for retaining its employees even under circumstances which might not be economically justified. However, the authors see a transition occurring within Japanese culture in the direction of "Western" values, and this provides a central theme around which their research is organized.

The authors accordingly developed 40 questionnaire items designed to ascertain "the degree to which workers accepted and were motivated by unique and traditional cultural patterns." Each of the items was provided with four landmark statements representing scale points from an "Extreme Eastern" (EE) to an "Extreme Western" (EW) response. One item in particular serves well to illustrate the authors' measures of feelings of duty and obligation which they refer to as *gimu*:

I believe workers are willing to work hard on their jobs because:

(EE) They want to live up to the expectations of their parents and family

They feel that they should do whatever work is assigned to them

The harder they work, the more successful they expect their career to be in the company

(EW) The harder they work, the more money they expect to earn.

A MAJOR part of the report presents and discusses worker responses to each of the 40 items in this "Cultural Continuum Checklist." Some differences among sub-groups of respondents are indicated. For example, older workers tend with some exceptions, to be more "Eastern" in their responses than younger. Those with more formal schooling tend to be more "Western" than those with fewer years of school-

ing. Likewise, workers with rural backgrounds are more "Eastern" in their orientation than native urbanites. It is interesting to see that these relationships are very much like those found in this country between the above demographic characteristics and measures of conservatism.

The majority of workers chose moderate responses to the items in the "Cultural Continuum Checklist," suggesting to the authors that contemporary Japanese culture is a mixture of both "the old and the new." It would be nice, of course, to have longitudinal data to document with more clarity what is old and what is new in the worker responses but these are unavailable. The authors may one day provide data of this kind since they intend to continue this line of research—and Japan is still very much in a state of transition.

The authors make a great deal of age differences which they interpret as strong support for their contention that Japanese culture is moving away from traditional values of *gimu*. However, the tendency for older persons to be more traditionalist or conservative than younger (in the United States as well as in Japan) does not imply necessarily that society is moving away from traditional values. The authors are probably right in their contention about social change, but perhaps for the wrong reasons.

The definition of an Oriental-Occidental cultural continuum is an exceedingly difficult task. The reader who insists upon understanding just what this elusive continuum is, conceptually, will have some difficulty. Readers may find several of the items in the scale a little hard to reconcile with their own estimates of Japanese-American value differences. This is perhaps to be expected in any scale which is formed on the basis of expert judgments where some disagreement, even among experts, is likely to occur. But one may wonder why so large a proportion of the 40 items yield bimodal distributions of response. Do these distributions represent a polarization among respondents along a "cultural continuum" or do they reflect some disjunctures in the researchers' choice of landmark statements?

Problems of this kind lead the reviewer to agree with the authors that their checklist needs further work and refinement.

Nevertheless, the authors do provide through their checklist an explicit, operational definition of their "cultural continuum." Furthermore, their instrument is amenable to refinement, standardization and replication. This is a step forward in an area where research has not graduated very much beyond speculation and impressionism.

## For Citizens in Groups

George M. Beal, Joe M. Bohlen and J. Neil Raudabaugh

*Leadership and Dynamic Group Action.* Ames, Iowa: Iowa State University Press, 1962. Pp. 365. \$6.00.

Reviewed by GORDON L. LIPPITT

Two of the three authors, George M. Beal and Joe M. Bohlen, are professors of sociology and economics at Iowa State University. The third, J. Neil Raudabaugh, is Chief of the Program Research Branch of the Division of Extension Research and Training of the US Department of Agriculture. The reviewer, Gordon L. Lippitt, has been productively active in the field of group dynamics since its infancy but not quite his. Name any academic or governmental operation involving the care, feeding and understanding of groups and the chances are good that he is or has been there. At present he is Professor of Behavioral Science at George Washington University and Director there of the Center for the Behavioral Sciences.

THE RESEARCH and interest in group dynamics in the past twenty years has encouraged numerous publications on "how to understand groups." This book is an attempt to make effective group action understandable to citizen leaders. In the pursuit of its purpose it tries a) to give a basic understanding



## **FAMILY ENVIRONMENT AND DELINQUENCY**

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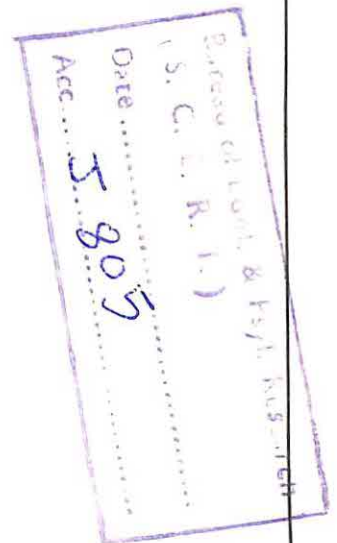
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of group behavior and b) to present an overview of techniques for achieving group goals. Such a two-headed attempt is difficult but worthy; one frequently finds that such a dualistic goal may limit the authors' attention to one with an accompanying exclusion of the other. Fortunately such is not the case in the present volume.

In Part I of the book, the authors present a simplified summary of the research on group dynamics. It is clear, concise and generalized. There are only limited references to the research upon which the generalizations are based, but this was evidently the authors' intention. This reviewer felt, however, that some appropriate references would have helped identify some of the research which made possible the generalizations.

The format of the first section is readable, with an easy style and an excellent size of print. It was bothersome to this reader, however, to have cartoons of sometimes dubious relevance interspersed in the text of that particular section.

The first three chapters gave a rationale for the concern for groups in a democracy. These chapters set the tone for what is to come. This reviewer felt that the section on the "Individual in a Group Setting" was one of the areas in which new material was presented. The various sections on group characteristics were well done, but the main contribution is its simplified style rather than new knowledge.

In Part II of the book the authors present a list of some techniques for group action, a list that can be extremely helpful for adult educators and citizen leaders. Each technique is discussed in terms of characteristics, possible effective uses and limitations. Such an analysis of each method is helpful, and there is particular value in the author's listing of "cautions."

The final section of the book deals with tools for group evaluation. Included in the book are actual evaluation forms. References to the sources of the forms would have been helpful to the reader.

The chapter on "Use of Evaluation" is brief and weak and does not do justice to this important area.

From an overall point of view, the

book makes a contribution to the field by its easily read style, summary of useful methods, and a fine collection of evaluation forms. Except for a brief early statement of leadership, there is little on this subject in the book. In several portions of the book allusions are made to group leadership, but a more basic frame of reference explaining situational flexibility for leadership might have helped clarify leadership for

the reader.

Each chapter ends with questions for stimulating the reader's thought or for use as a discussion guide in a group. The questions are appropriate and helpful to the reader.

The authors attempt to accomplish too much in one book, but the effort is usable, easily read, and one more addition to the rapidly growing literature on group behavior.

## The New World of Superconsciousness

Alan W. Watts. Forewords by Timothy Leary and Richard Alpert

*The Joyous Cosmology: Adventures in the Chemistry of Consciousness.* New York: Pantheon Books, 1962. Pp. xix + 94. \$5.00.

Reviewed by DONALD R. GORHAM

*Alan Watts is at present guest lecturer at the Medical School of The University of California in Berkeley. He came to the United States from England in 1938 and in the course of his career has been editor, minister, college professor and author. Among his several other books are The Way of Zen, Nature, Man and Woman, The Wisdom of Insecurity, and Psychotherapy East and West, the latter published in 1961. The reviewer, Donald R. Gorham, is Chief of the Experimental Studies Unit of the Veterans Administration Central Neuropsychiatric Laboratory at Perry Point, Maryland. He completed his undergraduate work at Colgate University and received his doctorate from the University of Pennsylvania. After teaching for twenty years at the Eastern Baptist Seminary and Keuka College, he entered the clinical field with the Veterans Administration at Waco, Texas, in 1950. For ten years, he was lecturer in the graduate faculty at Baylor University. For the past three years he has taught in the University College of the University of Maryland. He is author of the Proverbs Test and co-author with J. E. Overall of the Brief Psychiatric Rating Scale.*

EVERY psychologist who has dared to contemplate the nature and meaning of fantasy should read *The Joyous Cosmology*. This description of the experiences of a mystic-philosopher-psychologist is so realistic that the reader, willingly or not, enters into this new world. His reaction will probably be at one or the other of two extremes; deep vicarious enjoyment and participation or stout denial, skepticism and withdrawal.

In the Foreword, Timothy Leary and Richard Alpert of the Harvard University Center for Research in Personality, refer to the book as "a great human document" which pushes beyond the work of Aldous Huxley and gives us "perhaps the best statement on the subject of space-age mysticism."

Alan Watts describes with startling clarity and poetic beauty his drug induced experiences in this other world. Time in this world is different: a present which is so unhurried that it reaches back into the distant past and forward into the implications of the future. The senses are wonderfully enhanced and interrelated. Color and form are one. The seen object and the seeing person—eye, nerves, brain—are all a part of



a process by which the external world is brought into the self and made one with it. Every object in this world seems to be alive:

"Everything gestures. Tables are tabling, pots are potting, walls are walling, fixtures are fixturing—a world of events instead of things."

Sound takes on form and texture. The emotional qualities of sensations become very acute and an integral part of their impact. The sensory world seems to take the form of patterns of gesticulations. Nature speaks to one directly:

"The bud has opened and the fresh leaves fan out and curve back with a gesture which is unmistakably communicative but does not say anything except, 'Thus!'"

These understandings that are communicated to Watts are a function of his background, scholarship, interests and aspirations. He decries the duality, particularly of Western culture, which "growing out of semantic confusion and psychological prejudice" separates the mind from the body. He believes that dualism, the greatest of all superstitions, has changed man from a self-controlling into a self-frustrating organism.

A CERTAIN few Eastern *gurus*, or masters of wisdom, and Western psychotherapists have found ways of tricking or coaxing the organism into integrating itself. Watts believes that three drugs—mescaline, lysergic acid diethylamide and psilocybin—can give persons temporarily this feeling of being integrated. The prologue of the book presents something of his philosophical and theoretical point of view, together with a description of the drugs and their use. *The Joyous Cosmology* consists of his account of many drug-induced experiences condensed into the story of one day for the sake of poetic unity.

According to Watts, many of the problems of "knowing" vanish in the drug-induced state. The "inseparable polarity of opposites or the mutuality and reciprocity of all the possible contents of consciousness become vividly apparent in this new world." This new perception is apparently related to the

changed time sense, since in this other world the mind is not bound to move either sensuously or conceptually in serial fashion from one aspect to another. Objects, ideas or feelings are experienced as a whole.

In his contemplations, Watts achieves a self-understanding which amounts to that "'original identity' which ordinary language and our conventional definitions of man so completely conceal." His mind and body are at one with each other. The new self feels a mystical kinship with those about him; indeed with all generations of mankind. But beyond this he feels as one with the original nuclei of the world and a part of the whole creative process. The whole experience produces "a peace so deep that it sings to be shared with all the world." Here is perhaps the best attempt of Watts to portray in words the thinking-feeling state of this other world:

"In the type of experience I am describing, it seems that the superconscious method of thinking becomes conscious. We see the world as the whole body sees it, and for this very reason there is the greatest difficulty in attempting to translate this mode of vision into a form of language that is based on contrast and classification. To the extent, then, that man has become a being centered in consciousness, he has become centered in clash, conflict, and discord. He ignores, as beneath notice, the astounding perfection of his organism as a whole, and this is why, in most people, there is such a deplorable disparity between the intelligent and marvelous order of their bodies and the trivial preoccupations of their consciousness. But in this other world the situation is reversed. Ordinary people look like gods because the values of the organism are uppermost, and the concerns of consciousness fall back into the subordinate position which they should properly hold. Love, unity, harmony, and relationship therefore take precedence over war and division."

In contrast to these contemplative fantasies is the "closed eyes fantasy" as experienced by Watts. These experiences, in the reviewer's judgment, are closely related to the pure drug effect

and would be more typical of the phenomena reported by subjects of LSD experiments. They also suggest a kinship to the hallucinatory behavior of psychotics and probably are the foundation of the experimental psychosis effect thought to be achieved by the use of psychoto-mimetic drugs. Watts describes this state as:

"For the most part ever more complex variations upon a theme—ferns sprouting ferns sprouting ferns in multidimensional spaces, vast kaleidoscopic domes of stained glass or Mosaic, or patterns like the models of highly intricate molecules—systems of colored balls, each one of which turns out to be a multitude of smaller balls, forever and ever."

To round out the poetry of the presentation, the author has included 21 photographs of exquisite beauty. They are so well placed in the text that often they seem literal illustrations of the objects or abstractions being discussed.

In his prologue, Watts gives some suggestions, some warnings, and expresses his hopes as a philosopher. Had he not done so, the reviewer would have emphasized that the psychoto-mimetic drugs cannot be considered a panacea for personal and social ills. In the author's own words:

"I do not mean to generalize. I am speaking only of what I have experienced for myself, and I wish to repeat that drugs of this kind are in no sense bottled and predigested wisdom. I feel that had I no skill as a writer or philosopher, drugs which dissolve some of the barriers between ordinary, pedestrian consciousness and the multidimensional superconsciousness of the organism would bring little but delightful, or sometimes terrifying confusion."

For his part the author is hoping for the development of a brotherly love with its attendant social implications. He bases this hope not just on drug induced insights, but on:

"all those trends in philosophy and psychology, religion and science, from which we are beginning to evolve a new image of man, not as a spirit imprisoned in incompatible flesh, but as an organism inseparable from his social and natural environment."



# The Black Box Revisited

Gerald S. Blum

*A Model of the Mind.* New York: Wiley, 1961. Pp. v + 229. \$6.95.

Reviewed by HARRY I. KALISH

*The author, Gerald S. Blum, studied at Rutgers and Clark Universities and received his PhD from Stanford. From Stanford he moved to the University of Michigan where he is now a professor of psychology. Among a variety of other achievements, he developed the Blum-Blacky projective test for children and authored Psychoanalytic Theories of Personality (1953). The reviewer, Harry I. Kalish, studied at the State University of Iowa, receiving his PhD in 1952. He has taught at Duke University, the University of Missouri, Adelphi College and at the State University of New York, Long Island Center, where he is now Professor and Chairman of the Department of Psychology. He is an ABEPP Diplomate and a consultant to the VA training program in clinical psychology. He maintains an interest in the relationship of learning theory to psychopathology and psychotherapy; in his publications the concepts of anxiety and stimulus generalization occur prominently.*

PROFESSOR Blum's new book represents a marked departure from his previous focus on the explication of psychoanalytic concepts and on the development of projective tests for children. The present work is an attempt to devise a "miniature behavioral system" that transcends the narrowness of contemporary learning theory and the metaphorical quality of psychoanalytic formulations. The author's dissatisfaction with learning theories is prompted largely by their unwillingness to supply an account of the processes intervening between S and R (this will come as a great surprise to Koch (1959) and to the Skinnerians who are already troubled by the proliferation of models and

mediating variables in current psychological thought). The shift away from psychoanalytic theory indicates an important change in Blum's thinking and represents one of many recent attempts to deal with the uncritical acceptance of many psychoanalytic principles. Like Kardiner (1960), Blum has become disenchanted with the usefulness of the energetic principles in psychoanalytic theory and is prepared to abandon them for concepts having a greater possibility of empirical demonstration. One of the purposes of the book, therefore, is to effect a rapprochement between selected concepts in psychoanalytic theory and the new conceptual model.

The radical nature of Prof. Blum's work is, of course, demonstrated by his use of the word "mind" in the title; although it is difficult to see why the author felt it necessary to exhumate the word, since its use contributes nothing beyond an avowal of faith concerning the locus of his conceptual system. In constructing his model, Blum based both the formal language and its syntax (these are nowhere explicitly differentiated) on concepts obtained from psychology (inhibition, anxiety, etc.), neurophysiology (facilitation, reverberation, etc.) electronic circuitry (feedback, induction, etc.) and, to some extent, psychoanalytic theory. The experiments in hypnotism are ostensibly designed to coordinate the formal system with its empirical counterparts.

One of the chief difficulties in reading and understanding the book is in its organization. In the opening chapters the author attempts an overview which contributes very little toward an understanding of the relationship of the formal model to the experiments discussed in the subsequent chapters. (A glossary of terms, indispensable for this type of

work, is not included in the book.) While the experiments in themselves are exceptionally instructive examples of the manner in which hypnotism can be used to supplement much traditional laboratory work with human subjects, their role in the model is often obscure. It is difficult, for example, to decide when the experiments are derivatives of the model or when they serve as operational procedures designed to give the various formal terms their proper empirical substance. In some instances it appears as if the model were an afterthought. Moreover, the chapter-ending summaries, designed to clarify the relationship of the experiment to the model do not accomplish their purpose. In the summary at the end of the chapter on facilitation, the author indicates that this experiment "tested predictions stemming directly from the model" while the evidence suggests that the experiment served rather to define the facilitation construct.

Early in the book, the reader is cautioned not to expect a review of the literature and the author continues to make good his promise throughout. This is rather unfortunate because several chapters could have profited from a proper presentation of the supporting literature. In those instances where the results of an experiment do not appear to be consonant with the prevailing literature, the presentation of the opposing point of view is vague and is not supported by references. For example, in Chapter 5, which deals with a series of experiments concerning perceptual inhibition, the author maintains that "... interference theory does not make explicit use of the role of anxiety and suffers by contrast." Since no reference to any particular interference theory is cited, the statement appears to be somewhat gratuitous. If a reference to Spence and Farber's work was intended, the statement is a direct misrepresentation of their theoretical position in which anxiety is assigned a central role as a drive in the determination of the ultimate strength of competing responses.

In considering the relationship to his behavioral system of existing concepts in learning and perception (Chapter 10), the author defines generalization



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A text for graduate students preparing to become school counselors, and a manual for practicing counselors. Its purpose is to define and explain the role of testing in counseling in order that the counselor can assess more effectively the personalities of his pupils. The counseling use of tests is emphasized. The organization of testing programs, the administration of tests, and the implications for testing of occupational information, counseling interviews, counseling relationships, and counseling research are discussed in detail.

## ☐ CONFLICT AND CREATIVITY

Edited by **SEYMOUR H. FARBER** and **ROGER H. L. WILSON**, both of the *University of California School of Medicine*. McGraw-Hill Paperback Series. Available in January, 1963.

This unique symposium on the community's demands for both creativity and conformity presents essays by twenty-eight well-known authorities in the arts, medicine and the social sciences. Each discipline encourages a different approach to the problems of conformity and diversity and methods for the resolution of differences. This interdisciplinary discussion of the forces acting on the mind clarifies existing thinking and raises new questions which can become the subject of future research. The challenge of discussion is maintained in these essays drawn from the proceedings of a second conference on *Man and Civilization: CONTROL OF THE MIND*, held at San Francisco in 1962. The first OF THE MIND (McGraw-Hill Paperbacks, \$2.95).

## ☐ MOTIVATION: as Related to Personality

By **DOROTHY RETHLINGSHAFER**, *University of Florida*. McGraw-Hill Series in Psychology. Available January, 1963.

Emphasizing changes in human activity, this book uses the experimental approach. It considers a biological approach to motivation, but stresses studies using human subjects. The book is organized around such concepts as arousal, personal pace, and drive. In addition, consideration is given to choice as motivationally determined. Illustrations are drawn from both industry and clinical observations.

## ☐ Psychoevaluation: ADAPTATION - Distribution - adjustment

By **MILTON E. HAHN**, *University of California, Los Angeles*. Available in February, 1963.

Book is founded on the knowledge and skills which have been the basis of counseling and psychotherapy: assessment, diagnosis, evaluation and communication. Discussion is limited to the normal individual within the age range of thirty to senility, and covers the personality growth and development of these individuals through their most active working years, and through the process of planning the last thirty or forty years of their lives. The most important feature is the emphasis on communication by several means in order that the individual better understand himself and his environment. Book is intended for professional psychologists and graduate students in psychology.

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as "...signal spread among connected circuits and networks." In addition to his disinterring of the old Pavlovian notion of cortical irradiation, Blum has dismissed the accumulation of that work in the area of generalization which tends to emphasize its epiphenomenal character and its relationship to discrimination. Unfortunately, the entire effort seems to be characterized by this cursory quality.

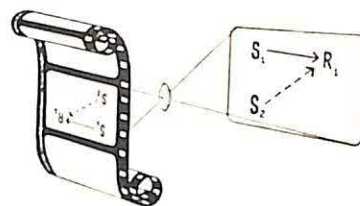
In its present form it is difficult to judge the utility of the conceptual system. Blum has attempted to maintain a parsimonious and consistent internal structure and, in certain instances, appears to have generated testable deductions. But since the success of a model in psychology is directly proportional to the amount of effort psychologists are willing to expend in its behalf, the future of Blum's conceptual system will depend partly on the author's tenacity and, more importantly, on its general acceptance. This particular model offers most psychologists very little more in terms of comprehensiveness than do most models in psychology and a good deal less in terms of precise functional relationships. It is highly doubtful that clinicians, for whom the conceptual system appears to have been created, will hasten to use it for interpretative purposes despite Blum's attempts to retain the more familiar language of psychoanalytic theory. For most clinicians the term "psychodynamic" continues to be intimately related to some variant of psychoanalytic theory of personality development while Blum's system attempts to supply the ahistorical principles governing behavior at any given moment.

The model offers very little that is new in the way of therapy. The alleviation of a troublesome synesthesia in one of the experimental subjects through the use of hypnosis is quite dramatic, but it is not a unique outgrowth of the conceptual system since such techniques have been used by others to obtain similar results.

In *Model of the Mind* Blum has succumbed to the *horror vacui* which will continue to plague most psychologists; it is a malady which can only temporarily be relieved by small doses of concept construction.

# INSTRUCTIONAL MEDIA

Edited by A. A. Lumsdaine



## Programing Unprogramed

Edward J. Green

*The Learning Process and Programmed Instruction.* New York: Holt, Rinehart, and Winston, 1962. Pp. xi + 228. \$4.00

Reviewed by SUSAN MEYER MARKLE

Green, the author, is an associate professor of psychology in the Department of Psychology and Psychiatry at Dartmouth College, where he is director of a project that is developing programs in the medical sciences. He got a BA from Indiana in 1949, and was one of those who went on from Indiana to Harvard (where he got his MA and PhD) when Skinner moved there in 1948. Dr. Green has published several articles on operant conditioning of human subjects. This is his first book. The reviewer, Dr. Markle, is a programmer who has striven to teach the background and principles of programmed instruction to classroom teachers and others in Chicago, Tampa, Newton (Mass.), Denver, and elsewhere, and has recently published a junior high school vocabulary program (entitled, "Words"). Unlike the author, she has written about programing in programed form (see Della-Piana's review of *A Programed Primer on Programing*, CP, Feb. 1962, 7, 64-67). She also previously reviewed for CP one of the first published programs (English 2600; CP, Apr. 1961, 6, 133-136). Following four years with Skinner's teaching-machine project at Harvard and two years with the Center for Programed Instruction in New York, she has recently joined the research and teaching staff of the School of Edu-

cation at the University of California, Los Angeles.

NOT many moons ago a flood of programs on programed instruction inundated the reader in the field. Now programers are abandoning their art and turning to the book. Edward J. Green has been active in operant conditioning since his Harvard days and is now director of a large-scale programing project at Dartmouth Medical School. He has undergone the tribulations of testing the written word for its effectiveness as a teaching tool. From a programmer whose communication skills have been shaped in 'trial by student,' we expect a new kind of book.

Green states that he has "tried to bridge the information gap" between the experimental psychologist who avoids the practical problems of education and the classroom teacher who finds little of practical use in the twistings of white rats. In the latter category we may include the programmer who graduated from the classroom rather than from the laboratory. Green proposes to teach operant conditioners about programing, programmers about operant conditioning, and classroom teachers about both. The gap is too great for this book to span.

The initial repertoire which students



are assumed to have sets a lower limit on the student population for both a program and a text. The audience for Green's book is limited to those who have considerable acquaintance with psychological jargon or to those brave readers whose reading behavior is reinforced by less than total comprehension. One finds such terms as "learning curve," "asymptotic performance," "independent variable," "molar operations," "functional relationships," "escape and avoidance," "implanted electrode," "behavioral topology," "entropy," "noise," and "echoic" undefined. One expects in such a text a high rate of emitting the word "reinforcement," but one also expects from a programmer a logical sequence in which the word is taught before it is used to describe other events. Such is not the case here. The word is defined on page 40, which leaves it operating as an undefined term when it first occurs on page 10: "The present definition of verbal behavior is that it includes all behavior whose reinforcement is mediated by another organism." This is not the only violation of logical sequence as the author wrestles with the difficult task of deciding what should precede what. Familiar as those terms and formulae seem to most psychologists, it is a rare classroom teacher, indeed, who will comprehend the psychological foundations of programmed instruction on the basis of reading this text.

If this book is beyond the ken of the non-psychologist, perhaps it will offer a course in programmed instruction for the non-programmer. Terminological difficulties are minor unless one chooses to quibble over "implicit programming" for "intrinsic programming," "interverbal" for Skinner's "intraverbal," or "textual prompt" for "textual behavior." ("If the material is simply read, the behavior is not prompted." Skinner, 1957.) The non-programmer will, however, find many questions not answered. The careful reader will be bothered by inconsistencies. The knowledgeable reader will be bothered by inaccuracies.

Most programmers are presently evading the identification of the reinforcer involved in programmed learning. Apparently some students perform well without confirmation, an experimental

finding that is not mentioned anywhere in the text. If reinforcement theory is not to be dispensed with, we must find another reinforcer. Green describes a possible candidate: "Let us propose that reinforcement be interpreted as the result of the organism's reducing a gap between a prior and postaction state of affairs. Learning is a very sensitive and fragile form of this kind of situation. It requires that the organism recognize or discriminate his own state of knowledge and be capable of recognizing a change in it. He must discriminate the adequacy, the range of his own behavioral repertory and discriminate an increment in that range and repertory. Perhaps it is not the recognition that a gap between what is required and what is accomplished has been reduced that is crucial, but that this recognition must be established as a generalized reinforcer in order to be effective. The child must have a sense of the importance of learning" (p. 118). Presumably this 'sense of progress' is the reinforcer that remains when confirming answers are removed and the reinforcer that is absent when an over-simple program insults the student's intelligence. Later, however, Green gives a careful outline of how to develop a variable-ratio schedule in a program, with the reinforcer, conveniently enough, being the easily manipulable confirming answer rather than his hypothesized 'sense of progress.' To investigate the effects of schedules of reinforcement on learning from programs, experimenters are going to have to be more clever than this.

In an argument for low error rates on programs, this reviewer is credited by Green with having "shown learning to be more efficient if errors are reduced or eliminated" (p. 142). This achievement would gladden the heart of many investigators, including this one who merely claimed to have 'supported' the theory. However, in discussing his own data, Green says of an identical high negative correlation between error rate and examination performance: "We shall not take this as evidence that a high error rate produces poorer learning, although our particular systematic orientation might tempt us to do so. One must approach with caution the results of correlational analyses" (p.

189). Our systematic orientation has changed in 40 pages.

THE gap between the recipes for programmers contained in this book and the honest presentation of his own programming project is startling. Gilbert's advice—begin with a single student and let him teach you how to teach him—is liberally quoted, but Green admits that, given the extreme pressures on medical school students, "it was impossible for us to use students in the initial composition of the [medical sciences] program" (p. 181). The advice "the person most able to develop an effective program in a given subject is the person most familiar with that subject" (p. 151) stems, perhaps, from sad experience, as related in "none of the programmers engaged in this project were familiar with the technical aspects of the subject matter to be programmed . . . . The obvious limitations of the programmers . . . made inevitable a divergence in the emphases given various portions of the program compared with the emphases which the students desired, as given by the lecturer" (p. 181). These are the realities, perhaps, which laboratory psychologists must recognize as part of the problems of education. There is no admission or evidence in the book, however, that other programmers have found ways around the problems.

Examples of frames from actual programs are rare. Of four mentioned, only two exemplify significant variables, and the most emphasized one contradicts almost every bit of advice given elsewhere in the book. The frame: "The fasciculus gracillis has its origins in the *dorsal root neurons* below the

3

midthoracic level. It consists of uncrossed fibers. It is located in the *dorsal funiculus* of the entire cord. It

2

terminates in the nucleus gracilis of the medulla oblongata and its function is proprioception." This is to be seen several times with different randomly determined 'holes' in it, as indicated by 2 and 3 below the words to be dropped, "until the final frame called for all five items of information concerning the



fasciculus gracilis." Such a frame conflicts with: "the only information that should be in a frame is that required to answer the particular question" (p. 172); "when an excessive number of answers are required the linguistic structure of the frame is demolished" (p. 173); "the particular response should be evoked under as broad a spectrum of conditions relevant to that response as possible" (p. 154); and "students tend to ignore information following a blank in a frame . . . The programmer must guard against placing any significant amount of information after the last blank in the frame" (p. 154). The practical problems are enormous indeed, if theory is so little related to practice. The frame itself was introduced to illustrate "fading" or "vanishing," a programming technique which Green claims is too new to evaluate against "more conventional techniques." The first description of it is credited to Holland in 1960, although in fact the technique can be found described in Skinner's 1958 paper and in use in Porter's 1957 spelling program.

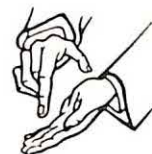
As a first course in programmed instruction, the text is incomplete. Holland's project at Harvard and the Dartmouth project are described; in the latter case, the report is detailed and new to the literature. There is no mention, however, of the projects at Earlham or Hamilton Colleges, nor of the many researches that have been reported below the college level, with the exception of a brief miscasting of Eigen and Komoski's research on set theory with high school students into a logic program for primary grades. It is unfair to claim that any particular study should be included. In the last five years, however, data have been gathered which could be included to raise the level of recent books above the 'pure talk' of the early papers in the field. The naive reader would hardly believe that so little is known even now.

As a stimulus to thought and indubitably to argument, this book is of interest to the professional 'operant-programmer.' As an illustration of the effects of programming experience on textbook writing, it may interest specialists in transfer of training. As a text for the untutored, it suffers from the faults

for which programmers often belabor textbooks: inadequate knowledge of the initial repertoire of the students and

poor analysis of the subject matter to be taught. In terms of the goals the author set, the book missed the mark.

## ON THE OTHER HAND



*In this Department CP invites discussion of reviews and of books reviewed. Here is the place for that kind of intellectual dissent that promotes progress in understanding. Let your criticism be ad verbum, not ad hominem. Seldom does a criticism merit more than half the space of the text criticized—never more than equal space and then only when the letter is interesting and well written. CP edits letters when it thinks they should be. Single-spaced letters will be returned for double-spacing.*

### FIE ON FESHBACH AND CP

S. Feshbach's review of Buss's *The Psychology of Aggression* (CP, July, 1962, 7, 268-269) was disappointing because it provided insufficient information about the book, because Buss's theoretical viewpoint was not clearly stated, and because valuable features were omitted.

The first part of the book examines laboratory studies of aggression, with emphasis on aggressive responses and reactions to hostile stimuli. The second part deals with aggressiveness as an enduring response tendency with attention to measurement, competing theories, and psychopathology. The last part examines developmental aspects and prejudice. In each area Buss toughmindedly assesses methodology and attempts to refocus and clarify issues and to point to meaningful next steps in research. There is a wealth of previously unpublished material (e.g., dissertations) as well as detailed reappraisal of classic research.

Buss's integration is a creative one in which prevails a systematic application

of a stringent S-R behaviorism. Although this writer, and evidently the reviewer, cannot share Buss's theoretical bias, it should have been noted that Buss does not merely reject the venerable frustration-aggression and psychoanalytic approaches: he has provided a provocative alternative in which aggressiveness is regarded as a habit, a view which allows laboratory and inventory to converge on the same phenomenon. Furthermore, Buss describes a new technique for laboratory study of aggression, one that eliminates ambiguities and ethical problems besetting prior research.

Clarity of exposition and explicitness in definitions and assumptions are the most striking features of *The Psychology of Aggression*. The reader knows when he agrees and disagrees, a factor that could have facilitated the reviewer's treatment. Feshbach's disappointment at not finding "a satisfactory theory of aggression" suggests a chimerical aspiration. It is highly unlikely that a single theory will be found to account for a domain including such diverse elements as conditioning of hostile words, physiology of anger, and social prejudice.

Finally, one shortcoming must be ascribed to Buss. Why was Buss disdainful of psychoanalytic theory when a modern offshoot of that theory provides a notion which may explain his own development as a productive behavioral scientist? To wit, the APA Directory gives Buss's birth year as 1924 while CP's editor shows him receiving the PhD in 1912—surely a classic instance of "regression in the service of the ego!"

TIMOTHY C. BROCK  
Iowa State University

### STICKING UP FOR THORNE

I would like to take exception to the review by Dr. Bugental of Dr. Thorne's book entitled *Personality* (CP, July, 1962,



7, 273-4). The objection is based on the factor of incompleteness. Since I have had the opportunity of using this text in my Human Adjustment course, I find along with my students that Dr. Thorne's fundamental undertaking proves to be very worthwhile and what is more to the point, he has achieved the task that he set out to accomplish.

What psychology needs at the moment is a thorough reconsideration and rediscovery of the individual. Dr. Thorne, depending on his vast storehouse of information about "things psychological" garnered from his clinical and scholarly duties, has directed his highly creative skill to the cogent point that personality is a constantly changing state. This concept of change is one that is not adequately handled by our discipline, especially by the experimentalists and the statisticians who are logically more concerned with the commonalities of personalities rather than with its uniqueness. As the psychiatrists in their mental status examination speak of the person's mental status at the time of the examination, Dr. Thorne too, emphasizes the personality state.

There are certain aspects of the book that have not been given their proper emphasis. They may be listed as follows: 1. Omission of the salient features of Chapter Two in which the various systems of psychology are set in terms of an operational analysis. 2. The theory of personality integration in terms of etiological factors of behavioral organization and hierarchical levels is unique and new. 3. The importance of conscious mental life as a central phenomenal given is stressed throughout the text. 4. The current clinical experience indicating the importance of such matters as the nature of becoming, as well as the person in the world, have become crucial in the existential problems that are faced by each person in trying to become self-actualized. This should have been mentioned even though there is very little objective research. 5. In attempting to state postulates, Dr. Thorne was outlining hypotheses which could become topics of large research programs in order to validate this approach. Thus, its heuristic nature was the main intent.

If these five pertinent omissions were included in Dr. Bugental's review, a fairer appraisal would have been the result.

T. S. KRAWIEC  
Skidmore College

#### WHAT? A GRUNTLED AUTHOR?

I have just finished reading Chuck Wenar's review of my book on clinical research design and analysis in the September issue of *CP* and I am so impressed by its astuteness that I feel absolutely impelled to write to you.

Dr. Wenar has been, perhaps, overly gracious in discussing the book's merits, but this is not uncommon among reviewers. What stirred me up was the skill with which he unerringly pointed out every major defect in the text, defects which I have discovered in the course of using the book as a text book during the past year. Dr. Wenar seems to have been able to have read the book not only from the level of his own advanced research sophistication, but also emphatically from the viewpoint of the unsophisticated clinician.

Dr. Wenar deserves a hearty round of congratulations for an excellent job of critical reviewing.

EUGENE E. LEVITT  
Indiana University Medical Center

#### SHOULD MURRAY EAT HUMBLE PIE?

The reviewer of *Readings in Child and Adolescent Psychology* by Lester D. Crow and Alice Crow (Murray, *CP*, July 1962, 7, 263) points out that it is "A collection of 92 abridged versions of articles, book sections and papers in developmental psychology." Here are the facts: 45 of the selections are abridgments, and 47 (more than half) of the selections are complete articles, excepting only the deletion of references at the end of some selections.

The reviewer also adds "one wonders what the book is for." He perhaps can find the answer in his own institution. There may be irony in the fact that the faculty at Syracuse University (of which Professor Murray is a member) has used, during the first two semesters after the book's publication 595 copies, or approximately 300 copies each semester.

LESTER D. CROW  
Brooklyn College

#### CP'S INTERFERENCE IN HUMAN LIVES

In dealing with the biographies of authors and reviewers, *CP* ought not to change psychologists' jobs without permission or to have them born at times that would have surprised their mothers mightily or to report live psychologists deceased or deceased ones alive. In most

of such cases, the biographees have a right to legitimate complaint; in others the errors are simply confusing to readers. I can understand some of the difficulties involved in the simultaneous achieving of accuracy, functional utility and reasonable readability in the sketches. So I do not want to castigate too severely *CP*'s editor. But if somebody does not point out errors, how is *CP* to know its need to find ways to avoid them?

It has recently been pointed out that *CP* unjustifiably started a new department at the University of Pennsylvania (*CP*, May, 1962, 7, 199), inexcusably lost Leontiev entirely (*CP*, June, 1962, 7, 245), and gave Arnold Buss a PhD twelve years before he was born rather than 28 years after. Also *CP* recently gave Judson Brown a job at Iowa State when State University of Iowa was actually paying his salary (*CP*, Feb., 1962, 7, 41). Now other errors have come to light. Last Spring *CP* in one brief sketch changed mightily both the jobs and the geography of two authors (*CP*, Mar., 1962, 7, 116). It erroneously moved Edwin B. Parker from the Institute of Communication Research at the University of Illinois and put him to work in an organization of the same name 1800 miles away at Stanford. While it was at it, it sent Jack Lyle from the School of Journalism at UCLA, where he is sure he is located, to work with Wilbur Schramm at Stanford. Last summer *CP* (*CP*, Aug. 1962, 7, 300-308) failed to note that Horace B. English had died thirteen months before the review of his book was published.

Who does all this human erring? The reviewers who send in the biographical material? The editorial assistant? Typists? Printers? Or is it the editor himself? And whoever it is, isn't it the editor's final responsibility?

FILLMORE H. SANFORD  
University of Texas

Editorial Note:

Yes. FHS

#### UNLABELED REPRINT

Some time ago I was sent two books by Philip E. Vernon, both published by the Philosophical Library, to review for *Contemporary Psychology*. One of these was *Intelligence and Attainment Tests*, originally published by the University of London Press in 1960. That one, I reviewed (*CP*, Oct. 1962, 7, 378). The other *The Measurement of Abilities*, was designated on the rear of the title page as "Published 1961 by the Philosophical



Library Inc . . ." Apparently it is only a disguised reprint of the 1955 revision of Vernon's excellent 1939 volume of the same title. In the 6½ page Bibliography on pages 259-265, there is just one reference later than 1955, that being the 1960 version of *Intelligence and Attainment Tests*. Furthermore, the later of its two prefaces is dated May, 1955. Slowly one comes to realize that this supposedly new book is not new at all. That one, I think CP should not review. It probably should not even be mentioned.

JULIAN C. STANLEY  
The University of Wisconsin

#### A MIXED-MODEL COOKBOOK

Frederick Kanfer's review (CP, August, 1962, 7, 245) of Grinker, MacGregor, Selan, Klein, & Kohrman's book, *Psychiatric Social Work: A Transactional Case Book*, prompted us to take a second look at both the review and the book. The first matter that struck a dissonant note was the reviewer's consistent use of the term "interaction" where the term "transaction" was obviously indicated. This, in spite of the fact that Grinker, et al., explicate (but in a minimally adequate fashion) the differences between actional, interactional, and transactional levels of organization (p. 19)—a differentiation *crucial* to the full understanding of the implications and parameters encompassed by the transactional frame of reference.

Naiveté on the reviewer's part in the fundamentals of transactional theory is further reflected in his failure to comment upon Grinker's reductionistic use of the transactional paradigm. By the title of the book, one would infer that the authors were rather heavily committed to the transactional approach, but their neglect to consider such essential components of the corrective transaction (psychotherapy) as assumptions and their prognostic reliability, value hierarchies, and expectancies, clearly points up their delimited use of the transactional frame of reference, both in theory and in practice.

The reviewer noted a "... remarkably close correspondence . . . between the proposed theoretical framework and its use in case illustrations"—a correspondence which we did not perceive. The authors' mixed-model reductionism has undoubtedly obscured this hiatus in theory and practice. Another significant contribution muddying the already muddy theoretical waters is the following factor astutely recognized by Kanfer:

"... the occasional overlapping of transactional and psychoanalytic language . . . suggests that a therapist's bias *vis à vis* a personality theory may predetermine the contents of therapeutic explorations and affect the relationship, even though the therapist's explicit therapeutic operations do not stem from the same theory." Here, the present writers would agree wholeheartedly with Kanfer and would add that Grinker, et al., are theory-bound to traditional, dyadic, second-hand analytic therapy in spite of their Wagnerian overtures to transactional theory (which in practice amount to little more than occasional, Haydneseque *appoggiaturi*). If one doubts this, a quick look at the index will reveal a multiplicity of such terms as abreaction, transference, countertransference, super-ego, ego functions, ego strength, id, oedipal feelings and primal scene; yet, as previously noted, *nowhere* in the index—or in the text—will you find *mention* of such key transactional concepts as assumptive world, expectancies, and significances.

Two other points on which Kanfer lets Grinker off rather lightly are deserving of brief elaboration. The latter speaks of "The researches reported in this volume . . ." and Kanfer in his review quietly but pointedly places quotation marks around this use of the word "research" without further comment. We feel constrained to carp at the authors' use of the word "research" (nowhere in the volume can one find the report of any empirical research) not just as a matter of pedantic preciosity or academic gamesmanship, but because it is a reflection of a very real semantical and epistemological problem that must be faced. It appears that by the word "research," Grinker, et al., are denoting a class of events which we prefer to label "conceptualization" in the form of scientific inquiry (the problem of problemization)—a process *apart* and *distinct* from empirical hypothesis testing.

The second point concerns the ubiquitous issue of the supervision of psychotherapy. Kanfer seems to soft-pedal the insidious (yet flagrant to those with their third ear to the ground) medical *arrogation by containment* in Grinker's miasmatic assertion of noblesse oblige: "For our purposes we consider that psychotherapies are conducted in clinics under *psychiatric administration* (italics are the authors') so that we can transcend the problems of training, supervision and responsibility (italics ours)." Such imperious transcendentalism is disturbing

to more democratically-minded Emersonians. These overtones of professional imperialism are indeed "disturbing to many," as Kanfer puts it. Professional imperialism on the part of *any profession* by assimilation, containment, preemption, and institutionalization via whatever Machiavellian power-politics raises *ethical, moral, and social* issues with which regrettably few social and behavioral scientists have concerned themselves.

In brief for both Grinker's transactions with transactional psychology, and Kanfer's transaction with Grinker's transactionalism, we suggest *caveat emptor*.

JAY TOOLEY  
STEVE PRATT  
Larned (Kansas) State Hospital

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- YABLONSKY, LEWIS. *The violent gang*. New York: Macmillan, 1962. Pp. xiii + 265.



For what a man had rather were true he more readily believes. Therefore he rejects difficult things from impatience of research; sober things, because they narrow hope; the deeper things of nature, from superstition; the light of experience, from arrogance and pride, lest his mind should seem to be occupied with things mean and transitory; things not commonly believed, out of deference to the opinion of the vulgar. Numberless in short are the ways, and sometimes imperceptible, in which the affections colour and infect the understanding.

—FRANCIS BACON





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*Contemporary Psychology: A Journal of Reviews* is published monthly. The subscription per year is \$10.00, foreign \$10.50, single number \$1.00.

Published by the American Psychological Association at 2210 W. Dallas, Houston, Texas and 1333 Sixteenth St., N.W., Washington 6, D. C.

Second-class postage paid at Houston, Texas and at additional mailing offices. Accepted for mailing at special postage rate stated in section 132.122, Postal Manual.

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# Contemporary Psychology

A JOURNAL OF REVIEWS

VOLUME VIII

March, 1963

NUMBER 3

## Moil in a Mainstream

Colin Cherry (Ed.)

*Information Theory: Papers Read at a Symposium on 'Information Theory'*

Held at the Royal Institution, London, August 29th to September 2, 1960. Washington, D. C.: Butterworth, Inc., 1961. Pp. vii + 476. \$16.50.

Reviewed by WILLIAM A. HILLIX

Edward Colin Cherry, who edited the present volume, was trained as an engineer and is now Professor of Telecommunication at Imperial College, University of London, where he has been since the end of World War II. His long list of earlier publications include the books *Pulses and Transients in Communication Circuits* and *On Human Communication*. The reviewer, William Hillix, is a Senior Scientist at the Navy Electronics Laboratory in San Diego, and also Lecturer at San Diego State College. His career includes Navy experience during WW II, teaching English, farming, fathering twins, completing a doctoral program with Melvin Marx, and a year as USPHS postdoctoral fellow. He went to his present position in 1959 and there he is. He has recently been collaborating with Melvin Marx on a book, *Systems and Theories in Psychology*, soon to be published by McGraw-Hill.

THE BOOK Dr. Cherry has edited is not wholly on information theory. He says:

"The papers now presented range over a wide field and, in spite of the title, are not confined to pure information theory. . . . At the same time, I have tried to select out of all the

papers offered those which seemed most likely to appeal to a common audience."

What is this common audience? I suspect that they have less in common than Dr. Cherry hopes, but let me try to deduce from the 36 papers the properties of the man to whom all of these papers could be addressed. He must certainly have an interest in coding and in the detection and correction of errors (papers 1-7). He must want to know more about communication systems than how to dial his telephone (8-10). He is fascinated by the logic of hypothesis-testing (11) and by human information-handling (12-18). Neurons, and especially neural models, are his cup of tea (19-21). Electronic learners interest him (22-28, 30), as does classification theory (31-33) and various linguistic problems (29, 34-36).

I, at least, have not met this man, but perhaps he can be approximated. The reason he can be, I believe, is that all of these diverse interests have a node at the data processing machine. Information theory is now just one of the threads leading to the node or, to put it another way, information theory has metastasized, and, through its change in form, has begun to invade much of the scientific body.

Because of this somewhat disorderly

growth, the authors of this collection represent medical laboratories, physical laboratories, electrical engineering and electronic departments, psychology departments, phonetics departments, and departments of neurophysiology, as well as the main stream in mathematics and communication. This "common audience" has grown less common as the field has grown, and we must bear in mind that this volume includes only a sampling of the available disparity.

The psychologist is not heavily represented in this fourth London Symposium on Information Theory. He will do a lot of pecking in Cherry's book for a few grains of corn. Of the 52 contributors involved in this volume's 36 papers, at least four are psychologists, and five or six others are suspect. The remaining forty-odd are clearly non-psychologists who are happy with their lot, and they have written papers whose relevance to psychology is remote at best. That is certainly no criticism either of the authors or the editor. In what follows, the reader should remember that I am considering this book only from the point of view of the psychologist; I am not at all competent to judge how well this volume achieved its primary goals.

The first ten papers on coding, detection theory, statistical theory, and telecommunication systems are least likely to interest the psychologist. Their tenor is very largely coding, with additional specialization on errors and error corrections. A. M. Andrew in his paper does make a side reference to coding in sense-organs, but that is as close as the first ten papers come to directly biological subject matter. I. J. Good's paper on the evaluation of evidence is of general scientific interest. The next 10 papers on human reaction



to information (12-14) and sensory information and biological models (15-21) are of most direct relevance (I will return to some of these later). The psychologist can afford to be mild rather than wild about the next seven papers despite the topic *allegedly represented* (learning mechanisms and other artifacts). "Learning in random nets" by Minsky and Selfridge is an exception to this, but their side-swipes at perceptrons can be found elsewhere. The last group of papers again present slim psychological pickings, though here at least the topic (classification theory; syntactics and semantics) is of enough general interest to make reading worthwhile. The simplicity of Professor D. M. MacKay's concluding paper "The Informational Analysis of Questions and Commands" is a welcome relief after the (for your reviewer) fierce problems presented by the mathematics in much of the rest of the book.

I would single out one paper from this volume as most clearly demonstrating the usefulness of information theory in unifying psychological data. It is E. R. F. W. Crossman's "Information and Serial Order in Human Immediate Memory." Dr. Crossman is able to reconcile findings from experiments using lists of varying length, composed of symbols of varying information content. Immediate memory capacity on the various tasks is shown to be more nearly constant than was supposed earlier. Dr. Crossman's contribution was noting that the serial order of symbols, as well as the identity of symbols, must be remembered.

The other papers of psychological interest generally either did not use information theory or measures, or did not really need to use them. Examples: Averback and Sperling could get by as well in reporting on visual memory by using only the number of letters reported as units; they actually computed the short term visual memory capacity in bits, but might not have bothered if they had not been about to attend a symposium on information theory. Information theory really neither added nor detracted much from their results. The same could be said

of Julesz's interesting work on binocular depth perception. Its only relation to information theory was indeed remote; a computer was used to generate the visual presentations. Green and Swets used concepts from detection and decision theories in generating and discussing their experiment on deferred detection decisions.

Is the promise of information theory for psychology being realized? The papers presented in this volume seem at least to justify the question.

I believe that information theory as a mathematical set of tools is presently ahead of the empirical observations to which it will eventually be applied. To say that the promise of information theory is not being realized is much like saying that the promise of calculus is not being realized. Both wait upon the collection of more data to which the models are appropriate.

It may have once appeared to the over-optimistic that information theory would revolutionize psychology. There was not enough data available to support a true revolution. The promise will be a promise for a long time, just as for the lover in Keats' poem. As often happens in a new theoretical endeavor, the abstraction has initially attracted too much attention to itself, at the expense of the world to which it must apply.

This does not mean that the psychologist now or at any time can afford to ignore developments in information theory. Information theory and its ramifications remain in active turmoil. Cherry's collection serves notice that the psychologist who ignores basic information theory as one of the accepted tools of his trade, and the new developments that are continually augmenting this tool, will soon find himself on the outside of one of the main streams of activity.

Two sidelights on the main issues deserve a passing comment. First, it is clear that animal (notably rat) psychologists have a new set of allies. Nine papers report on the behavior of machines; only eight give behavioral data on living animals. The authors of the first set of papers should now be available to reinforce their beleaguered col-

leagues who have for years been saying either, "I don't care whether people behave like my subjects," or "I don't see why people *shouldn't* behave like my subjects."

Finally, it has occurred to me that the proceedings of a symposium are sometimes like a burlesque show. Proceedings show the almost naked workings of science; the discussions reveal errors and animosities, corrections and courtesies, almost as though scientists were people. As in burlesque, the product is not always smooth, artistic, or well-connected, but it is revealed, and in that there is honesty.

## McNemar

Quinn McNemar

*Psychological Statistics: Third Edition.* New York: Wiley, 1962. Pp. v + 451. \$7.75.

Reviewed by BENJAMIN FRUCHTER

The author, Quinn McNemar, the President-Elect of APA, needs little introduction and that is just what he will get. The reviewer, Benjamin Fruchter, received his PhD from the University of Southern California and has had the great good fortune of spending most of the time since then at the University of Texas, where, among other things, he is presently Professor of Educational Psychology, a position that involves, among other things, the teaching of statistical methods to graduate students. His publications include the book *Introduction to Factor Analysis* (1954).

THIS BOOK is the third edition of a well known and widely used textbook on psychological statistics. It is strongest in the area of analysis of variance and hypothesis testing, although there is a balance of coverage, including materials on descriptive statistics and correlational methods.

Since the presentation of the material usually covered in an introductory course is somewhat condensed, its use as a textbook is most appropriate at



the "intermediate" level. The commonly used statistical methods, with the exception of factor analysis, are covered with varying degrees of thoroughness. The book is directed primarily to students, with good discussions of assumptions and uses of the statistical methods. Some topics such as discriminant function, which is covered in less than a page, are presented in a manner that is more suitable for the reference worker. The style of writing is for the most part as clear and concise as can be expected for such complicated subject matter.

New material in this edition includes a chapter on trends and differences in trends, and a summary of the empirical work that has been done on the effect of assumption violations on the  $t$  and  $F$  tests. Also some additional proofs and derivations of the formula for the standard error of the mean, for the difference between independent proportions, and of the connection between variance and chi-square are included. The relationships between some statistical methods, such as the  $\chi^2$ ,  $t$ , and  $z$  tests with the  $F$  test, reliability with analysis of variance, and interaction with correlation, are indicated. A section of exercises and discussion questions relating to the various chapters is included at the back of the book. The emphasis in the exercises is on reasoning and understanding rather than on computation and derivation.

Standardization of symbols is still a problem in psychological statistics. McNemar has shifted to the use of three symbols for the standard deviation,  $\sigma$  for the population,  $S$  for the maximum-likelihood estimate, and  $s$  for the unbiased estimate. He also uses  $\mu$  for the population mean, and  $M$  for the sample mean, with the  $x$  symbol being reserved for use only in the section on the analysis of variance.

With the exception of some additional distribution-free methods very few of the newer developments have been incorporated into the revision. There is, for example, no mention of decision theory, Markov processes, or the electronic computer.

In summary, this is a somewhat augmented and expanded (in length and price) version of the previous edition

with most of its strengths and weaknesses. The stress is on the assumptions and understanding of the statistical

methods, and there is relative freedom from inaccuracies, inconsistencies, and minor errors.

## Left Hand on the Right Side?

Jerome S. Bruner

*On Knowing: Essays for the Left Hand.* Cambridge: Belknap Press of Harvard University Press, 1962. Pp. 165. \$3.75.

Reviewed by RICHARD JESSOR

*Jerome Bruner, the author, is Professor of Psychology at Harvard University and Co-Director of the Center for Cognitive Studies at that institution. He has had faculty connections with Princeton University, with Cambridge University, with the Institute for Advanced Study. But mostly he has been at Harvard University and there has done so much, published so widely and received so many honors that he constitutes a problem for CP's biographer. The reviewer, Richard Jessor, might be said to have taken two doctoral degrees from Julian Rotter at Ohio State, one by effort and one by marriage. (His wife, Lee, is an OSU psychologist as well as a wife and mother.) He has inclined toward a phenomenological approach in his study of personality and is presently plying that approach in a research application of Rotter's social learning theory. In the last four years with his wife as a collaborator, he has worked on a study of deviant and conforming behavior in a tri-ethnic community in Southern Colorado. He is Professor of Psychology at the University of Colorado.*

IN THE day-to-day practice of our craft it is difficult to find a vantage point from which to gain perspective and to sense the course of history. What is ephemeral and what is lasting are matters of debate and there is no escaping the fact that, to some extent, each man writes his own history. Nevertheless, the deep ferment of the past decade in

psychology has become clear to many psychologists, and some of its causes and some of its consequences can at least provisionally be discerned. Most obvious has been the restless discontent over self-imposed constraints as to what constitutes our proper subject matter and what are the proper methods of scientific study. The emphasis on behaviors and on operations, both in the narrowest sense, had simply not yielded an image of man satisfactory to ourselves or to scholars in the other sciences of man and in the humanities. What seems to have been missing—and what has since emerged as perhaps the clearest consequence of the ferment—was a concern for man as an *experiencing* being, an organism with relatively unique capacities for language and symbolism, with deep sensibilities, imaginative, a thinker and a seeker after meaning.

It is these concerns which animate this beautifully written and eloquent set of essays. Revised and reworked from a talk or paper delivered on some occasion during the past five years, each essay ventures boldly yet sensitively into some realm of man's experience, frequently a realm about which psychology has had, till now, little to say. The essays range widely from a consideration of the essential structure of a creative work to an appraisal of art as a source of knowledge; and from a concern with the importance of personal acts of discovery in the educational process to an analysis of the role of knowledge in commitment and action. Despite their separate origin and the



sweep of their subject matter, the essays have a surprising unity and coherence—they all converge on some aspect or function or consequence of knowing.

There is another basis for connectedness in these essays. They are written for the left hand, the hand traditionally symbolizing imagination, feeling and intuition in contrast to the right which has been the representative of order, logic, and rational deliberation. The right hand in psychology, according to Bruner, "... has become too stiff with technique, too far from the scanning eye." The essays, then, represent an attempt to explore the approach of the left hand, an approach perhaps best suited to the initial charting of unknown regions of man's experience, "... an approach whose medium of exchange seems to be the metaphor paid out by the left hand. It is a way that grows happy hunches and 'lucky' guesses, that is stirred into connective activity by the poet and the necromancer looking sideways rather than directly." Lest the reader become apprehensive at what might sound like utterly undisciplined inquiry, I can assure him that throughout these explorations the right hand knew what the left hand was doing.

THE BOOK is divided into three sections, the first entitled "The Shape of Experience," the second "The Quest for Clarity," and the last "The Idea of Action." In "The Shape of Experience" the arena for discussion is the creative enterprise represented largely by literature and painting. Bruner is concerned with art in terms of what it can tell us about thinking, especially creative thinking, and how it provides a source of knowledge. To this end he explores with impressive skill the role of metaphor as a primary mode of concept-formation in art. He shows the way in which the device of the metaphor meets the cognitive needs for economy and organization by suggesting hidden likenesses in diverse aspects of experience and presenting us with surprising and powerful unities.

A not insignificant emergent from Bruner's analysis of metaphor is the deep similarity of the processes of creative concept-formation in those tradi-

tionally sundered disciplines—art and science. One is reminded here of Bronowski's profound essay on *Science and Human Values* in which he tells us how Kepler "felt for his laws by way of metaphors" and instructs us that the symbol and the metaphor are as necessary to science as to poetry. Such discussion is at a refreshingly far remove from that sterility we psychologists not long ago indulged in about the problem of "surplus meaning" in our concepts.

The essays in the next section explore the process of imparting and acquiring knowledge—the domain of education. Here, too, Bruner's concern is for those kinds of conceptual structures which organize knowledge in economical fashion, permit easy retrieval from memory, and enable the learner to go beyond the available information. He shows how objectives such as these—which honor the connectedness of knowledge—are best achieved by an architecture of education that permits and encourages *personal discovery*. There are in this discussion the seeds of a theory of learning in a richer sense than psychology has thus far construed that term.

A fact which becomes abundantly clear from the discussions of education in this section and of esthetics in the preceding one, is that traditional models of motivation have little or no relevance to an understanding of knowing. Bruner finds it necessary to reject the "extrinsic grammar" of drive theory and to seek a congenial cognitive approach to motivation in Robert White's concept of competence. With it he is able to suggest the nature of the satisfaction intrinsic to looking at a painting, listening to a quartet, or working out a mathematical problem.

THE FINAL set of essays follow out some of the ways in which ideas relate to action in the contemporary world. Though somewhat less coherent a unit than either of its predecessors, this section includes a perceptive analysis of the ways in which the "latent culture"—the guiding myths and values and socialization practices of our society—inevitably shape and take their toll of human experience. Drawing out the implications of the irreversible limits suggested

by the sensory deprivation literature, Bruner asks: "Are we mindful of what it takes by way of intensive exposure to certain forms of experience to unlock human capacities of certain kinds, whether for looking at art or for manipulating abstract symbols?" His answer, which cumulates throughout the book, is, of course, negative, and in this sense the essays represent a welcome form of social criticism, criticism illuminated by empirical knowledge in the science of man.

There is much more in this little book to catch the inquiring eye—the recurrent concern for the conservation of cognitive capacity; the provocative tracing of the role of effort in esthetic appreciation and in learning; the repudiation of such hoary ideas as "readiness" in education; and the development of the concept of effective surprise in creative work.

It is possible, obviously, to argue with many of Bruner's views, but it seems inappropriate to the spirit of their presentation to do other than nurture them. Clearly Bruner has been successful in what he set out to do, to explore the range of the left hand in the realm of knowing. His fresh and often poetic mapping should make travel much easier for the right-handed workers who will follow behind.

In a remarkably penetrating Epilogue to the third volume of *Psychology: A Study of a Science*, Koch details the decline of the "Age of Theory" and notes the consequent openness of the contemporary psychological scene. He concludes: "The more adventurous ranges of our illimitable subject matter, so effectively repressed or bypassed during recent decades, are no longer proscribed." Bruner's book bears him out.



*In order that a man may stop believing in some things, there must be germinating in him a confused faith in others. It is curious to note that almost always the dimension of life in which the new faith begins to establish itself is art.*

—ORTEGA Y GASSET





# Views of Science. Microscopic and Very Otherwise

Arthur Pap

*An Introduction to the Philosophy of Science.* New York: Free Press of Glencoe, 1962. Pp. xiii + 444. \$7.50.

Reviewed by FELIX E. GOODSON

*The author of this posthumous book is Arthur Pap who at the time of his death in 1959 was a member of the Yale Department of Philosophy, and who, to quote Brand Blanshard, was "one of the ablest younger philosophers of his generation." He was born and brought up in Zurich, but came to the United States in the forties to earn his PhD at Columbia University and then to stay on in this country. The reviewer, Felix Goodson, is an experimental psychologist (University of Missouri PhD) who has always had an enduring interest in philosophy with particular emphasis on the problems inherited by psychology from philosophy. He is Associate Professor of Psychology at DePaul University where for a number of years he has taught the history of psychology. Always highly productive of journal articles, he is currently working on a book on psychological theory. In the planning stage is another book, this one to be called Themes in the History of Psychology.*

THE BOOK contains 420 pages (19 chapters) of intensive, at times laboriously minute examination of science—its language, methods, and problems. It is divided into five sections: (1) meaning and verifiability; (2) mathematics, logic and experience; (3) induction and probability; (4) causality and laws of nature and (5) explanation and justification. In his treatment of these topics, although such is not explicitly stated, the author has three basic orientations—logical positivism, relativity

theory, and phenomenology—which powerfully affect both his arguments and his conclusions.

One of the major tasks of the philosophy of science according to Pap is "to distinguish different kinds of 'proofs' corresponding to different kinds of propositions, and thereby to prevent a confusion of different standards of justification of beliefs, and to make explicit the criteria which scientists apply in evaluating the degree to which the known evidence warrants a given proposition." This comment sets the tone for the greater portion of the book. Thus in direct alignment with the tenets of logical positivism the author analyzes many of the statements and assumptions of science in terms of the various kinds of evidence one can accept as establishing their truth. Although syntactics, i.e., the rules governing the manipulation of symbols per se, is dealt with primarily in Chapters 5 through 8, where such topics as the nature of arithmetical statements, the laws of logic, and types of geometry are analyzed, the author uses logical analysis as a constant tool throughout the book in his search for the conditions necessary for the establishment of empirical truth.

The style and form of expression vary greatly from one topic to the next. Curiously, in those chapters dealing with formal logic, mathematics, and theory of probability the sentence structure is often so involved that one is prone to recall that Pap's first language was German and that one of his early fixations was on the Hegelian dialectic. The reader's task is made even more

difficult by the author's tendency to deal at unusual length and in exhausting detail with issues which from the standpoint of the day to day operations of science seem sterile and trivial. This immersion into minutiae most often occurs when the author seemingly becomes obsessed with the need to clarify logically the meaning of a given statement. It is not that such analyses are incorrect or without significance in the confines of logical positivism, it is simply that such detailed perusal seems alienated from the ongoing work of scientists and the development of their disciplines.

IN CRITICIZING Pap's style it must be remembered that the book was published posthumously, and that a number of the essays may have been presented without a final, critical scrutiny on his part. Further, it must be admitted that the unique character and one of the appealing aspects of the book is derived from the pervasive presence of the author's own personality which manifests itself not only in his curiously consistent reactions to issues but to the style within which these reactions are expressed. Indeed, as one follows the occasionally tortuous threads of the writer's arguments it becomes apparent that he (the reader) is participating in the creative thinking process with all its false starts and meanderings. Sometimes when the author becomes aware of the involvement of his discussion he begins the next statement with, "The point is . . ." and then an effort is made to recapitulate and clarify his thinking. Thus the reader follows Pap into numerous blind alleys and along many trivial tangents, from which at times he is rescued by recapitulation. On occasion, however, the argument simply disappears into omnidirectional ambiguity leaving the reader defending his own lack of insight with critical commentary.

It must not be assumed that the entire book is tedious and abstruse. This is not the case. Certain essays, for instance those on geometry, causality, determinism, and behaviorism, are remarkable for their clarity and coherence. Indeed, the entire book is marked by inconsistency of organization and expression. It fluctuates from the tedi-



ously ambiguous to the impeccably clear; from lucid coherency to almost complete lack of integration and organization; from pedantic hairsplitting to profound observation; from meticulous analysis to expansive generalization. Yet the many faults of the book are far outweighed by its virtues; the greatest of which is Pap's piercing insight into difficult issues and his ability to consider such issues with a depth of understanding rarely achieved.

Here are but a few of Pap's conclusions: (1) the major objective of philosophy should be an obstinate search for clear meaning; (2) so called private experiences are confirmable in terms of publicly observable stimuli; (3) the widely held belief that Euclidean geometry has been refuted by the development of consistent non-Euclidean geometries is completely unfounded; (4) an explanation of behavior in terms of inferred needs or motives is not only legitimate but highly desirable; (5) Hume was correct when he stated that causation is nothing more than a relation of uniform succession holding between observable changes; (6) relativity theory finds its roots in subjective idealism, and Newtonian mechanics in physical realism; (7) determinism of human action is incompatible with freedom—and a failure to understand this truism is due to the tendency to equate freedom with "rational choice" and determinism with compulsive or coerced behavior; (8) teleological explanations are clearly legitimate scientific devices; (9) phenomenal states of organism are important determiners of behavior and should be studied by psychologists.

These conclusions are in each case arrived at following an exhaustive examination of related issues and arguments. Admittedly, to remove them from context and qualifications makes them stand out in dogmatic relief; yet they give indication of the diversity of topics considered and suggest, which is actually the case, that the author has great confidence in his own rational processes and a capacity to state fearlessly the conclusions arrived at by their indulgence.

Although the book demands more than a casual grounding in symbolic logic, mathematics, and physics before

it can readily be digested, it is highly recommended for graduate students in psychology. Pap gives priority to the problems of physics but he also considers at length certain issues of interest to many psychologists, i.e.: mind versus behavior, free will versus determinism, teleology and emergence, and the status of value judgments. Furthermore, in spite of previously mentioned problems of style and expression Pap does a better than adequate job of treating the traditional issues of philosophy of science, and he presents a truly exceptional evaluation and discussion of the quasi-metaphysical issues and assumptions which lie at the outer edges of science.

## Talent Search

John C. Flanagan, John T. Dailey,  
Marion F. Shaycoft, William A.  
Gorham, David B. Orr & Isadore  
Goldberg

*Design for a Study of American Youth: 1. The Talents of American Youth.* Boston: Houghton Mifflin, 1962. Pp. 240. \$1.95 (paper) \$4.00 (cloth).

Reviewed by  
DONALD L. THISTLETHWAITE

*All authors of the present book are associated with the American Institute of Research. John Flanagan is the President of the Institute as well as Professor of Psychology at the University of Pittsburgh. John Dailey has been program director since 1958 of the Institute's Project TALENT, an enterprise from which the present book springs. Shaycoft, Gorham, Orr and Goldberg are his co-workers. The reviewer, Donald Thistlethwaite, did his doctoral work with Edward Tolman at the University of California. From Berkeley he moved to Western Reserve University and to the University of Illinois, and then served for a while as associate director of research with the National Merit Scholarship Corporation. In 1961 he moved to Nashville, Tennessee, to become Professor of Psychology*

*at Vanderbilt. His dual primary interests are in social and in educational psychology, and in line with these interests he has been conducting a study, under the sponsorship of the U.S. Office of Education, on factors influencing the recruitment and training of talented college students. He will produce a book on this matter.*

THE title of this highly readable book, the first of four initial reports planned for Project TALENT, is apt. It literally presents a plan, or scheme for a study of American youth. No substantive results are presented. Consequently proof of the values of this unprecedented undertaking must await future reports. We must be content to appraise prospects.

Project TALENT is an educational research program which began in March, 1960 with the testing of nearly 500,000 students in over 1,300 secondary schools. Like Terman's study of gifted children, the project aims to follow up examinees at periodic intervals—one, five, ten, and twenty years after graduation from high school. Like the wartime research on aptitude classification test batteries it seeks to discover formulae for predicting success or failure on the job. The task which Flanagan and his associates have set for themselves is much more difficult than either of these parallels, since a broader range of talents is to be related to success in hundreds of civilian careers.

Half of the book is devoted to describing sample items and the rationale for each test and inventory included in the two-day test battery. The battery included approximately 40 aptitude and achievement tests, a student activities inventory yielding scores on 13 personality scales, an interest inventory designed to assess 16 interest areas, and a 400-item student information blank. With two exceptions all of the tests and inventories were new. To insure that each test made a unique contribution to the battery it was required that a significant part of each test's reliable variance be free of overlap with any of the other tests.

It is hard to say how successful will be this attempt to discover better meth-



ods of identifying human talents. Certainly the authors' predictive battery is a model of careful workmanship and a highly sophisticated sample of the art. Of course, the hard-headed psychologist wonders whether the art will ever be equal to prediction of success in "hundreds of careers within many occupational fields." Even though the sights may be aimed too high there seems little doubt that Project TALENT will increase our information about the talents predictive of success in many occupations.

SOME of the questions posed in the authors' prospectus are of the causal variety: How can talent be developed? How can the talents of American youth be brought to a point of high productivity and usefulness? Thus a second aim of Project TALENT is to discover better methods for developing and utilizing human talents. The major focus of manipulable policies and events at this stage of the project is necessarily the high school. By comparing the products of high schools which differ in size, teaching staff, per-pupil expenditure, curriculum, guidance services, etc. it will be possible to identify school characteristics associated with the greatest payoff. Although the designs to be used in testing out the effects of different educational and counseling experiences in high school are only briefly sketched, they appear adequate for the task. For example, the authors state that with their IBM-7070 they can compute partial correlation coefficients holding constant up to 90 or 100 other measures.

One possibly serious omission in the data collection procedures is the failure to obtain measures of the personal and behavioral characteristics of teachers. Some teachers are more skillful at nurturing and developing talent than others. Knowledge of the characteristics which distinguish effective teachers should help us to select and train teachers. Such data might well have more important implications for developing talent than the school data collected. It is hoped that this omission will be remedied and that equal attention will be given to the student's educational experiences in college in the follow-up surveys on Project TALENT.

There are few psychologists who are not concerned with at least some of the prediction, counseling, and training problems posed in this important re-

search effort. Certainly this book is required reading for all persons wishing to follow the progress of Project TALENT.

## Forty Years a Revisionist

Franz Alexander

*The Scope of Psychoanalysis: 1921-1961.* (Selected papers of F. Alexander). New York: Basic Books, 1962. Pp. xix + 594. \$12.50.

Reviewed by STANLEY F. SCHNEIDER

*The author, Franz Alexander, came from Budapest to this country in 1930 to accept an appointment as Visiting Professor of Psychoanalysis at the University of Chicago. In 1932 he became the first Director of the Chicago Institute of Psychoanalysis. Since that period, both in Chicago and more recently on the West Coast, he has continued to work vigorously and to write prolifically in the psychiatric and psychoanalytic area. The three of his books he reports himself to remember most clearly are The Age of Unreason (1942), Fundamentals of Psychoanalysis (1948) and The Western Mind in Transition (1960, CP, July 1961, 6, 244). Stanley F. Schneider, the reviewer, received his University of Michigan PhD in 1953 and since that time has worked mostly in Ann Arbor, first in the Department of Psychology and later in the Department of Psychiatry in the Medical School. His interests and activities have involved the training and teaching of clinical psychologists, the psychiatric education of medical students, and both the public and private practice of clinical psychology. He has recently become chief psychologist at a psychoanalytically oriented training clinic in Los Angeles, and there his pattern of activities has moved somewhat away from the academic and somewhat toward a service orientation. He still, however, finds himself involved*

*with the development of students and with the large questions of where psychology is going and what can be done to see that it's not too late when it gets there.*

THE NEARLY fifty essays in this volume reflect the productive work of one of the most inventive and controversial figures in psychoanalysis. They show a remarkable diversity of content, although most of them are devoted to psychoanalytic theory and treatment. Alexander's earliest papers are products of an especially exciting period in the history of psychoanalysis, marked by Freud's development of the structural hypothesis. Alexander seemed kindled particularly by the complex network of mutual influences between the ego and superego, and by pathological states characterized by the intransigence of the superego as well as by the possibilities inherent in the modification of the severity of this newest psychic institution.

The author shows a rather striking consistency in the development of his ideas, predicated upon the *relative* neglect of the more immutable features of the personality, specifically those subsumed under the id. If we accept this core proposition, we may examine the way his rich contributions radiate, like the spokes of a wheel, to clinical understanding, theory, treatment, to profes-



sional education and training and to the state of modern man in society. The prevailing emotional tone of this volume is one of optimism, the prevailing attitudes those of flexibility, adventure, belief in change and modification, open-mindedness, respect for scientific rigor and an apparently boundless curiosity about what people and social institutions are like. It is easy to be caught up in Alexander's sheer intellectual zest and persuasiveness, not only out of respect for the man and his ideas, but also because he writes with a beauty and simplicity of style surpassed only by Freud himself among the more prolific psychoanalytic authors.

The early clinical papers deal with a gallery of maladaptations calculated to be the despair of most therapists—the need for punishment, moral masochism, character types bent on self destruction and criminality. Alexander's continuing interest in criminal behavior culminated in his re-evaluation of our entire concept of justice for the criminal, a re-evaluation based upon the notion that punishment remains ineffective because the criminal needs it as a salve for his pathological conscience, and then, having suffered, he feels able to justify new crimes. Alexander's distinctions between guilt and inferiority feelings, between structural and instinctual conflicts, and his delineation of the neurotic character contain insights of lasting value to both the clinician and the personality theorist.

One may follow the metamorphosis of Alexander's thinking on instinct theory in this volume, from his early doubts toward a tentative resolution of this still vexing problem. In place of a dual instinct theory (sexual and aggressive drives) he offers a view based upon the degree of organization of impulses: impulses vary from those discharged in an isolated fashion for their own sake (sexual) to those discharged in the service of the organism as a whole as constituent parts of a complex, organized goal structure (non-sexual). The assumption is made of a principle of surplus energy—energy that may be released in all manner of erotic activity, including play, exploration and creativity. The role of aggression is subordinated, since aggressive impulses may be discharged sexually (as in sadism) or non-sexually, in the service of self-preservation, and therefore aggression may be treated in the same fashion as any other type of discharge. Thus revised, instinct theory



FRANZ ALEXANDER

appears consonant with communication theory, the law of entropy and, ultimately, with general systems theory.

On the level of culture, the author contends that adventure, change, creativity and individuality are the results of the dynamic use of surplus energy, in contrast with the need for security, which is bound up with those energies required for survival and self preservation. He feels that man's greatest achievements accrue from the way leisure time is used, and he is dismayed by our current preoccupation with security, by our apparent loss of identity and by our other-directedness.

Alexander's flexibility, daring and optimism are nowhere more evident than in his innovations in the area of treatment and, contingent upon these, in his proposals for psychoanalytic education and training. Nor is any of his work more controversial. His restlessness with protracted treatment and with the therapeutic ineffectiveness of abreaction and intellectual insight in themselves was accompanied by his growing awareness of the influence of all factors, cultural as well as biological, upon the uniqueness of the individual child in the immediate family constellation. Alexander's conviction that reliving emotional experiences in treatment was of greater therapeutic importance than the recollection of repressed memories became the central issue in his struggle with orthodoxy. In order to attain this 'corrective emotional experience' Alexander was willing to

manipulate the transference, and even the environment, consciously and actively, to create that atmosphere most effective in undoing the results of the patient's early pathogenic setting. He firmly regarded the therapist as a real person in a real interpersonal involvement, rather than as a 'blank screen', and his educative philosophy eschewed technique in favor of research and psychodynamic understanding. Some observers feel that these changes have blurred the distinction between psychoanalysis and psychotherapy, a distinction without meaning to Alexander so long as any treatment is based upon sound psychodynamic principles. His ideas in this area richly deserve the clinician's attention, but they obviously cannot be detailed here.

Before turning to a somewhat more general evaluative appraisal, I would comment briefly on two papers which are outstanding, for quite different reasons. "Two Forms of Regression and Their Therapeutic Implications" (1956) is notably brilliant, not only for its masterful clarification of the manifold relations between pregenital and oedipal phases of development, but also because it provides a very excellent illustration of regression as a defense. The other paper, "A Note on Falstaff" (1933) is Alexander's sole excursion here into literature. The marriage of psychoanalysis and literature has produced some lamentable offspring. Alfred Kazin tersely observed how many psychoanalysts wanted to be writers and how many writers have tried to become analysts. He found it difficult to say which group had less to contribute to literature, since both wrote badly and reduced art to formulas in the process. But Alexander's little paper is a model of restraint and illumination, and can serve as an example to those with literary aspirations.

THE volume's title is misleading, since this collection hardly represents the scope of psychoanalysis. The reader will find nothing here, for example, about the major developments of the past two decades in ego psychology. Names like Kris and Erikson are given only the briefest mention, and Hartmann, Rapaport and Lowenstein are conspicuously absent. Alexander does not disregard the ego, but he seems almost exclusively concerned with synthesis and integration, its highest order functions. Again,



in his optimistic framework, he tends to bypass the scutwork of the ego in favor of a grand end-product, a tendency conceptually related to his underemphasis on instinctual features in general and aggression in particular. It is highly consistent with this orientation that his methods of treatment have become highly interpersonal in nature at the expense of being intrapsychic, stressing the present rather than the past. This relative neglect of the past is tangentially evident even in his sympathetic critique of existentialism, which he values as a revolt against dehumanizing influences in society. It seems unimportant to him that existential theory revolves almost *solely* around the concept of identity, or that existential therapy is ahistorical. Despite these things, the transference retains paramount significance in his treatment. But in his method it must be often abridged, and, since it is certainly actively manipulated, one wonders how fully it is allowed to develop. Indeed, Alexander's most extreme treatment modifications may be paraphrased as the encouragement of acting in the transference in place of remembering.

These features have made him *cogitatio*, if not *persona, non grata* in certain psychoanalytic circles, and while it may be difficult for the outsider to know just who still acknowledges whom as a sibling in the psychoanalytic fraternity, there can be no doubt they all claim the same father. Freud thought enough of aggression, for example, to make it central in his scheme of things, and Alexander certainly does not share his later pessimism. Perhaps we may understand Alexander's ability to diverge boldly from orthodoxy if we compare his presidential address to the American Psychoanalytic Association (1938) with Rangell's, given a generation later (1961). For Alexander, psychoanalysis had already 'come of age' in 1938, whereas Rangell is just now able to grant it its majority and feels that its identity is still in jeopardy. Long ago Alexander could get on with things that finally seem possible to his colleagues.

The thorny issue of psychoanalysis versus psychotherapy has been an enormous drain on the talents of a productive group. If it remains contentious, it

should be settled by a controlled study in which divergent therapeutic approaches can be compared in terms of their efficacy, for the issue will not be resolved emotionally. Alexander's current researches into the therapy process are an admirable step in this direction; however, it must be noted that he appears to have already answered for himself the questions he poses in this research. Psychoanalysis faces the same dilemma that confronts the virgin—how to remain pure in a world of compromise. Both may be notably enriched by allowing *certain* compromises, as psychoanalysts surely ought to know, since purity, after all, is a state of mind.

Alexander appears as the champion of the unorthodox, but, one may ask, what orthodoxies remain? In theory, the ego-psychologists are unorthodox compared with the orthodoxy of forty years ago, and they have their own brand of optimism, albeit cautious. In practice, it is hard to believe that most present day psychoanalysts still regard themselves as blank screens, that they do not take quite scrupulous account of themselves as real people in therapy, or that they do not modify treatment when it is appropriate to do so. And Freud, of course, was the most unorthodox of all. If he were not, he could not have fashioned his theory to begin with, nor could he have altered it so conspicuously during the course of his lifetime. Although I have no wish to minimize what may appear to some as fundamental differences, it is conceivable that Alexander may be fighting a few straw men.

The foregoing should not detract from Alexander's great contributions. Apart from his value as an imaginative and courageous maverick, from which any science can profit, he remains a humanist and a scientist in the best sense of these words, and he bears a more than slight resemblance to a modern Renaissance man. Any psychologist, indeed any person of intelligence and curiosity, will be rewarded by his journey through this volume, and he will be in the company of an original mind and a vital personality.



## Personality, With Care

Ralph Mason Dreger

***Fundamentals of Personality: A Functional Psychology of Personality.*** Philadelphia: J. B. Lippincott, 1962. Pp. xi + 388. \$6.95.

Reviewed by ALFRED B. HEILBRUN, JR.

The author, Ralph Mason Dreger, received his PhD from the University of Southern California and presently is Professor at Jacksonville University and Director of the Duval Country Child Guidance and Speech Correction Clinic. His past includes more than a decade as a Methodist Minister and a period of teaching at Florida State University. Alfred B. Heilbrun, the reviewer, received his PhD from the State University of Iowa, has spent two years on the staff of Veterans Administration Neuropsychiatric Hospitals, and has taught for six years in the Department of Psychology of The State University of Iowa where he is now an associate professor. He is an ABEPP diplomate in clinical psychology and has authored or co-authored about forty research or review articles on personality measurement and functioning.

IT is difficult to imagine a more arduous task than authoring a basic text on personality when you consider the mountain of research evidence, theory, and nonsense which has accumulated to this date. Dreger has approached his assignment in an erudite and scientifically conservative manner and has added a sufficient number of naturalistic and clinical observations to enhance readability for the beginning student. To begin with a summary statement, I was favorably impressed by this book, and the reader would do well to temper the coming and inevitable criticisms with this fact.

The general plan of the book is a commendable one in light of its intent as a basic text; the reader is given a short-course on the principles of the scientific method in psychology followed



by a survey of the more visible definitions and theories of personality which have been proposed. Of no less importance, the author gives considerable attention to the question of whether there is sufficient evidence of behavioral consistency in humans to warrant a science of personality (he attributes a narrow verdict to consistency).

The core of the book resides in Dreger's presentation of a schema for conceptualizing personality, a schema which is better described as 'fresh' rather than 'new.' Human biological and psychological functions are classed into four areas: drive or tension, control, societally-induced control, and role. The author acknowledges a psychoanalytic framework to his formulation with the time-honored trinity of id, ego, and superego functions corresponding to the first three classes and with social role functions added. The reviewer wonders why Dreger felt it necessary to develop his construct of personality in a Freudian mold, since his tension-control model has heuristic value of its own without a great deal of tribute due to psychoanalysis for inception or formal congruence.

The chapters on the control functions, which include perception, motility, "binding" tension, judgment, synthesizing, and conscience, sometimes sag under the weight of demonstrating regulatory significance to such a broad spectrum of habit systems. However, the research literature is liberally consulted in presenting his point-of-view, and after a while the thesis becomes a believable one. It should be noted in this regard that Dreger conducts himself impeccably throughout the book by continually reminding the reader that construing personality as he has represents nothing more than a way of making sense out of the fact that simple and complex behavioral consistency occurs at all. No purveyor of unobservable demons, he!

The increasing importance of role theory in understanding interpersonal behavior is recognized and role functions are given equal status to drive and control functions in Dreger's description of personality. A rather limited space is allotted to the contribution of social, cultural and situational phe-

nomena in shaping role behavior, but the author's propensity for illustrating his points by anecdote, excerpts from literary works, and the like usually serves to clarify what might otherwise be unclear.

THE REMAINDER of the book serves two purposes. For one, the phenomena subsumed under the four personality functions are considered in terms of two additional dimensions which cut across each. Consciousness and unconsciousness as a dimension are dealt with in a more behavioristic than Freudian manner, although considerable attention is paid to the clinical implications of varying accessibility of our own behavior to awareness. The other cross-classification of personality is in terms of the central versus peripheral (nonobservable versus observable) character of the personality functions.

The final section of the book is employed to present the reader with an overview of adult personality. What are the 'units' of personality? Can personality be meaningfully ordered from more specific traits to the most general styles of living? Probably because of his training at the University of Southern California, the image of J. P. Guilford and the method of factor analysis loom large in the delineation of traits presented as basic to human behavior. By my predilections, this represents an overemphasis on a still controversial method of analysis, yet criticism must be tempered by Dreger's admission that the dividends of factor analytic precision are still in the future. Giving it a place of importance in the book is an act of faith.

The virtues of this book reside in a readable style, a logical organization of materials, and the scientific conservatism of the author which yet does not cripple the expression of viewpoints. Its outstanding weakness is inherent in the subject matter chosen. Personality is a nebulous construct and does not lend itself readily to either scientific or literary examination. Dreger has done a good job in pushing water uphill.



## Psyche and Disabled Soma

James F. Garrett and Edna S. Levine  
(Eds.)

*Psychological Practices with the Physically Disabled.* New York: Columbia University Press, 1962. Pp. xi + 463. \$8.75.

Reviewed by W. E. FORDYCE

James F. Garrett and Edna S. Levine, who collaborate here as editors have collaborated earlier as authors to produce *Psychological Practices in Rehabilitation* (in press). Both of them did their graduate work at NYU and both went on afterwards to become leaders in the field of rehabilitation. Garrett as assistant director of the Office of Vocational Rehabilitation of the Department of Health, Education and Welfare, Levine as associate research scientist at the New York State Psychiatric Institute. The reviewer, W. E. Fordyce, is a doctoral product of the University of Washington, who, after a number of years with the VA programs in the states of Minnesota and Washington, has returned to Seattle where in recent years he has concerned himself with problems of rehabilitation and where he is now in the Department of Physical Medicine and Rehabilitation of the University of Washington Medical School. He has worked extensively with the blind as well as with other kinds of disabled individuals.

THE EDITORS of this book are a pair of senior instigators to the development of psychology's role in rehabilitation. Having helped psychology find a place on the rehabilitation team, they seek now to help future members know what to do. They succeed.

This book is a symposium of chapters setting forth many of the important kinds of information about "... some of the special 'disability' influences and problems in terms of (1) medical-physical aspects; (2) psychological implications both in regard to the indi-



vidual, as well as to family, community and sociocultural and vocational milieu; (3) special considerations in psychological appraisal, management, and in rehabilitation; and (4) suggestions for research as well as for improvement in psychological management and rehabilitation" (p. x). Each chapter is authored by a psychologist experienced in dealing with the selected kinds of disabilities discussed.

There is here a wealth of information for readers newly arrived at a depth consideration of non-medical aspects of disability and rehabilitation, but it is not, basically, a book of ideas, theories, concepts. New journeymen in all the rehabilitation specialties, and, perhaps, grandfather-clause rehabilitation professionals will find concrete information about disability influences and problems of management. Psychologists and their graduate students will find detailed discussions of methods of assessment and of problems specific to the wide range of disabilities considered.

This book reflects the inconsistencies and uncertainties of much of present day clinical psychology, in or out of rehabilitation. Despite a sequential outline approach followed well in nearly every chapter, the loose reins of the editors and the scatter in self-concepts of chapter authors is evident. Nearly every author has something to say about selection of psychological tests for assessment of patients with the particular disabilities they discuss. Seidenfeld in his chapter on arthritis and rheumatism—certainly one of the book's most informative and clearly written chapters—calls attention to the dubious validity of the projectives. He calls upon Eysenck and others for documentary support of his position. Several of the other authors hold somewhat less doubt. An extreme seems to have been approached in this regard by Madan who says (p. 274), "Since the emotional life of the patient (he is dealing with facial disfigurement) is all important, there should be a concentration on the projective techniques." This book may burn in Minnesota!

Cobb, in a perceptively written chapter, focuses on counseling problems relating to cancer patients and their families. Aside from this chapter devoted specifically to counseling or psy-

chotherapeutic problems, most of the authors address themselves on a limited basis to comments and recommendations on treatment procedures. Those who comment to such matters, with an exception to be noted in a moment, seem quick to recommend psychotherapy. Had they shared Seidenfeld's familiarity with the publications of Eysenck they might have made different recommendations. Shontz in his excellent discussion of problems of severe chronic illness points out the limitations imposed by resources of time and staff on the recommendation of psychotherapy. He points to the virtues of working through other team members such as the occupational therapist, etc., in striving for therapeutic objectives.

CENTRAL to all of this is the question of the role of psychologists in rehabilitation programs. There are excellent comments about working with and through fellow rehabilitation team members. Several authors call attention to the important contribution the psychologist can make in helping to orient other team members to psychological aspects of patient behavior. Concern is shown frequently for helping other team members to understand their impact on the patient. Given all of this, the collective effect of many of the chapters gives this reviewer a disappointing perception of psychology's clinical role in rehabilitation as that of collector of test-type facts and inferences. There seems to be too little concern with what might be paraphrased as a kind of middleman role somewhere among the complexities of behavior of patients and their families, the complexities of skills and interpersonal interactions of the rehabilitation team, and complex forces in the community.

Rehabilitation, in the eyes of the present reviewer, is best understood as a social process involving the inter-play of a wide range of forces and of people: patient, family, and staff. The heterogeneity of kinds of people, kinds of training, and professional self-concepts of rehabilitation team members is ever apparent in the process. These forces are immediate to the patient's rehabilitation. At a more general, and though frequently also specific, level for the

patient, is the inter-play of extramural forces such as agencies sponsoring patient services, employer resistance to hiring the disabled, and the generalized and common tendency of people to over-react to patients—protect or sometimes to over-protect, sometimes to upset these patients. These forces too are part of psychological practices with the disabled. As Shontz points out, "the psychologist rarely has time available to do (treatment practices) . . ." (p. 436). Putting aside the issue of the efficacy of psychotherapy, it can be argued that a reason the psychologist has so little time is because of the need for his skills and services in the interplay of the forces noted. If this book is a primer on psychology's role in rehabilitation (which could have been a well deserved though not listed sub-title) more attention ought to have been given to these considerations.

The present reviewer's plea is not for consideration in a book such as this of any wide sampling of these and similar issues. The plea is for more preparation of the graduate student in psychology to anticipate better the basically social-economic character of the rehabilitation process.



*Newton, in his Principia, deduces from the observed motions of the heavenly bodies the fact that they attract one another according to a definite law . . . In his Principia, he confined himself to the demonstration and development of this great step in the science of the mutual actions of bodies. He says nothing about the means by which bodies are made to gravitate towards each other . . . With that wise moderation which is characteristic of his investigations, he distinguished such speculations from what he had established by observation and demonstration, and excluded from his Principia all mention of the cause of gravitation, reserving his thoughts on this subject for . . . the Opticks.*

—J. CLERK MAXWELL







#### CP AND RULES OF ENGLISH USAGE

**I**N 1775, Samuel Johnson, in commenting on his own labors on his dictionary, had this to say:

"It is the fate of those who toil at the lower employments of life, to be rather driven by the fear of evil, than attracted by the prospect of good . . .

Among these unhappy mortals is the writer of dictionaries; whom mankind have considered, not as the pupil, but the slave of science, the pioneer of literature, doomed only to remove rubbish and clear obstructions from the paths through which learning and genius press forward to conquest and glory, without bestowing a smile on the humble drudge that facilitates their progress. Every other author may aspire to praise; the lexicographer can only hope to escape reproach, and even this negative recompense has been yet granted to very few."

No editor of an APA journal feels, in his saner moments, quite so put upon as did Dr. Johnson and none would dare, before an audience of psychologists, express so nakedly his feelings about his function. But editors do worry and fret about the general usefulness of the editorial role, about ways both graceful and satisfying to serve as "slaves to science" and, at a lower level of anxiety, about ways to handle the multitude of niggling little problems that arise when they find themselves making suggestions, often on the basis of mere intuition, concerning the cherished sentences and paragraphs of a writer. In the face of such concerns, editors can be expected to do a variety of things—some sensible, some purely defensive, all perhaps understandable—to bring their efforts in line with their standards and their aspirations. One reaction is to seek for rules. Rules lend

a sense, if often a false one, of neatness, safety, propriety, legality. Rules that are explicit and widely accepted also help contain an editor's arbitrariness, for an author can cite such rules to an editor as easily as the other way around, and everyone gains protection from unbridled idiosyncrasy. Editorial rules, then, have both clinical and communicative advantages. But there is no truly authoritative handbook for editors, no general code and perhaps not even a body of workable common law. And even if there were, no editor would be completely happy about any given set of regulations. Each editor would like to formulate his own rules, for he has the feeling, justified or not, that his problems with his authors are unduplicated in the history of the written sentence. And he may be right; but right or not he still wants to make his own rules, for he must cultivate the double impression that his job is important and that he knows what he is doing. Perhaps, too, he has the forlorn hope that consistency cast into rules will reduce daily dissonances even with respect to nit-picking editorial practices. When an editor forms his rules, they may be quite explicit or may remain only semi-articulated; they may become exceptionless and rigid or they may be such as to give only general guidance; they may be very extensive or relatively sketchy. But rules do happen.

**I**T SEEMS probable that any journal editor will, in time, evolve a set of editorial rules for himself. These rules may become so elaborate, so rigid and so fusty that every edited writer may end up saying exactly the same thing in the same style about whatever subject is under consideration. When that day comes, of course, the given journal will die, or will discover that it has

long been dead. It will have no articles, no contributors, no readers. Only rules. Rules can kill. But a journal, including *CP*, still needs rules—sensible rules sensibly applied. And any journal, including *CP*, does have rules. *CP* does not have any highly codified or detailed rules, but it has some. For example, Professor Boring stated and adhered to the rule for *CP*<sub>1</sub> that reviewers should not diagnose or characterize or make inferences about the personal attributes or motives of an author, but must deal instead with what the author says. *CP*<sub>2</sub> happily adheres to this rule, and so do most of its reviewers. There are other rules presently in force, but in line with the necessity, fully explained earlier, that every editor work out his own rules. *CP*<sub>2</sub> has begun the job and has formulated at least one of its own. It is one of less significance than Boring's rule against *ad hominem*s, but it is a rule that can be of some general utility in dealing with a number of problems. The rule says:

*Writers and editors should concern themselves with questions of proper English usage; readers should not have to.*

The last clause in the rule carries its meat: it means, of course, that the reader should be free to follow the writer's meaning, and in his seeking for meaning the reader should not be jarred or irritated by usages that either obscure meaning or grate upon his sensibilities. And if both the reader and writer have a sense of the sound and rhythm of words, the reader should be allowed the uninterrupted, nonirritated enjoyment of flow and cadences and patterns. When a reader encounters a jarring usage he loses, at least momentarily, both the train of thought and the pleasure of following it. So, you writer and you editor, protect the reader from any unnecessary and unintentional jars. Do not, for example, use data as a singular noun. The word may soon become singular, through the repeated use of it as such by psychologists, but most readers still prefer data to be plural. Do not split infinitives, unless there is no discoverable way to express your meaning without the split, or unless as a matter of vital principle you need to



insult schoolmarm rules; many people have learned, and painfully, to avoid splitting infinitives and are disturbed when anyone else gets away with it. Do not string out adjectival nouns. Not only is this a way to create puzzles for your reader, but it's a way to make him come down with an interrupting worry about why you are not able to express the same meaning a little more clearly, a little more conventionally, by giving in to the established expectation that adjectives modify nouns. See that pronouns at the front of clauses or sentences have clearly defined referents; no reader should have to search through a preceding paragraph to find the meaning of a free-floating "this." However great the clarity achieved, do not use the same word three times in the same sentence; the reader will wonder, perhaps with empathetic embarrassment, about the size of your vocabulary. And so on, from the stuffy beginning to the stuffier end of books on English usage.

No rule or set of rules, however meticulously followed, will make poor writing into good writing. But whatever is the nature of good writing—and this is a matter lying far beyond the world of ironclad certainty—it involves containment, control, discipline; linguistic proprieties can be insulted only by those who are licensed by greatness or genius to depart from conventionalities. Expository writing, even for the great and the gifted, is probably more demanding of discipline than is any other kind. Rules are necessary. And they can help, within limits, improve the quality of writing.

The general rule under consideration here may bring a bit of discipline into the writing of sentences; and it may bring explication and justification into the editing of some of them. But it has a built-in weakness. It can be applied only on the basis of the author's or the editor's intuition about the jarring effect any given usage will produce on how many readers. And the usefulness of the rule still depends upon the rigor of its application. If one should go so far as to say that any trauma of any magnitude to the sensibilities of any reader is to be avoided, he is surely on the road not only to great dullness but

to spineless other-directedness. If the rule is not applied at all, the way is open for that syntactical anarchy that makes good communication impossible. Between the extremes of rigidity and laxity of application, the rule leaves room for an author to find clear expres-

sion and in his own style. He can still find ways to jar the reader if he wants to, but adherence to widely shared expectancies concerning usage can give the reader the good and non-distracting feeling that the writer, in Robert Frost's term, is "moving easy in harness."

## Biology Back in the Saddle?

Paul H. Hoch and Joseph Zubin (Eds.)

*The Future of Psychiatry.* New York: Grune & Stratton, 1962. Pp. xii + 271. \$8.75.

Reviewed by OSCAR A. PARSONS

*The team of Paul H. Hoch and Joseph Zubin, editorially responsible for the present volume, is well known to the readers of CP. The pair has edited fifteen volumes of a series based on events occurring at the meetings of the American Psychopathological Association. The reviewer, Oscar A. Parsons, now plays a triple role involving medical operation and a single one in the Psychology Department at the University of Oklahoma. In the Medical School he is Professor of Medical Psychology, Head of the Division of Behavioral Sciences in the Department of Psychiatry, Neurology and Behavioral Sciences and is Psychologist in Chief at the University Hospital. In the Department of Psychology he is a professor. He has focussed a great deal of his energy since 1959 on developing for medical students and for residents in psychiatry a teaching program in the behavioral sciences. His research interests include perceptual processes and information processing in brain damaged patients and in chronic schizophrenics.*

AT THE 50th annual meeting of the American Psychopathological Association, sixteen investigators and clinicians undertook the difficult task of predicting "things to come" in psychiatry. As in the psychiatric interview, the question "What are your future plans?" evoked varied responses ranging from relatively conservative to expansive and optimistic predictions. Psychologists will

find three aspects of the book of interest: 1) some recent advances in the biological aspects of behavior; 2) the place accorded to psychology in the future of psychiatry; and 3) the manifest divergence of views as to the psychiatrist of the future.

The dominant impression conveyed by this volume is that the future of psychiatry lies in tying behavior to the biology of the organism. While at first glance this may seem to reflect the specific interests of the members of the American Psychopathological Association, there seems little doubt that this is an adequate representation of trends in the field of psychiatry today. Certainly had such a book been written ten years ago it would have had much greater emphasis on possible contributions from psychoanalysis, experimental psychodynamics, and from interpersonal and communicational approaches to behavior. Psychodynamic theories are here tacitly accepted as important but limited. There is a spirit of optimism about unraveling the relationship between mind and brain at a level which will anchor psychodynamic concepts in physiology and neurophysiology.

Apparently by recognizing that prognostications are meaningful only when compared to a base line, most of the authors provide an overview of some recent developments in their field. On the biological side such consideration is given to neurochemistry, neurophysiology, pharmacology, physiology, biochemistry, genetics, maturational and



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general biological factors. Of particular relevance to the interests of many psychologists are papers by Marrazzi, Greenblatt and Rioch. Marrazzi focuses on the import of disruption of homeostasis at the synaptic level in the CNS for understanding psychoses and he imaginatively indicates directions that psychopharmacological research in excitatory and inhibitory processes is likely to follow. A sense of enthusiasm and excitement is conveyed by Greenblatt's citation of a number of areas (e.g. classical and operant conditioning, motivation, emotion, psychotherapy, social behavior) where bridges between biologic aspects and behavior patterns are being constructed. Rioch, in a much more cautious vein, declares that the phenomena within psychiatry are social phenomena and that only occasionally will psychologic data be relevant to psychiatric problems.

Of the three papers which are primarily concerned with psychologic approaches, Rado's most clearly provides a conceptual framework for synthesis of psychodynamics with biological functions. In contrast to Rioch, he sees the choice of psychodynamic theories ultimately being dictated "behavior physiologists." Appel finds psychoanalysis facing many problems and states that it cannot afford to remain tied to "repetition of intellectual formulae." Meehl's indictment of the overgeneralization of the concept of purpose in contemporary psychodynamics is challenging to all who profess allegiance to, and act upon, motivationally based theories of behavior.

A miscellany of other papers includes discussion of the future of epidemiological studies, contributions of psychiatry to education, and social organization of psychiatric services. The tenor of these pages suggests a disenchantment with individual psychotherapy as an answer to the larger problems of mental health and a growing awareness that psychiatry must become attuned to the social matrix and social philosophy within which it operates. A wise and scholarly paper by Nolan D. C. Lewis places the present and future of psychiatry in its historical perspective. His insistence on the need to avoid dogma, develop curiosity and to search

for answers to fundamental questions may well be applied to any discipline.

**W**HAT part does psychology play in the future of psychiatry? The present authors expect contributions from the experimental laboratories in the areas of learning, problem solving, memory and physiological psychology. Special emphasis is accorded studies involving classical and operant conditioning; indeed the presidential address by Gantt is essentially a tribute to Pavlov and his methods. In contrast, social psychology and personality development receive only passing reference. Recent experimental developments in areas such as person perception, impression formation, cognitive dissonance, determinants of yielding and group behavior, attitude change, parent-child interaction, and the like, receive scant attention. There are no papers directly concerned with a current issue of some importance to psychiatrists and psychologists i.e., who should do psychotherapy? However, the impression one gains from several authors is that as knowledge in this area accrues, responsibilities will be assumed by those individuals whose training and background best fit them to cope with certain types of therapeutic problems. Zubin writes "The battle for the possession of psychotherapy will persist only as long as ignorance about the nature of therapy and its efficacy remain".

The conclusion of most authors is that psychiatry's future is bright but there is disagreement about the attributes of the psychiatrist in the Brave New World. Will he be Marrazzi's "practicing clinical experimentalist," the socially oriented general psychiatric practitioner of Gruenberg, the mental health educator described by Funkenstein and Farnsworth, or the computer-guided pharmaco-psychiatrist feared by Greenblatt? While all these predictions will undoubtedly be off the target by an appreciable margin, the stimulus value of such an effort is impressive. Indeed this reviewer could not help but conclude that a volume devoted to the 'future of clinical psychology' would have many provocative similarities and differences.

## Narrow Training for Broad Functioning

Dorothy Mereness and Louis J. Karnosh

*Essentials of Psychiatric Nursing.*  
6th Edition. St. Louis: C. V.  
Mosby, 1962. Pp. 312. \$5.50.

*Reviewed by W. K. RIGBY*

*The first of the authors, Dorothy Mereness, is a widely experienced and broadly educated nurse. The second author, Dr. Karnosh, is a now retired psychiatrist with an equally broad and deep training in his field. W. K. Rigby, the reviewer, obtained his PhD at the University of Pittsburgh and has had a long and productive career, both in service and administrative capacities in the Veterans Administration program. Presently he is Chief of Psychology in the VA area office in St. Louis. In the past he has served as a lecturer at Washington University and presently holds an appointment as Clinical Research Associate in its graduate school. An ABEPP Diplomate, he is now chairman of the board of examiners of the Missouri Psychological Association.*

**T**HIS WORK was written as a textbook for students of nursing "who are having an initial experience with psychiatrically ill patients," and its aim is that of helping "the student to understand and function effectively in the therapeutic role of the psychiatric nurse." The sixth edition, it updates the material in the 1958 edition, shows a considerable change and improvement in format, and some reduction in length. The book has evolved vastly from the original edition by Karnosh and Edith B. Gage, published in 1940.

The material presented is written in a clear, easily understandable style and reflects the long and rich experience of the authors in direct work with patients. There are excellent passages concerning attitudes and roles of the nurse in a psychiatric setting and a great many practical recommendations and suggestions for making the nurse's efforts ef-



fective and productive. Organization of material is in three sections emphasizing the nurse's role and self understanding, understanding of patients, and an overview of some pertinent general aspects of psychiatry. The first section is likely to be the most interesting and perhaps the most useful for student nurses. It includes an excellent chapter, worthy of further emphasis and expansion, on nursing care for the physically ill. A weaker part of the book is the material on personality theory. While the need for brevity no doubt contributes to this result, a good deal of this material is not likely to be particularly meaningful or helpful to student nurses.

A DISAPPOINTING feature is the relatively narrow scope and somewhat too concentrated focus on nursing itself. At the end of each chapter there is a list of supplemental references which for the most part point to work of other psychiatric nurses, and little attention is given to contributions of other groups or to the potentialities of research. The book will indicate to student nurses that the treatment team is made up of a psychiatrist and a nurse; other disciplines are given very superficial attention. And perhaps a too rosy picture of psychiatric supervision is presented. Not all psychiatric institutions are completely staffed by the highly trained and sophisticated personnel which this book may lead students to expect.

As an introductory text, this book has much to commend it, the above limitations notwithstanding. Student nurses will find it readable, enlightening, and helpful in their everyday work. The first section could be reviewed with profit by members of co-professional groups who work with nurses in direct patient care. The many-faceted role of the nurse in a psychiatric setting is continuously evolving and requires continued evaluation and continual efforts toward progress. Advanced courses and in-service training are needed to supplement any beginning text, and frequent revisions of all texts will be necessary. This one has evolved well and, despite some limitations in breadth, is a valuable addition to the literature.

## Research Strategies of a Master

Samuel A. Stouffer

*Social Research to Test Ideas.* New York: Free Press of Glencoe, 1962.  
Pp. ix + 314. \$8.50.

Reviewed by KURT W. BACK

*Samuel A. Stouffer was, at the time of his death in 1960, professor in Harvard's Department of Social Relations and Director of that department's laboratory. The recipient of many honors from his colleagues in sociology and other disciplines, he was perhaps best known to psychologists for his role as principal author of the four volume work American Sociology in World War II, a series including the famous American Soldier (1949-50). Later he produced Communism, Conformity and Civil Liberties (1955). The reviewer, Kurt Back, as reported recently in these pages (CP, 1963), and as will be reported again in the subsequent issue in which CP reviews his 1962 book, Slums Projects and People: Social Psychological Problems of Relocation in Puerto Rico, is Professor of Sociology and Psychiatry at Duke University. He received his PhD in Group Psychology at M.I.T. and there, as well as later at the University of Michigan, he was associated with the Research Center for Group Dynamics. He is now involved in a study of work and retirement. CP will probably be hearing more about that, too.*

THE PAPERS in this book come from more than thirty years of research effort. They were collected by the author as a demonstration of how the social scientist, working with demographic and survey data, can reach demonstrable propositions. Short introductory pieces for the several sections explain the kind of setting and materials which led to a particular mode of analysis. The book is addressed partly to the general reader to show, without exaggerated claims, what social science can do, but it will be even more

valuable for the student, to inspire him to apply the same precision with the same ingenuity. The author died just as the compilation of the papers was completed and the volume was completed as a tribute to Samuel Stouffer by Paul Lazarsfeld, who also provided a discerning introduction presenting Stouffer's like work as practically co-extensive with the rise of empirical social science.

Most of the book is given to examples of empirical methods in different fields of sociology and social psychology. However, the best introduction to the general approach are two programmatic papers, "Some Observations on Study Design," and "Quantitative Methods in the Study of Race Relations." The first is a plea that sociology not become dull journalism, that it not settle for few data and much interpretation presented in an academic jargon. He discusses the ideal design to test a proposition, with data collected before and after the introduction of the critical variable, and having experimental and control groups; and he shows the consequences of deviation from this complete system. In this discussion the moderation of Stouffer's approach becomes clear. He derides the studies based only on "after" data in the experimental condition and that supply the other three cells through "brilliant analysis" which fill pages of social science journals. On the other hand, he realizes that not all research can conform to the canons of experimental design and that, through forethought and patchwork, some valuable conclusions can still be drawn. The second essay describes the application of these standards to research on race relations. Where do we have sufficient information to



make firm statements, where should we collect more, where is basic research needed? He mentions, for instance, that the consequences of Negro migration to the North have never been systematically studied, that the methodology of interviewing may give important clues to understanding the position of the Negro, that controlled laboratory studies of the main variables can feed into the research in natural situations, and that the predictable social change following the Supreme Court decision on school integration gives a natural laboratory of different communities to test different propositions. This program shows concretely what Stouffer means by a painstaking empirical attack on a problem. The first ten chapters of the book show some instances of this approach on limited objectives.

One of the striking features of all these research reports is their rigorous parsimony. Theory is not invoked beyond the relations shown in the data. Neither are statistical techniques used beyond necessity; the main approach is tabular analysis. Both these restraints combine to make for unpretentious and pleasant language. Both these features are marks of self-restraint, so Stouffer shows that he is an able theorist and statistician. As to the latter, some of the later chapters show some innovations which he has made in statistical technique. Characteristically, the most valuable is not his early work with partial correlation and case-study prediction, but his development of a simple scaling technique—the H technique—which permits the use of the maximum amount of data while adhering to the standards of Guttman scaling.

THE STUDIES themselves range over a variety of topics, from migration and fertility to role conflict and relative deprivation. They are not presented for their substantive interest, but to demonstrate similarities and differences in the method of reasoning. The differences depend on the type of material and the purpose, which determine the amount of elaboration of the data. If the use is an immediate decision, arrangement of the weight of the evidence from dif-

ferent sources may be best, as in advising about the possible changes in the point system of discharge after World War II. On the other extreme we have a precise mathematical model of migration, based on twenty years work of refinement and testing the model of intervening opportunities and competing migrants. The fitting of the style of presentation to the purpose at hand is again an example of elegant parsimony.

The common core of the approach is equally striking. One can almost outline the procedure step by step. First is the delineation of a question which can derive from several sources, practical and theoretical (note that the title of the book is *Social Research to Test Ideas*, not to "test theories"). Then comes the ascertaining of specific operational consequences of this idea. Next step is the search of data which can show these consequences. A study may be designed especially for this purpose. But equally important is the use of existing data, and even the study which is just being conducted. After all, the scientist does not stop thinking after he has stated his hypotheses and begun on his research operations (a position which rigorous adherence to the principle of hypothesis-testing may lead us to take), but he may get fresh ideas from the very data which he is collecting. Next comes the evaluation of the results and the refinement of the hypotheses and the search for new data to test this refinement. And there is no final step.

ONE may doubt whether the approach shown here is not too much of a reaction against the common theorizing, the "dull journalism" discussed in the paper on *Some Observations on Study Design*. Stouffer chooses as his motto Shakespeare's dialogue between Glendower and Hotspur: To Glendower's "I can call spirits from the vasty deep", Hotspur replies: "Why, so can I or so can any man; But will they come when you do call for them?" It may be well to remember that neither the theorist nor the empiricist was the victor, but Prince Hal, who could combine many approaches in the pursuit of his goal.

However, strictures of this kind should not obscure the real merit of this book. It does communicate the excitement of data analysis which is the core of social research, but slighted in most textbooks and courses. And it does so by giving the privilege of seeing a master at work. While the student is introduced to the art of reasoning from cross-tabulations, of questioning them and of designing further cross-tabulation and additional data to answer his questions, the expert will find delight in many examples of brilliance in the deceptive simplicity of research method. One example must suffice. It comes from the research memorandum on the family in the depression and is the more striking as it uses only available data. Stouffer reasons that the depression accelerated the loss of social function of the family. What would be objective indicators of this change? Stouffer finds three, which can be defined without relation to attitude measurements: marriage viewed only as legalization of sexual intercourse, called impulsive marriage; marriage across other social groupings, such as religion, nationality and class, called mixed marriage; and pregnancy before marriage, called forced marriage. The rate of mixed marriage can be obtained in some places from the tabulation of data given on marriage certificates; the rate of forced marriage by comparing birth and marriage records, compiling births with seven months of marriage. But how to define impulsiveness? Stouffer reasons cogently for three indicators: marriage outside the bride's and groom's home; marriage by a civil officer; divorce or separation within five years. While it is possible to give reasons why each of these indicators may be due partially to other causes, trends in all three give a reasonable indication of changes in impulsive marriages. Collection of data from Australia, Canada and several of the United States show consistent evidence on this question. This is just one example of ingenuity needed to test one armchair hypothesis.

The emphasis on rigorous empirical research is combined with a high sense of the responsibility of the social scientist and several essays, especially "So-





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By James M. Sawrey and Charles W. Telford  
San Jose State College

This new text presents the development of adaptive behavior within the framework of learning and motivational concepts. The text provides sufficient descriptive material, at the outset, to serve as basic groundwork for understanding the dynamics of behavior. The authors include a systematic presentation of the adjustment processes, both normal and abnormal by means of the concepts of learning and motivation. The text is consistent in its theoretical approach and strongly emphasizes research findings.

##### **READINGS IN THE SOCIAL PSYCHOLOGY OF EDUCATION**

By W. W. Charters, Jr., Washington University and  
N. L. Gage, Stanford University

This unique new text of readings is the first book devoted explicitly to the emerging field of the social psychology of education. It has been sponsored by the Society for the Psychological Study of Social Issues, a division of the American Psychological Association. The Society also selected the editors. The readings included were chosen for theoretical soundness and readability, as examples of the empirically grounded application of social psychology to educational issues. The text may be appropriate for a variety of courses.

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#### Other Recent Texts

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##### **THE CAUSES OF BEHAVIOR: Readings in Child Development and Educational Psychology**

By Judy F. Rosenblith, Massachusetts General Hospital  
and Wesley Allinsmith, University of Cincinnati

This 1962 text has been lauded as "the best collection of readings in psychology published in at least a decade." One of the most notable aspects of this comprehensive text is the recency of articles. Approximately one-half of the selections appeared originally in 1958 or subsequently, some as recently as 1961. Among the selections are readings by such eminent authorities as B. F. Skinner, Robert Sears, Jerome Bruner, David Ausubel, Samuel Kirk and Leona Tyler.

##### **PSYCHOLOGY**

By Allen Calvin, Frank McGuigan, Charles Hanley, Michael Scriven, James Gallagher, and James McConnell

This volume brings together the writings of a highly select group of specialists. The entire book has been carefully and thoroughly edited by Dr. Calvin to maintain a balance and continuity of style and content throughout.

##### **HUMAN DEVELOPMENT IN WESTERN CULTURE**

By Harold Bernard, Oregon State System of Higher  
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The text examines human development from multiple, rather than single (experimental, descriptive, or psychological) approaches. Emphasis is on the converging of psychological, cultural and individual aspects to show the global nature of development. The concept of developmental tasks which considers age, culture, physical, and psychological aspects of growth, provides the avenue by which the multiple approaches are presented.

##### **Student Workbook to Accompany PSYCHOLOGY**

By Paul J. Woods

This study guide is concerned entirely with the task of helping the student to learn and understand the material in the text. The book has two sections for each chapter; (1) an open-ended outline which is to be filled in by the student, (2) self-test multiple-choice questions.



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ciology and the Strategy of Social Science" reflect this concern. It is well to be reminded in a book on methods that commitment to social values goes well with rigorous technique, and Stouffer is a prime example. Besides its usefulness for researchers and students, this book is a fitting memorial to a great man.

## Italy's Delinquents

**Gino Faustini.** With the collaboration of **Maria Teresa Conte** and **Santina Cortellesi**

*Aumento o Trasformazione Nella Delinquenza Minorile?* Roma, Italy: Scuola per la formazione del personale per la rieducazione dei minorenni, 1962. Pp. 174.

Reviewed by **PIER ANGELO ACHILLE**

*Gino Faustini, the principal author, is a Consultant of the Department of Justice of the government of Italy and also a professor in a school for specialized teachers and educators of juvenile delinquents. Neither CP nor its reviewer could get information on his present collaborators. Pier Angelo Achille, the reviewer, is a senior resident at the Institute of Psychology at Catholic University in Milan, but at present holds the position of Assistant en Recherche at the University of Montreal, where he is conducting, under the direction of Father Noel Mailloux, O.P., a two-year study on the methods of treatment and reeducation of juvenile offenders.*

**H**AS juvenile delinquency decreased in Italy or have the patterns of Italian delinquency changed? The authors of the present volume try to reply to this question. From the beginning of the century to the present, statistics suggest a decrease at the rate of 3.5 per cent per year. In the same period the ratio between offenses against property and other offenses has changed, suggesting also a change in pattern. Larceny-

theft remains the most frequent offense but its relative percentage in the total occurrence of offenses is noticeably diminished.

A sharp analysis of all available statistical data is the method adopted by the authors. The Italian Code considers juvenile offenders young people between fourteen and eighteen who commit violations of established law. Offenses are grouped in four major categories of the Criminal Code, i.e. offenses against property, against family and morality, against persons and life, against the State and administration of justice. Statistical figures refer to young offenders recognized by a court to have committed a violation in one of the four areas. In the present volume a juvenile offender is a young person condemned by a court. Major and minor offenses are judged on a triple degree basis. Major or general courts judge major crimes or delicts. Minor or trivial offenses, namely "contraventions" in Italian terminology, are referred to correctional judges. Juvenile courts have been established since 1934 and deal with general matters of judging and rehabilitating juvenile offenders.

The number of youths condemned in major courts has noticeably decreased whereas there has been a marked increase in the occurrence of trivial offenses. This observation would tend to suggest that juvenile delinquents are possibly more inclined to behave in a more generalized antisocial manner than to commit definite major crimes.

**I**N the study of the general decrease in juvenile delinquency, the authors examine the disturbing effects of war and the geographical distribution of juvenile delinquency. It might have been assumed that the war was a disturbing factor on the younger generation of that period. Nevertheless indirect disturbing effects on those children born during the war might also have been expected during a later period. It is impossible to reconstruct a real situation from fragmentary records and inadequate statistics reported during the war, when disorganization was prevalent in civil life and subsequent division of the State made regular administration of justice

impossible. On examining the long term disturbing effects, an interesting fact comes to light; the increased rate of juvenile delinquency apparent in the years following World War I is completely absent in corresponding years following World War II.

On the whole, the study of the geographical distribution of juvenile delinquency confirms the decreasing trends. A general examination of available data shows a sharply defined decrease of delinquency in agricultural and underdeveloped districts in the South and a more regular diminution in industrial and more economically developed districts in the North. More traditional juvenile offenses such as larceny-thefts have diminished everywhere. Crimes against persons and life, especially in connection with traffic law violations, have increased in more developed regions. There is no correspondence between geographical distribution of general delinquency and juvenile delinquency.

General legal classifications on which reported statistics are based do not permit of reliable criminological conclusions. A large variety of motivations can be implied under the same legal classifications. The perusal of the volume brings to mind some intriguing questions, beyond the scope of statistical and legal bounds. How can the steady decrease in the incidence of juvenile delinquency records be explained? What influences have contributed to the limitation of the juvenile delinquency rate in recent years? Despite the absence of major manifestations of delinquency in young people, is it not possible that the war has had disturbing effects on other less observable aspects of behavior? What economic, sociological and general cultural factors underlie the particular distribution and patterns in juvenile delinquency? These and other problems require a complementary explanation from related disciplines interested in juvenile delinquency, namely psychology and sociology. In a comprehensive presentation the reviewed volume puts at the disposal of students interested in Italian juvenile delinquency a considerable sum of data, hitherto unavailable.



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# Indians and Ink Blots

Gardner Lindzey

*Projective Techniques and Cross-Cultural Research.* New York: Appleton-Century-Crofts, 1962. Pp. ix + 339. \$6.00.

Reviewed by BERT KAPLAN

The author, Gardner Lindzey, has done and continues to do a number of things at least as important as his consulting for CP. He is the sort of person to whom one refers books that fall between or across the usual substantive categories. A product of Harvard's Department of Social Relations, he is well known as a social psychologist, as a personality theorist and as a professor of psychology at the University of Minnesota. The present book, one of a number he has brought to the light of day, is an outcome of a fellowship year at the Center for Advanced Studies in the Behavioral Sciences. The reviewer is the Bert Kaplan who edited the recent book *Studying Personality Cross-Culturally* (CP, June 1962, 7, 210-212). He is likewise the Bert Kaplan who received his PhD at Harvard, who has been in the psychology department at the University of Kansas since 1953, and who has done a number of valuable things about his interest in cross cultural affairs, including a Microcard publication, *Primary Records in Culture and Personality*. In the same area are coming other publications to be based on a just completed three year study of mental illness among the Navaho. On the way also is a book, *The Inner World of Mental Illness*, a volume presenting first person accounts of the experience of mental illness. In the fall he is joining Rice University as Chairman of its new Psychology Department.



GARDNER LINDZEY AND FRIENDS

vital link in the functioning of socio-cultural systems. It has thus been more than a passing fad that a considerable body of research has been concerned with describing personality processes in a variety of literate and nonliterate societies in every part of the world. The question of how such studies are to be conducted constitutes one of the most interesting challenges that faces psychology today. Projective techniques have played perhaps the major role to date, a fact which because of continuing doubts about their validity in our own culture and even greater doubts about their applicability in radically different cultural settings, has led to a certain amount of concern. It was perhaps a consideration of this kind that led the Committee on Social Behavior of the Social Science Research Council to invite Gardner Lindzey to undertake a critical survey of the extensive dependence on these techniques in cross-cultural research.

Dr. Lindzey has provided what in many respects is a classical stocktaking; the only limitation to my enthusiasm is that his critical comments have been confined mostly to methodological issues. With wisdom, thoroughness and exemplary fairness he has reviewed the bulk of the significant work in the field, given an account of methods and findings and weighed values and faults. This is undoubtedly the best methodological review that these studies have received, and it is an invaluable resource for anyone planning cross-cultural research of any kind.

In preparing the groundwork for his critique, Dr. Lindzey has, incidentally, produced one of our most valuable introductions to the projective techniques themselves. Covering a span of 150 pages, his four preliminary chapters constitute both an excellent introduction to the study of projective techniques and a very sophisticated treatment of projective technique theory for advanced workers. This book then is destined to join the small handful of works that contribute significantly to the clarification of projective theory and practice.

THE MAIN focus of the book, however, is the critique of the cross-cultural use of projective techniques. In summarizing what he finds wrong with this body of research, Dr. Lindzey describes the following "modal flaws": 1) the doubtful independence of many of the personality inferences; 2) the lack of objectivity in relating projective technique to ethnological sources; 3) a peculiar failure of anthropologists to provide a full description of the circumstances under which the tests were administered; 4) a failure to explore the possible contribution of nonpersonality factors; 5) an apparent unawareness of the examiner's influence on the test performance; 6) a failure to select well-matched samples where cultural groups are being compared; 7) the tendency to take group averages and treat them as descriptive of the group as a whole; and perhaps most damaging of all, 10) the fact that there has apparently been little accumulation of sophistication and wisdom in carrying out such studies. Taken together these findings constitute a

IT HAS become abundantly clear to most social scientists in recent years that personality processes constitute a



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Appleton-Century-Crofts is pleased to announce that the first Century Psychology Series Award for a distinguished manuscript in psychology has been presented to

**DR. BERNARD RIMLAND of San Diego, California**

for his study of Kanner's syndrome of apparent autism. In this unusually significant monograph, which will be published probably in the summer of 1963, Dr. Rimland reviews early infantile autism, proposes a theory which accounts for early infantile autism as the consequence of a specific cognitive defect, and finally enlarges the theory by showing that the assumed brain damage is consistent with a broad range of findings in the fields of normal and abnormal personality and intelligence.

Dr. Rimland is the Director of the Personnel Measurement Research Department of the U. S. Naval Personnel Research Activity in San Diego.

Appleton-Century-Crofts, publisher of The Century Psychology Series, edited by Richard M. Elliott, Kenneth MacCorquodale, and Gardner Lindzey, offers an annual prize of \$1500 in cash and a favorable contract for publication to the author whose manuscript provides a significant original contribution to the field of psychology. The deadline for submitting entries for the 1963 award is October 1. Questions concerning the award or manuscript requirements should be sent directly to the publisher.

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damaging criticism of the whole body of research, a damage which is not mitigated by Lindzey's corresponding list of contributions.

As someone identified with cross-cultural personality research I would like to be able to controvert Dr. Lindzey's critique and defend the legitimacy of what has been done. In all honesty, however, I must say that his points seem to me to be essentially just and correct. If his demands are occasionally contradictory, as when he seems to ask for both independence from contamination by *and* full consideration of relevant ethnographic materials, or occasionally somewhat rigid as when he asks people to produce materials or analyses that are irrelevant to their particular problems, there can be little doubt that his charges are substantially correct.

In mitigation it can be claimed that some of these problems are inherent in the use of projective techniques anywhere, something Dr. Lindzey acknowledges, and that other problems have to do with the fact that cross-cultural personality research has been done largely by anthropologists who are relatively untrained in the more rigorous criteria of psychological research and, more significantly, are professionally unequipped to deal with the extremely difficult problems encountered in this kind of research. We are still awaiting the serious entry of the psychologist into this field and while Dr. Lindzey's critique correctly represents the psychologist's understanding of all that is wrong with what has been going on, it also represents our failure thus far to make any significant positive contribution to the solution of problems. We can expect the anthropologist to do more careful research work, but it is not reasonable that we expect him to solve our basic problems for us.



*We do not worry about being respected in the towns through which we pass. But if we are going to remain in one for a certain time, we do worry. How long does this have to be?*

—PASCAL



## Some Psyche for the Soma

A. H. Chapman

*Management of Emotional Disorders: A Manual for Physicians.* Philadelphia: J. B. Lippincott, 1962. Pp. vii + 259. \$8.50.

Reviewed by DUANE DENNEY

*The author, A. H. Chapman, is a physician who obtained both his undergraduate and medical degrees at Yale. He had psychiatric training at Washington University School of Medicine in St. Louis, and has published clinical articles on a wide variety of psychiatric subjects. Presently, among other things, he is Assistant Professor of Psychiatry at the University of Kansas School of Medicine. The reviewer, Duane Denney, is also a psychiatrist, now functioning as an instructor at the University of Oregon Medical School. His clinical interests have involved him primarily with psychiatric consultation services to the general hospital. His intellectual and research interests range from electrophysiology to social psychology. At the moment, in addition to teaching, he describes himself as engaging "in various pokings and probings of the thalamus and cortex of felix domesticus in an attempt to identify some of the mechanisms involved in classical conditioning."*

DR. CHAPMAN's book should prove useful to all practicing physicians and to those behavioral scientists and other allied professionals who are interested in the role of psychological factors in illness. The author obviously found it necessary to limit the depth of discussion of many specific topics, but succeeded in his principal aim of helping the "...busy physician who is treating many patients," and to "stick to concrete suggestions and specific advice."

The book, in keeping with its subject, is extremely broad in its coverage. The

first and largest portion of the discussion is devoted to the treatment of the non-psychotic disorders as defined in the American Psychiatric Association's *Standard Nomenclature of Diseases*. Little attempt is made to discuss psychodynamic considerations except in those few areas where the physician is advised to attempt some exploratory psychotherapy. Even here the author wisely avoids psychiatric jargon and needlessly complicated formulations.

His discussion of the levels of dysfunction in psychosomatic illness is a good example of a simple model which not only helps the physician and the patient understand current thinking about these disorders, but almost automatically suggests a rational and often simple treatment program. For instance, peptic ulcer may be considered a disease which can be understood as a dysfunction at any one or all of six different levels: (1) the breakdown of the gastric mucosa in the presence of chronic hyperemia, hypermotility, and hypersecretion, (2) increased activity in the vagus nerve which produces changes in the gastric mucosa, (3) a dysfunction produced at the level of the brain stem where various autonomic functions are coordinated, (4) a dysfunction at the level of the symbolic and integrative activities of the central nervous system, (5) difficulties at the level of the interpersonal transactions between the patient and important persons in his life, or (6) distressing personality problems in the lives of important other persons in his interpersonal environment.

The last part of the book represents an attempt to help the physician with the enormous number of problems



which inevitably come to his attention but do not fit any particular psychiatric diagnostic category. Such problems include marital discord, sexual incompatibility, childhood and adolescent behavior problems, and difficulties inherent in aging. In this portion of the book the author has inserted also some comments on the types of psychiatric orientation usually encountered among psychiatric consultants, and some comments on interviewing technique. It is inevitable, because of the range of this discussion, that the book loses some of the succinct and clearly utilitarian style that marks the initial chapters, but the author's recommendations remain plausible and practical. For example, it is candidly suggested that, "...inadequate spankings are worse than none at all... An effective spanking should be administered on the bare buttocks with a short hard object...such as a hair brush... The child can be turned over one knee while the parent's other leg is used to clamp the child's struggling legs down... The spanking should be given with an attitude of firm execution of justice, with perhaps a touch of justified indignation."

One deficiency in the book concerns the discussion of the role of allied professionals (clinical psychologists, social workers, public health nurses, physical therapists, vocational rehabilitation counselors, legal aid advisors, and many others). They are mentioned only briefly as helpful in the rehabilitation of the mentally ill hospitalized patient. There is a considerable body of evidence to indicate that these groups can and must play a large role in the treatment of non-hospitalized patient if the goals of the Joint Commission on Mental Illness and Health are to be realized. It is time for the practitioner who is concerned for a large segment of the psychiatrically disabled population to begin working with these professional groups and learning from them.

Certain portions of the book may appear condescending to a number of physician readers. Examples of points which are self-evident to most physicians include statements such as "the deeper the depression, the more probable is a suicide attempt," "it is better to say that the 'patient has a problem

with alcohol' than to argue with him about whether or not he is an alcoholic," or that "when confronted with an organic brain disorder of unclear etiology, the physician should explore the possibility of exposure to medications, patent medical remedies, household cleaning substances, heavy metals, industrial fumes, etc."

Since the book will undoubtedly be read by physicians, it will be of interest to those psychologists and other behavioral scientists who are interested in the ways that emphasis on psychosocial factors in illness and health are being disseminated throughout general medicine.

## Many Gallic Elephants

Serge Moscovici

*La Psychanalyse, Son Image et Son Public: Étude Sur la Représentation Sociale de la Psychanalyse.* Paris, Presses Universitaires de France. 1961. Pp. xi + 650. 20 NF.

Reviewed by BOHDAN ZAWADZKI

The author, Serge Moscovici, "Chargé des Recherches au Centre Nationale de Recherche Scientifique," is a French social psychologist who since 1953 has been publishing in the *Bulletin de Psychologie* and *L'Année Psychologique* important articles on methodology of research in public opinion and attitude scales. The reviewer, Bohdan Zawadzki, is associate professor of Psychology at City College of New York, and was introduced in the October, 1961 issue of CP, as a man widely traveled and both broadly and deeply educated.

THIS book is good news for those concerned with the fate of European psychology. It testifies eloquently to the vigorous revitalization of social psychology in France.

This new volume in the *Bibliothèque de Psychanalyse et de Psychologie Clinique*, edited by Daniel Lagache,

professor at the Sorbonne, has an ambitious purpose: to open a new area of research by applying the concepts and methods of empirical social psychology to the problems of sociology of knowledge.

The central problem of the study is: "how does a science become transformed into its public image?"

To answer this question the author and his staff, after a pilot study, have conducted a public opinion poll in which 2265 persons answered at least fourteen questions each. The respondents constituted a representative sample of the Parisian population and several specially selected samples (professionals, university students, pupils of vocational schools, middle class people, and workers) stratified according to suitable criteria.

The questions asked aimed at disclosing 1. the degree of acquaintance with psychoanalysis and 2. the variety of evaluations with regard to several aspects of psychoanalysis. They included questions such as: "According to you, what is psychoanalysis?", "What does one tell his analyst?", "Which practice does psychoanalysis resemble most: conversation, confession, suggestion, narcoanalysis or hypnosis?", "Can analysis change personality, for good or for bad?", "How effective is psychoanalysis?", "Would you undergo analysis?", "What kind of people resort to it?", "Do more men or women resort to psychoanalysis?" and so on.

The major factual findings are presented on the whole in a clear fashion; whenever group differences are stated, the probability of null hypothesis is duly indicated. (This is still a rare novelty in most European psychological writings). The findings are not surprising: they reflect a state of utter confusion in the public's minds, all degrees of misinformation, and a wide variety of reasons for either acceptance or rejection of the doctrine and its applications. To the American reader one finding is of special interest: the great frequency with which psychoanalysis is regarded as an American invention and importation, harmful, because it serves the purpose of adjusting the patient to American way of life which is energetically rejected.



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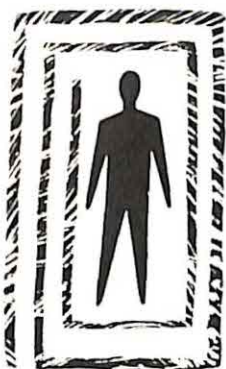
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The second part of the book is based on the content analysis of 1451 articles on psychoanalysis which appeared in 241 popular publications during an unspecified period of time after 1951. Again, the study of this mirror and agent of public opinion discloses most chaotic conditions; only the Catholic and the Communist press present a more or less consistent body of opinion.

The factual findings, which in themselves could have only limited local and temporal interest, serve the author as an empirical basis for developing an intricate network of conceptual distinctions and hypotheses, valuable in guiding future research. His theory is concerned with various aspects (individual, social, linguistic) of the formation of the "public image" on any issue, as well as with the processes by which such an image influences individual and group behavior. The theoretical structure erected by the author is truly impressive; in it lies the greatest value of the book. The scope of this review does not permit detailed presentation of the numerous concepts which the author introduces into his theoretical analysis. It must suffice to call the psychologists' attention to the chapter on the cognitive aspects of "la représentation sociale," in which the author makes an ingenious use of some of Piaget's ideas. The author's general orientation may be best described as an original synthesis of ideas coming from three main sources: from the French sociological tradition (Durkheim and Tarde), from Marxism and from the contemporary American empirical research on public opinion.

The empirical study which led to quite important theoretical results, paradoxically, suffers from one fundamental weakness. To study the distortions in public opinion of the true image of anything under consideration, one must state what this true image is. This the author refuses to do, although he implies a certain "model." Even the author of the preface, Professor Lagache, brings out this difficulty. Undoubtedly the popularity of psychoanalysis raises many questions which can be answered only by confrontation of popular misconceptions with the objectively ascertained state of affairs. The complete

social psychology of psychoanalysis is still to be written.

This shortcoming of the book does not diminish the value of author's penetrating insights and constructive sug-

gestions with regard to the theory and methodology of research in the area of public opinion. All interested in that area should read the book. The effort will be richly rewarded.

## Words, Music and the Brain

Karl Kleist. Translated by F. J. Fish and J. B. Stanton

*Sensory Aphasia and Amusia: The Myeloarchitectonic Basis.* New York: Pergamon Press, 1962. Pp. vii + 95. \$5.00.

Reviewed by RALPH M. REITAN

*The author, the late Professor Karl Kleist, was at the time of his death in 1960, Chairman of Neurology and Psychiatry, University of Frankfurt am Main and Director, Research Institute for Brain Pathology and Psychopathology. He is the author of Gehirn-pathologie (1934) and has been described as the last of a great generation of famous German clinical neuropsychiatrists. The reviewer, Ralph Reitan received his 1950 PhD from the University of Chicago and stayed on there to teach for a while before moving to the Indiana University Medical Center where he still is teaching. His title is Professor of Psychology (Neurology) and Director, Section of Neuropsychology, Indiana University Medical Center. A primary interest, developed while he was working with brain-injured soldiers in World War II, concerns the psychological effects, including aphasia, of brain lesions. He has published extensively on the subject and has written on it for the 1962 edition of the Annual Review of Psychology.*

THE GERMAN tradition of detailed specificity in postulated relationships between aphasic symptoms and the location of critical brain lesions has received another contribution in this small volume. The author's interest is to correlate clinical symptoms of aphasia with areas of damage in the cerebral hemispheres and, more especially, with myeloarchitectonic regions, subregions, and areas of the temporal lobes.

The cytoarchitectonic structure of the cerebral cortex, in terms of the detail which has been claimed, has received serious criticism regarding the reliability with which morphological differences in cell bodies provide a valid basis for differentiation of specific areas (Bailey and von Bonin; Lashley and Clark). While similar cross-validation studies have not been done with regard to myeloarchitectonic areas, one would expect the results to be at least as equivocal because of somewhat more difficult staining problems with cell fibers as compared to cell bodies. Nevertheless, Kleist uses the myeloarchitectonics of the temporal lobe (a classification of differences in fiber structure into 7 regions, subdivided into 20 subregions, further subdivided into 60 areas) in his attempt to discern which areas were involved in patients with sensory aphasia.

Each of the patients used for illustrative purposes had extensive brain lesions, although in some they were principally restricted to the left temporal lobe. Careful myeloarchitectonic studies had been performed in every instance. Even if we were to assume perfect reliability, validity, and generality of these descriptions of the brain lesions, serious problems would still exist with respect to the clinico-pathological correlations. In some patients death occurred soon after the brain was damaged whereas in one instance approximately 20 years intervened. Changes in the brain lesions and in the clinical symptoms over long periods of time, uncontrolled with respect to temporal





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—Rudolf Arnheim, *Sarah Lawrence College*

1962; 828 pages; \$9.00 text

## Introduction to the Statistical Method

*Foundations and Use in the Behavioral Sciences*

by KENNETH R. HAMMOND, *University of Colorado*  
and JAMES E. HOUSEHOLDER, *Humboldt State College*

Enlivened by 31 numbered boxes that feed the student gems of historical interest or dramatic pronouncements about statistics, this text provides a pleasant introduction to the subject. For reasons easy to appreciate, I like the organization. It begins with its feet on the ground in the theory of measurement and goes all the way to the theory of decision.

—Stanley S. Stevens, *Harvard University*

1962; 431 pages; \$7.00 text

## Elements of Psychology

by DAVID KRECH and RICHARD S. CRUTCHFIELD  
*both of the University of California, Berkeley*

This is without a doubt a superior text in every way. It is probably the most coherently written and theoretically consistent book in the field today. It presents some innovations, e.g., the "box" presentation of actual experiments and the integrated treatment of physiology . . .

—Lee Sechrest, *Northwestern University*

758 pages; \$7.00 text

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correlations, constitute inescapable but extremely complex problems.

The clinical evaluation of aphasic symptoms of the illustrative patients leaves much to be desired. These evaluations appear to be based principally on casual interpersonal contacts of a number of physicians with the patients rather than upon standardized examinations administered to each patient. While this is quite understandable, since some of the patients were evaluated during the 19th century, the aim of establishing valid clinico-pathological correlations is hampered.

In the clinico-pathological correlations in individual patients Kleist frequently draws specific conclusions that appear to derive from his expectations rather than from fully adequate evidence. In discussing patient Pitt (p. 50), he says, for example: "There is only limited involvement of the second temporal convolution in the upper border of the territory of the subregio caudodorsalis. It is clear, therefore, that this region also has nothing to do with disturbance of the repetition of speech." This instance illustrates the problem of generalizing from specifically selected individual cases. A proper design should provide an equal sampling opportunity for inclusion of patients with coincidence of lesion location and behavioral deficit, of patients with damage of the area in question but without the behavioral deficit, of patients with lesions in other areas but with the behavioral deficit, and of patients with lesions in other areas but without the behavioral deficit. The consequences of disregard of such necessary comparisons lead Kleist to postulate what appear to be arguments of convenience to interpret his clinical findings. For example, he does not hesitate to postulate right temporal lobe dominance for speech comprehension and left frontal lobe dominance for speech expression in a particular patient when this suits his clinico-pathological rationale, even though no independent criteria for such a postulate are present in the case of this patient as compared with others in which such a postulate is not invoked. In the same area of investigation, *Speech and Brain-Mechanisms*, by Penfield and Roberts stands in striking contrast to this type of

permissiveness in interpretation.

Kleist offers many conclusions concerning the dependence of receptive language and musical functions upon the integrity of myeloarchitectonic regions, subregions, and areas. One may agree with Sir Russell Brain, in his brief introduction to this volume, that, "His

analysis of their (the patients) psychological disorders is always stimulating and his stress upon myeloarchitectonics may challenge those who do not accept his interpretation to provide a better one of a structural organization which may well possess an important functional significance."

## Into the Depths of Management

Chris Argyris

*Interpersonal Competence and Organizational Effectiveness.* Homewood, Ill.: Richard D. Irwin, 1962. Pp. vii + 292. \$6.50.

Reviewed by JACK R. GIBB

*The author, Chris Argyris, Professor in the School of Engineering at Yale University, has long been interested in the complexity of organizations. He is the author of Personality and Organization, and Understanding Organizational Behavior (CP, August 1960, 5, 272). The reviewer, Jack R. Gibb, has had at least an equally long and equally pervasive interest in group and organizational phenomena. He has kept his research interests alive while teaching at Brigham Young University and at the University of Colorado, and while holding appointments as Research Professor at the Fels Group Dynamics Center, and as Director of Research for the National Education Association's National Training Laboratory. Now a consulting psychologist in private practice, part of his time is presently occupied on a project, sponsored by the office of Naval Research, to study defensive behavior in small groups.*

Argyris is one of an articulate group of behavioral scientists and management consultants who have formulated theories of management that challenge both comfortable practices and traditional assumptions in the field of management. His statements of theory have been inventive, imaginative, and bold. Always challenging, sometimes intemperate, his statements have induced bimodal reactions ranging from ardent acceptance to militant rejection.

Argyris' latest book is a report of an attack upon a critical problem: the relationships between interpersonal competence and organizational effectiveness. The book has many strengths, some limitations, and will be provocative to several varieties of reader.

Viewed as a contribution to theory, the book is provocative and helpful. Stemming from a tradition in psychology represented by such persons as Rogers, Maslow, Sullivan and Fromm, his model relentlessly and cryptically presents the implications of the developmental, interpersonal, and phenomenological psychologies for managerial practice. His model is heuristic, susceptible to empirical test, and places focus upon a selected few variables which an increasing number of theorists and practitioners think critical to personality and organization theory.

Viewed as a case study of an industrial consulting experience, the book

THIS BOOK is likely to lead to productive controversy. Reported is a study designed to test a controversial theory and a controversial method of management development. The writing is incisive. The issues are clearly drawn and central to both managerial practice and to the development of managers. The conclusions are challenging. The reactions to the book are likely to be polar.



will be illuminating to a growing group of behavioral scientists who are building action and research relationships with industry. His diagnosis of the top management team of a large corporation is particularly penetrating and will have a valid ring to experienced consultants and to a growing body of theory-conscious executives. He gives informative and convincing protocols of a selected group of eleven executives who are undergoing T group training.

**V**IEWED as action research, the study suffers from the many methodological limitations that are perhaps necessarily incident to action research in large organizations, particularly until the client system is suitably developed to the point where conditions permit rigorous designs. Argyris makes a realistic compromise between the demands of a consultant-client relationship and the requirements of experimental design. As was indicated earlier, the model (derived from his theory) and the training method (the experimental treatment) are each controversial. Readers who are initially sympathetic to his growth-centered theory and who are favorably oriented toward the laboratory method of training will find much to corroborate their views. Critics who are unfavorably disposed toward the assumptions underlying his model or who have a negative orientation toward T group training, or both, may find the design and the findings less than satisfying.

The total situation required that the same person be the theorist, diagnostician, consultant, interviewer, and data analyst. The author is aware of this limitation and cites several safeguards taken to mitigate the possible effects of this personal variable. A colleague, Dr. Roger Harrison, was asked to make an independent administration of the Kelly Role Repertory instrument to the experimental and control group as a possible check on the observer bias. Harrison's findings are suggestive but equivocal. The members of the experimental group (those having T group training) did increase somewhat their tendency to describe each other in interpersonal terms after training, a finding that corroborates a prediction from the model.

However, the members did not transfer this tendency to descriptions of persons not members of the training group.

In major instances the results tend to corroborate earlier findings in studies assessing the effects of the laboratory method upon interpersonal competence and upon behavior within organizations. It would perhaps have strengthened the findings to have compared them with data from such studies as the Miles, Soar, Buchanan, or Weschler studies on T groups. Such a comparison would have been illuminating to the readers, would have added some corroborative strength to the findings, and would perhaps have influenced the interpretation of the data.

Because the book was written for a non-professional audience, many details of interest to the behavioral scientist are omitted. A complicated series of questionnaires, behavioral observations, and interviews are used at critical stages throughout the consultation, diagnosis, training, and post-training consultation. The data presented are interesting, often quite convincing, but necessarily fragmentary. The methodologist would find it helpful to examine in more detail the other items on the questionnaires, the protocols from the interviews, the statistical analyses of the data, and the detailed interview schedule.

The study has programmatic implications. Constructs such as trust, openness, and authenticity are not easily defined in satisfying operational terms. A model as suggestive and as significant as that presented in the book requires multiple measurements and rigorous empirical test. The study reported is a courageous exploratory test that opens the door to much research.

The book makes a major contribution to a problem of critical significance to applied behavioral science. It will be widely read and will create an impetus to further research and theory building.



*There is hardly a more direct way to encourage the grasp of the essence of a method than by following a recapitulation of its evolution.*

—ANATOL RAPOPORT



## Progressively Longitudinal

Margaret Willis. With a chapter by  
Lou L. Labrant

*The Guinea Pigs After Twenty Years.* Columbus: Ohio State University Press, 1961. Pp. xv + 340. \$5.95.

Reviewed by EVELYN WESTREM

*The author, Margaret Willis, is an historian who has been associated with Ohio State's University School since 1932. Evelyn Westrem, the reviewer, received her master's degree from the University of Southern California and now works as a social scientist in the Educational Research Division of the System Development Corporation. She has, and does things about, a special interest in remedial reading and in automated teaching.*

**T**HE BOOK, *The Guinea Pigs After 20 Years*, is a unique follow-up of a special educational program instituted at the University School at Ohio State University in the 1930's, a program following the philosophies for curriculum development of John Dewey and Boyd Bode.

The data on which the report is based were obtained from the entire graduating class of 1938. Fifty-two of the 55 class members returned biographical questionnaires, and extended personal interviews were conducted with the 45 students who had been in the program for three or more years. Since subjects of the study evidenced a range of I. Q.'s (71-170) and diversity in background, special education rather than selection seems to account for the results.

The thesis of the study can be stated best in the author's own words: "If basic high-school curriculum reorganization is worth the effort, it should have results which are apparent in the adult living of the students who experience it." The follow-up, then, was an attempt to associate reported aspects of the



adult living of the "guinea pigs" with their high-school experience.

The book is divided into three parts. Part I provides background information about the students, describes the educational program, and details the method of collecting and analyzing the follow-up data. Part II presents the data, classified and compared with other research studies. Part III describes the philosophy of the University School and summarizes the ways in which its values and goals were evidenced in the adult living patterns of the students.

The hazards involved in attempting such a treatment are obvious to anyone who has summarized questionnaire and interview data. Willis does not minimize the problems encountered. She explicitly declares her personal biases which might have influenced the interpretation and classification of responses. Frequent comparisons are made with other surveys. Many categories of responses of the University School "guinea pigs" are compared with Terman's gifted children and with a survey of the Princeton class of 1944; in every case the similarities and differences are noted so the reader is permitted to judge the appropriateness of the comparisons. Procedures and techniques are detailed sufficiently to enable investigators to collate future studies with her data.

It is impossible to summarize the data tabulated in some 63 tables in a short review. However, it seems clear that the "guinea pigs" compare favorably with groups reported in other research studies. The achievement of the educational "guinea pigs," as expressed by higher degrees and honors earned in college, surpassed Terman's gifted children. Their median income at the reporting date was equivalent to the Princeton class of 1944. In general, the data suggest that the "guinea pigs" are a superior group of adults. Proponents of the once popular progressive education movement will surely find comfort in the results of the study.

The value to educational psychologists of a longitudinal study of students participating in a special program is unquestionable. Too few studies such as this have been done. The literature abounds with reports of experiments, but follow-up studies are infrequent.

When a new program is instituted on an experimental basis, investigators are often anxious about future impairment of the student. Willis' examination of such students should help to allay this pre-experimental fear, since the "guinea pigs" apparently lost little, and perhaps gained much, from their service as subjects.

If educational goals are more than immediate, it is critical that educational programs be evaluated by succeeding studies of this caliber. The detail and rigor evidenced here make the book a genuine contribution to educational research. Many more of the same would certainly contribute to the needed re-examination of long-term goals of educational systems.

## For Educating Educational Researchers

Deobold B. Van Dalen. With two chapters by William J. Meyer

*Understanding Educational Research: An Introduction.* New York: McGraw-Hill, 1962. Pp. xi + 332. \$6.95.

Reviewed by ROBERT L. EGBERT

*Deobold Van Dalen, the principal author here, received his doctorate in education from the University of Michigan and, since 1946 has been Professor of Education at the University of Pittsburgh. William J. Meyer, contributor to the book, is a Syracuse PhD who is now Associate Professor of Psychology and Education, also at Pittsburgh. The reviewer is Robert L. Egbert, who did his doctoral work in educational psychology at Cornell University. On his way from Cornell to his present position with the System Development Corporation, he taught at Utah State University, at Brigham Young University, and worked for a while with the Human Resources Research Office. His present title, held since 1961, is that of Head*

*of the Administrative and Support Section of Educational Research and Development in the Research Directorate at SDC. One may now refer to him, with both brevity and accuracy, as a human factors scientist.*

PROFESSOR Van Dalen indicates two primary purposes for writing his book; it is to serve as a primer for the potential researcher and to help the classroom teacher acquire a better understanding and appreciation for research. In the opinion of this reviewer, the book will be successful in achieving the first objective but will be less successful in reaching the second. In writing for the researcher, it is essential to include details and depth of presentation that are beyond the interests and background of most classroom teachers. Van Dalen has done this and in so doing has written a valuable research methods text, but one which will probably have little appeal to the teacher.

Traditionally, educational research has not been adequately financed. In the past, this deficiency has been reflected by a shortage of good methodology texts and competent researchers. However, during the past thirty years, at least in part as a result of increased foundation and government financial support, real strides have been made in the quality and sophistication of educational research. A comparison of such research as reported in the journals today with that reported during the thirties reveals a marked improvement in control, design, and statistical analysis.

In a similar manner, texts describing methods of educational research have undergone a significant metamorphosis. From pedantic and superficial descriptions of surveys and questionnaires, these books have progressed to stimulating and significant discussions of educational research as a part of the general scientific endeavor and have stressed the importance of conducting research within a framework of theory.

Professor Van Dalen's book is another forward step and should be of considerable value as an aid in the academic preparation of research workers in education.



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HARPER & ROW, PUBLISHERS, 49 E. 33d ST., N.Y. 16, N.Y.



*Understanding Educational Research* is of particular value in depicting educational research as a part of the general scientific enterprise. In chapters devoted to "Methods of Acquiring Knowledge," "General Concepts Concerning the Scientific Method," and "Nature of Observation," discussions of such topics as "deduction," "induction," "assumptions underlying the scientific method" and "the goals of science" are more complete than is typical in educational research books. Many references are made to examples from education.

The research novice will also find superior discussions of "printed resources," "library skills," and "patterns of historical research." He will find much less help in selecting or developing the "tools of research." Here the discussion is superficial. Furthermore, there is a lack of information concerning development of such recent research aids and techniques as the digital computer and simulation methods.

Two somewhat uncommon features are the two chapters on statistics, by William J. Meyer, also of the University of Pittsburgh, and a series of excellent appendices giving examples of such significant topics as "constructing a theoretical framework," in which the author utilizes Ryan's *Characteristics of Teachers*; "hypothesis construction;" "deducing the consequences;" and "criticism of a theory." The chapters on statistics, dealing with "descriptive statistics" and "inferential statistics" appear sound but are so brief that the discussion might better have been restricted to uses and limitations of statistics rather than to attempt presentation of computational procedures.

Several contrasts appear in the quality of the content and format of this book. Obviously a great deal of thought went into organization and the interpretation of this organization through meaningful headings in the text. However, this complete presentation of organization is not apparent in the very brief Table of Contents. On the other hand, the index is well done. The text itself runs the gamut from far too much detail, e.g., ten pages on "improving note taking," to rather striking omissions such as the absence of chapter summaries.

Despite some weaknesses, *Understanding Educational Research* is a fine addition to the field and should contribute significantly to the training of future educational researchers. However, it will probably accomplish less than is intended in improving the knowledge and attitudes of classroom teachers toward educational research.

## On the Ancient Art of Rating

Thomas Whisler and Shirley F. Harper (Eds.)

*Performance Appraisal: Research and Practice.* New York: Holt, Rinehart & Winston, 1962. Pp. v + 593. \$8.50.

Reviewed by WAYNE K. KIRCHNER

*The first author, Thomas Whisler, is a specialist in the field of industrial relations and is now Associate Professor of Industrial Relations in the University of Chicago's Graduate School of Business. Shirley Harper, his collaborator here, is a librarian presently in charge of the library of the Industrial Relations Center at the University of Chicago. Wayne K. Kirchner is an industrial psychologist who did his graduate work with Donald Paterson at the University of Minnesota and who, since 1956, has held both research and managerial positions, not concurrently, with the Minnesota Mining and Manufacturing Company.*

THIS is an excellent book, not only for people who know nothing about performance appraisals and ratings, but also for people who do. At first glance, the book appears to be another collection of readings, but in addition to the readings that have been collected, the authors also spend some time putting across their own viewpoint on performance appraisals and also present case history information about actual com-

pany experience with performance appraisals. It is a handy reference source, yet it is also a book that presents some interesting thoughts on the future of performance appraisals in industry.

The readings range from articles written by such stalwarts as Donald G. Paterson and Arthur Kornhauser back in the early 20's to modern critiques of performance appraisals such as Doug McGregor's. There is no particular quarrel on the part of the reviewer with the readings that were selected. It is interesting, however, while browsing through these readings to discover that many of the things that we talk about today in the rating and appraisal area were talked about forty years, and in some cases considerably longer, ago.

"Deja vu" operates very nicely in this area. Of course, as the authors point out, there is evidence of ratings being carried on as far back as 221 A.D. Unfortunately, some eighteen hundred year old problems of rating—are still nagging away today—problems of objectivity, for example.

In any case, the authors are highly positive toward performance appraisals and their use. In addition they make some suggestions as to how performance appraisals might be used more effectively and it is here that the serious psychologist is probably going to find his most enjoyment. For example, the authors have suggested very strongly that an appraisal plan or system has to have some kind of control feature built into it and they have suggested further that for non-managerial personnel the best control is seniority. In other words, it appears that the authors are suggesting that ratings or appraisals are not always necessary, particularly on jobs where there is little differentiation among the people doing the jobs. This view is probably correct but many personnel men will shake their head about it.

Again, then, this book is a well collected set of readings, with fairly well done case history materials, plus a thoughtful summary statement of the authors' thoughts and predictions concerning the general field of performance appraisal. This reviewer liked it; he thinks most industrial psychologists and most personnel people, in fact, will too.



**SCIENTIFIC CREATIVITY: Its Recognition and Development**

*Edited by Calvin W. Taylor, University of Utah; and Frank Barron, University of California, Berkeley.* Selected papers from the Proceedings of the First, Second, and Third University of Utah Conferences: "The Identification of Creative Scientific Talent," supported by the National Science Foundation. In these papers, leading psychologists report on their pioneering efforts to define the criteria and characteristics of scientific talent. They provide a rich source of suggestions for future research and speculation 1963. 419 pages. \$7.95.

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*By Jerome P. Lysaught and Clarence M. Williams, both of the College of Education, University of Rochester.* Learn how to develop your own programmed instructional unit with this timely and useful guide. It offers teachers in all fields concrete suggestions on: selecting a unit for programming — defining the learners who will use the materials — stating the objectives in behavioral terms — choosing an appropriate program model — constructing actual items in sequence — editing and reviewing those items — and evaluating the results. 1963. 180 pages. \$3.95.

**SAMPLING TECHNIQUES—Second Edition**

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**THE MENTAL PATIENT COMES HOME**

*By Howard E. Freeman, Brandeis University; and Ozzie G. Simmons, University of Colorado.* This book is the result of a large-scale research study of psychiatric rehabilitation by the Community Health Project at the Harvard School of Public Health, with the support of grants by the Professional Services Branch and the National Institute of Mental Health. It describes the posthospital experiences and levels of rehabilitation of 649 mental patients with the objective of determining the factors that affect their behavior and performance after they return home. 1963. 309 pages. \$7.95.

**MEASUREMENT IN PERSONALITY AND COGNITION**

*Edited by Samuel Messick, Educational Testing Service, Princeton, New Jersey; and John Ross, University of Western Australia.* Based on a conference held at Princeton, New Jersey, under the auspices of the Educational Testing Service, this book brings together the latest discussions of major issues in personality measurement. The contributors are specialists with diverse backgrounds, ranging from factor analysis to psychoanalysis. The book, therefore, clarifies diverse modes of approaching the problem of personality measurement, from reliance upon clinical interpretation through developmental description and experimental manipulation to psychometric analysis. 1962. 334 pages. \$5.75.

**MULTIVARIATE PROCEDURES FOR THE BEHAVIORAL SCIENCES**

*By William W. Cooley, Harvard University; and Paul R. Lohnes, University of New Hampshire.* Multivariate statistical analyses have become powerful research tools in the behavioral sciences. This book provides a coherent introduction to the more useful multivariate techniques and their applications in psychology and education. It contains "ready-to-run" computer programs — and shows the reader how to use these techniques without confusing computational detail. 1962. 211 pages. \$6.75.

**COMPUTER SIMULATION OF PERSONALITY: Frontier of Psychological Theory**

*Edited by Silvan S. Tomkins, Princeton University; and Samuel Messick, Educational Testing Service.* The papers and discussions presented at a conference on computer simulation and personality theory held at Educational Testing Service and Princeton University in June, 1962. The purpose of the conference was to evaluate the potential of computer simulation for the field of personality. The papers and discussions analyze the problems of utilizing computers in general psychology, psychoanalytic theory, and cognition and affect. 1963. 325 pages. \$5.00.

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# Folie en Masse

Hugh Mullan and Max Rosenbaum

*Group Psychotherapy: Theory and Practice.* New York: Free Press of Glencoe, 1962. Pp. vii + 360. \$5.95.

Alexander Wolf and Emanuel K. Schwartz

*Psychoanalysis in Groups.* New York: Grune & Stratton, 1962. Pp. v + 326. \$8.00.

Reviewed by WALTER G. KLOPPER

The authors of the two books are identified by the reviewer, who is Walter Klopfer, identified here as an expert on group therapy who received his PhD from the University of California and who first became interested in group therapy while working in Berkeley with Hubert S. Coffey. He has taught at Duke University and at the University of Nebraska College of Medicine. Between these two pedagogical stints, he served as chief clinical psychologist at the Norfolk, Nebraska, State Hospital. Presently he is Professor of Psychology and Director of Clinical Training at the University of Portland, Oregon. He is the author of *The Psychological Report* (1960, CP, July 1961, 6, 236), and is president-elect of the Society for Projective Techniques.

AMONG the many similarities between these two books is the fact that all four of the authors are psychoanalysts, and that each pair comprises one psychiatrist and one psychologist. Alexander Wolf, one of the medical authors, is currently Associate Clinical Professor at the New York Medical College and a prolific writer in the area of group psychoanalysis. He is associated with his co-author, Emanuel Schwartz, at the Post-Graduate Center for Psychotherapy where Wolf is Supervising Psychiatrist and Schwartz is Associate Dean. Both of them have many other positions in the New York area as well.

domain, filling it with a spirited babel. Amidst the din, a few eloquently rational voices have been raised . . ." It is presumed from what follows that the eloquently rational voices are those of the authors and that they propose a rather concrete carrying over of orthodox psychoanalytic principles into the group situation.

Mullan and Rosenbaum on the other hand describe their book as a "theoretical and practical treatise that describes in detail the selection, preparation, and introduction of patients, as well as the hour by hour work that group therapy entails." Thus, our second volume is an "all about" book.

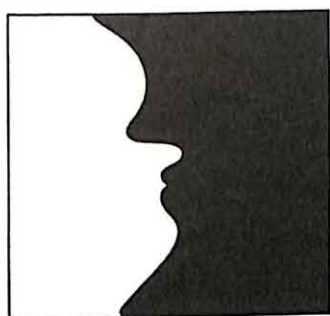
Hugh Mullan is a psychiatrist who practices privately in New York City and is also Medical Director of the New York Alcoholism Vocational Rehabilitation project. His co-author, Max Rosenbaum, is also in private practice in New York and serves as Education Director of the Association for Group Analysis, Inc.

Psychoanalytic group psychotherapy is evidently quite different from group therapy as practiced by other persons. Both the present books agree on encouraging the reader to focus his attention upon intra-personal phenomena, even though Mullan and Rosenbaum agree that the group may enhance the interpersonal efficiency of the patients as well. Relationships within the group are assumed to be false ones, as characterized by such terms as transference and countertransference. Wolf and Schwartz not only do not promote discussion of conscious feelings between group members, but actually discourage discussion of life problems and favor the discussion of fantasies and dreams. Thus, although both of these volumes are psychoanalytically oriented, the one by Wolf and Schwartz appears more doctrinaire and less flexible.

The intention of the authors differs from one volume to the other. In the book on psychoanalysis in groups, Wolberg states in the Foreword: "The same forces of bigotry and self interest that have blighted the field of individual psychotherapy have settled into this new

IN THE book by Wolf and Schwartz there is considerable ambivalence expressed concerning the value of the group as a curative agent. On the one hand there appears to be considerable reluctance to give the patients their head and let them discuss what they wish. One patient's attempts to help another are considered a form of resistance against his own role as a patient. The patients are strongly encouraged to discuss their feelings toward the analyst who remains the focus of attention. Progress is gauged by dreams and other standard psychoanalytic means. The interaction among the patients is grist for the mill only in terms of the depth of response evoked. All difficulties that group members have with one another are assumed to be irrational. All problems can hopefully be reduced to sexual ones and thus become analyzable. The other side of the ambivalence is expressed by these authors in their encouragement of the 'alternate' session. This is considered desirable because it provides an opportunity for interaction. The reasons given seem plausible, including the benefit to the patient in gaining peer support and the enhancement of self-esteem implicit in helping someone else. Why these benefits could not be enjoyed equally in the regular session is not made explicit. The authors decry routine combinations of group and individual analysis, as well as reluctance to provide alternate sessions as both reflecting excessive dominance and dogmatism on the part of





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See also—

### CONTEMPORARY RESEARCH IN PERSONALITY

By Irwin G. Sarason, University of Washington. 1962 Ill. 7 1/2 x 9 1/2 423 pages \$7.00

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the analyst. On the other hand, the term group dynamics is evidently regarded with abhorrence and it is claimed that group dynamicists are persons not interested in history or diagnosis, who ignore individual differences, and place all therapeutic responsibility on the group. The reviewer is led to conclude that Wolf and Schwartz, as judged from this book, are not at all unaware of the curative factors to be found within the peer group, but seem to find it difficult to incorporate this concept into their theoretical frame of reference. The style of the book is quite variable, sometimes being rather philosophical and rambling, and at other times being quite practical and dogmatic.

The book by Mullan and Rosenbaum is much more catholic than it is parochial. The basic practical suggestions for administering and carrying on with a group are pretty standard in this book as in the other. The group preferred is the small one, with the emphasis on heterogeneity of every kind, and with a further emphasis on very careful preparation of the patients for the group process over a period of months. The authors actually compare themselves to Wolf and imply that their approach is less concrete, and more general in applicability. Although they use the same jargon, the meaning seems to be more general. Thus, both sets of authors refer to the group as being in essence a 'family' configuration; but, whereas Wolf and Schwartz consider all relationships as having specific transference meaning, harking back to the original family, Mullan and Rosenbaum talk about persons in intimate situations where they can communicate fully and richly. The form of therapy being described in "Group Psychotherapy" is called regressive-reconstructive, and consists essentially of getting people to trust one another and express their feelings freely after which they are ready to understand their relationships more fully and utilize their intra- and interpersonal resources to a larger extent. This book contains good reviews of other people's work in each relevant area, and fascinating illustrative material. Even though Mullan and Rosenbaum, being psychoanalysts, express some guilt about their interest in actual

situations and interpersonal phenomena, they evidently do lean in that direction.

It seems obvious to this reviewer from both a reading of the current clinical literature and observations of practice, that this is indeed coming to be the decade of group psychotherapy. There seems to be a growing lack of faith in the efficacy of individual psychotherapy as a panacea, and a growing recognition of the realistic and economical nature of group processes for the treatment of emotional disorders. It seems highly encouraging that analysts with their historical proclivity for secrecy and privacy, are willing to emerge into the bright light of the group room and to consider exposing themselves to the cross-currents of real life. Hopefully, it is in this way that their wisdom can be separated from their folly. The two books discussed above are probably representative of the current thinking in this area, one of them representing a more traditional approach and the second being somewhat more avant-garde.

## To Catch Them Young

Douglas K. Candland and James F. Campbell

*Exploring Behavior: An Introduction to Psychology.* New York: Basic Books, 1961. Pp. vii + 179. \$4.50.

Reviewed by ROBERT H. KNAPP

*Douglas K. Candland received his PhD at Princeton University in 1959 and worked as a post-doctoral research fellow at the University of Virginia before moving to Bucknell University where he is now an assistant professor. He met with the second author, James F. Campbell, at the University of Virginia where Campbell remains as an experimental psychologist, specializing in the investigation of learning and motivation. Robert Knapp is a Harvard PhD who, after varied and strange experiences during World War II, settled at Con-*

*necticut's Wesleyan University. He has been there, except for relatively brief adventures elsewhere, for almost 15 years. Among his many publications are, with J. J. Greenbaum, The Younger American Scholar (1955) and, with H. B. Goodrich, The Origins of American Scientists (1953).*

It is probably true that all scientists, including psychologists, are coming to be recruited to their calling at an earlier age. If this be so, this publication has some real importance, for it is its clear purpose to present "scientific" as opposed to "popular" psychology at the high school level. This is not a textbook, though the first three sections are devoted to the range of psychological inquiry, the design of experiments and statistical techniques. Thereafter, however, there are some 12 chapters devoted to the review of experimental areas in psychology of the more classical type. Sensory process, perception and learning are heavily emphasized on the grounds that here "psychology more closely approaches the objective methods of the natural sciences." Descriptions of easily constructed experimental apparatus are given, while each of the experimental sections concludes with a short but useful bibliography. This book, therefore, should be particularly useful in inspiring psychological projects for "science fairs" and similar events. The contents of this book are more safe than imaginative, at the same time they are irreproachable and carry a convincing moral to the effect that psychology, too, may be scientific.

Many psychologists have expressed doubts whether psychology should be introduced into the high school curriculum. But this turns out to be an academic question. The plain fact is that psychology, in one form or another, is being taught at this level, mostly in conjunction with other courses but sometimes separately and in its own name. The present volume, therefore, does supply a thoroughly sound and respectable version of psychological research which should prove a most useful antidote to the careless introduction of "psychology" by non-professional persons at the high school level. Plato has



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Edited by SIGMUND KOCH, Duke University, Volumes V. and VI. available in March, 1963.

These are the fifth and sixth volumes in this vast, seven volume inquiry into the status and tendency of psychological science. Study II seeks an increased understanding of the internal structure of psychological science, and its place in the matrix of scientific activity. The first four volumes are also available.

## ☐ VIGILANCE: A Symposium

By DONALD N. BUCKNER and JAMES J. McGRATH, both of Human Factors Research, Incorporated. McGraw-Hill Series in Psychology. Available in March, 1963.

This volume has been developed from the papers and transcript of an ONR-sponsored symposium on Human Performance on "Vigilance Tasks." The eight participants of the symposium include the major theorists in the field and, as a group, represent a notable cross-section of the various approaches that have been taken to the study of human vigilance. Each chapter of the book represents a formal paper delivered at the symposium and the discussion and critique of the paper by the eight participants. Three general types of papers are included: those concerned with presenting new research findings, those presenting theoretical views and those dealing with methodological issues.

## ☐ ALCOHOL AND CIVILIZATION

Edited by SALVATORE P. LUCIA, University of California, San Francisco. Available in spring, 1963.

A collection of material taken from a symposium held at the University of California School of Medicine in November, 1961. The book presents an objective view of the historical, psychophysiological, and cultural uses of alcohol in society, bringing out the significance of fermented beverages and the role of alcohol in the structure of our society. Areas covered in the symposium fall into five major sections: the effects of alcohol on the body, alcohol and the mind, the social implications of alcohol, alcohol in our society, and the specific and interdisciplinary viewpoints of the symposium speakers . . . all experts in the field. Each chapter constitutes a section in which the assigned contributor emphasizes his specific interest in terms of historical and experimental evidence dealing with the topic.

## ☐ VERBAL BEHAVIOR AND LEARNING: Problems and Processes

Edited by CHARLES N. COFER, New York University; and BARBARA S. MUSGRAVE, Smith College. McGraw-Hill Series in Psychology. Available in March, 1963.

This volume represents the proceedings of the Second Conference on Verbal Learning held under the auspices of the Office of Naval Research and New York University in June, 1961. The majority of the contributors, all top scholars, are the same as those which appear in the book developed from the first conference: *VERBAL BEHAVIOR AND VERBAL LEARNING*, McGraw-Hill, 1961. Presents over-view of the entire field of verbal learning and its relation to verbal behavior. For the advanced student and professional psychologist.

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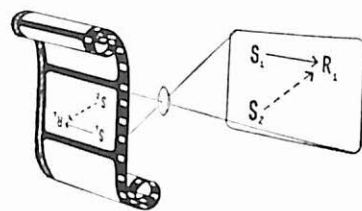
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# INSTRUCTIONAL MEDIA

Edited by A. A. Lumsdaine



## Teaching-Machine Melange

Robert E. Guild, Lecturer

*Teaching Machines. Part I* [30 min.]: *The Theory and Art to Date*; *Part II* [30 min.]: *Programming*. 16mm. black-and-white sound motion picture (kinescope). University of Washington Audio-Visual Services, Seattle, Washington. Rental: \$7.50 (each part); sale \$120 (each part).

Reviewed by DOUGLAS PORTER

Robert Guild, who created these kinescopes in what he describes as a "fairly casual local effort," is assistant professor of psychology and Coordinator of Institutional Educational Research at the University of Washington, where he earlier got both his MS and PhD. A BA from Wilamette University and World War II Navy veteran, he sandwiched in hitches with Rand and Boeing between his student and faculty days at the University of Washington. The reviewer, Douglas Porter is assistant professor of education at Harvard, where he also got his PhD. An experimental educational psychologist with training and predilections in the field of operant conditioning, his research interests are in the investigation of programming variables and evaluative procedures. He is one of the avant garde whose publications have been closely associated with the Skinnerian sally into education throughout the past five or six years.

INFORMATION on teaching machines and programmed instruction is available in just about any communication medium at just about any level, from programmed booklets to elementary "primers," from strip films to movies, from research reports to superficial descriptions. The above films were prepared as a two-part television show with the intended audience of "... intelligent laymen and non-informed educators . . ." The source of the films strongly recommends the use of both parts of the series, or Part I if only one film can be used. However, there are strong reasons to believe that an uninformed audience can be served better by showing Part I only, even if Part II is available.

In Part I, the field of teaching machines is sketched as an active commercial and academic enterprise that must be taken seriously. Several teaching machines are displayed, and a teaching machine is defined as a "... device which controls presentation of subject matter, requires a response, and provides knowledge of results . . ." Pressey is represented by a quotation that gives his provocative conception of teaching machines as efficient labor-saving devices. Skinner is presented as one who would apply "... simple learning principles (reinforcement) . . . more rigorously and broadly than most of us do." This is followed by "real life" examples of reinforcement and a short look at the ineffectiveness of reinforcing contingencies in the classroom. The most compelling section of the film is a brief case history that describes how a child failed to learn the division of fractions through the study of standard textual



materials, and how a controlled presentation by teaching machine would have prevented the slip-ups that led to failure. The usual claims for teaching machines are given (interest is maintained, individual differences are catered to, etc.), and the film ends with rhetorical questions on some of the controversial issues usually aroused by a discussion of teaching machines.

Except for the case study, the film is plainly descriptive and speculative in tone rather than analytical. Reinforcement is the only clearly psychological concept presented, but reinforcement principles go essentially undeveloped, making it impossible to give a plausibly complex analysis of instruction. This leaves the psychologically trained viewer with an impression of shallowness, and must leave the uninitiated with doubts that such "simple principles" can be applied fruitfully to complex behavior. However, to develop reinforcement principles adequately in thirty minutes is no mean task, and failure to do so is an understandable and perhaps forgivable shortcoming of most of the popular literature on teaching machines. Lack of analysis also shows in the treatment of claims for teaching machines. To state, for example, that "The machine has infinite patience; it doesn't scold or resort to sarcasm," is going to leave the layman unimpressed and the psychologist frustrated unless the effects of punishment are considered. Again, this is a general shortcoming of the popular literature.

**A**PPROXIMATELY the first five minutes of Part II is devoted to the display and operation of one commercially available teaching machine and associated programs, then emphasis is shifted to principles and the practice of programming. The core material of Part II consists of two samples of "programmed" instructional material: Socrates' dialogue with the slave boy from *The Meno* and some elementary physics material alleged to represent "... Skinner's conception of a program." Woven into this material are quotations and paraphrases from Skinner and others on general procedures for constructing a program. Quoted almost in their en-

tirety are ten rules of programming from a 1959 APA paper of Gilbert's, in which it is suggested, among other things, that the programmer cast aside all teaching machines until the program is made. Justifiable emphasis is given to the empirical process of program development by trial and revision, and the film ends with an optimistic statement on the success of teaching-machine instruction to date.

Despite the importance of the material in Part II and some excitement engendered by dramatization of the Socrates dialogue, the presentation is marred by a number of unfortunate aspects. Most unfortunate is the choice of samples of programming. The physics material attributed to Skinner is not his own, but was recast from his sample materials published in *Science*, and suffered in the process. Although the Socrates dialogue is lively and appealing it is also misleading. The film makes it clear that teaching machine programs are not based upon a theory of innate ideas, but that leaves the dialogue with the slave boy hanging in the air, for it is not clear where the boy's responses do come from or exactly what discriminations he is making in order to answer Socrates' questions. The other brief samples of programming shown are outright test questions that do not reveal the subtle item-writing techniques of programming. Altogether, the uninformed viewer is probably left with the hazy impression that a program is pretty much like a conventional test.

Although Gilbert's rules are picturesque and well taken, in many instances, they are not suitable fare for a general audience in which they could engender considerable misunderstanding. Finally, one is left with the distinct impression that the first five minutes of the second film constitute a commercial plug for the products of one teaching-machine manufacturer.

Summing up, Part I of these films gives a creditable general introduction to the field of teaching machines without sufficient samples of programs themselves, without discussing programming procedures, and without sufficient psychological depth to stimulate a sophisticated audience. Although Part II is concerned with programming proce-

dures and gives a clear idea of the importance of experimentally revising a program (important matters not covered in Part I), the examples of programming are not apt and some of the materials presented require discussion in order to avoid misunderstanding by an uninformed audience. In comparison to other films (NEA/DAVI's *Teaching machines and Programmed Learning*, and the American Institute for Research's *One Step at a Time* [see below]), Part I of this series probably provides the best available general introduction, while Part II falls short of the AIR film in describing programs and program development.

## Back to Page One?

Robert W. Mager

*Preparing Objectives for Programed Instruction.* Pp. 62. \$1.75. San Francisco: Fearon Publishers, 1961.

Reviewed by LASSAR G. GOTKIN

The author, Dr. Mager, is senior research scientist for Varian Associates and has recently, along with other activities in the programed-instruction field, devised a "walk-in" teaching machine to provide a controlled experimental learning environment. A 1954 Iowa PhD, he spent a number of years with Army's HumRRO and has taught at Ohio University and Sacramento State College. The reviewer, Lassar Gotkin, is an EdD from Teachers College, Columbia University. He is now director of the Reading Improvement Service at the Center for Programed Instruction in New York City, and is working on a program for junior high school students who read two or three years behind their grade level. He is particularly interested in extra-program variables (e.g., teacher, classroom environment) that influence programs' effectiveness.



THIS book is based on the reasonable premise that in order to prepare "instructional programs which help us reach our objectives, we must first be sure our objectives are clearly and unequivocally stated." If the objectives of this book (about preparing objectives for programmed instruction) are achieved the reader will, he is told, be able:

- "a. Given one or more instructional objective . . . to select those stated in performance terms.
- b. Given a well-written instructional objective . . . to identify the portion of it which defines minimum acceptable performance.
- c. Given one or more performance (test) items . . . to select those appropriate to the evaluation of the objectives." (p. 1)

The material is presented in a Crowder-like program format. After reading a few paragraphs about some concept, the reader is required to choose the correct answer to a question from alternatives (usually two). On the basis of his response, the reader is "branched" to a particular page. When an incorrect response is made, the reader is led through an explanation of his error and is then instructed to return to the original page to make another choice. To assess the adequacy of the learner's grasp of the text, a self-test is provided at the end of the book and the reader is told that for the author to have reached his objectives (stated on page 1), you can only make 7 errors or less out of 44 items."

Dr. Mager has written clearly about stating objectives meaningfully, identifying terminal behavior, and defining criterion performance. In this text he has stated his own objectives, provided a Crowder-type program to achieve them, and supplied a criterion test (along with a standard of performance) for the reader to assess his own learning.

The potential consumer has the responsibility of judging the programmer's objectives, critically analyzing the program, and questioning the standards—the latter two can even be explored empirically. This reviewer finds (1) the stated objectives trivial and out of line with the title and content of the book, (2) the branching items at the end of

each explanation out of accord with the explanatory material and requiring such gross discriminations that intelligent readers can make correct choices without reading the text, and (3) the test standard attainable without reading the text. Dr. Mager has provided an illustration of the difference between merely writing about programs and actually producing effective programs.

THE only behavior required in this program is that of making choices between two (or occasionally more than two) statements. However, the learner's ability to make correct choices, though necessary, is not a sufficient indication that he can prepare meaningfully stated objectives.

In a branching program the learner's path is determined by the choices he makes. Six secretaries who had not read the text were given the branching items. Analysis of their responses reveals that these "naive" subjects were able to make correct selections without benefit of the explanatory text. The subjects had a mean of 11 out of 13 (85%) correct choices without having read the text. Not only did these "naive" subjects make almost no incorrect choices, but, the only item missed by the majority was one in which the author erroneously maintains that the ability to "derive the quadratic equation" does not indicate that the individual can "solve quadratic equations." Many programmers overshoot an objective in order to assure achieving it. Deriving the quadratic equation requires performance of a set of operations which can be used to solve all possible quadratics. The data indicates that the items which the author believes measure understanding of the text involve such gross discriminations that most readers can make correct selections solely by reading the answer choices.

Criterion-test performance is established as 7 or less wrong, or 84% or more correct, which happens to represent performance at least one standard deviation above expectation (if the respondent were merely guessing on a test of two-choice items). The six "naive" subjects averaged 7 errors (criterion performance) on the self-test without

even going through the program. Four of the six reached criterion performance of 7 or less errors.

What Dr. Mager has written about objectives is well worth reading. Most readers, however, by making correct choices, would omit some of the best parts, e.g., a diagram and explanation differentiating prerequisites to a course, the course, and its objectives. Dr. Mager appears to have been confined by the method of programing he has chosen; both his objectives and the behavior required of the learner in the program are shaped to fit within the boundaries of the method. Furthermore, his choice of a standard of performance on the posttest is statistically naive and functionally misleading.

In this review I have raised three of the questions which the consumer should ask of every program: (1) Is the content appropriate? (2) Is it well programmed? (3) Does it achieve its objectives? For the latter two questions, I have demonstrated a few of the crude but powerful empirical checks that can be used to evaluate a program with only a few test subjects. Consumers, especially school personnel, have the responsibility to test programs even when the publisher provides statistics.

At the end of the terminal test the student is asked, "How well did you do?" If he has not achieved the author's standard, i.e., seven or fewer errors, the learner is instructed to return to page 1. The evidence suggests that Dr. Mager, not the student, ought to heed this advice.



*A dream is a strange thing. Pictures appear with terrifying clarity, the minutest details engraved like pieces of jewelry, and yet, we leap unawares through huge abysses of time and space. Dreams seem to be controlled by wish rather than reason, the heart rather than the head—and yet, what clever, tricky convolutions my reason sometimes makes while I'm asleep! Things quite beyond comprehension happen to reason in dreams!*

—DOSTOYEVSKY  
1877





# Convergent and Divergent Excellence

Jacob W. Getzels and Philip W. Jackson

*Creativity and Intelligence: Explorations with Gifted Students.* New York: Wiley, 1962. Pp. vii + 293. \$6.50.

Reviewed by WILLIAM E. COFFMAN

The first author, Jacob W. Getzels, is a Harvard PhD who during World War II was a clinical psychologist for OSS, who after the war served for a time as counselor at Harvard University and, who, since 1951, has been at the University of Chicago where he is now Professor of Educational Psychology. The second author, Philip W. Jackson, received his PhD from Columbia University. Before and after that he has taught in the Newport, New Jersey, public schools, at the University of Puerto Rico, at Columbia and Wayne Universities, and now at the University of Chicago, where he is Associate Professor of Education. He obviously does things other than teaching. The reviewer, William E. Coffman, has his EdD from Columbia University's Teachers College. He has taught in the public schools, in the US Army, at Horace-Mann-Lincoln Institute Teachers College and at Oklahoma A & M. Since 1952 he has been associated with the Educational Testing Service, where he is currently Director of Research and Development in its College Board Programs Division. His interest in testing goes back to his undergraduate days as a student of education and psychology at Wittenberg College but it received its major impetus from his experience as a doctoral candidate with Robert Thorndike at Teachers College.

IN THIS book, Getzels and Jackson are reporting the results obtained when they ask themselves some cogent ques-

tions about the nature of excellence and then apply their professional skills to the search for answers. Their skills are impressive, particularly those involving the application of creative imagination to the development of a network of relationships between their own data, the data reported by other investigators, and theoretical formulations regarding the nature of the creative process. In addition, they write a fluid line. The book is stimulating in content; it is also pleasing in form.

The central focus of the search is on an examination of the nature of two aspects of cognitive behavior, that which leads to high scores on a typical IQ test (*intelligence*) and that which leads to high scores on some unusual, and frankly experimental, tests requiring responses of a novel or speculative type (*creativity*). By using common terms to convey their specialized meanings, the researchers avoid the pedantry of scientific jargon; at the same time they introduce a hazard for the reader, for it is not easy to keep track of the transitions as the discussion moves from a consideration of the research data (where the technical meanings are intended) to theoretical considerations (where different meanings apply).

The problem under investigation is an important one. It is important for anyone designing an educational program for cultivating excellence in all its variety. It is important for college admissions committees trying to choose from among a flood of applicants those most likely to profit from educational

opportunities. It is important for psychologists trying to understand the etiology of behavior. And it is important for the psychometrician responsible for developing tests for the identification of talent. If the ability to adapt to the new is different from the ability to comprehend the old, we need to know the nature of the differences. If we are directing the spotlight on one kind of excellence to the neglect of another of possibly greater importance, we need to learn how to correct our deficiencies.

The reader searching for definitive answers to his practical problems will be disappointed. The authors warn that their book will be of little use to those searching for standardized tests of creativity for immediate use. Nor will those interested in generalizations regarding the extent of creativity among American adolescents find an answer to their questions. The study has been geared to the development of insights through an intensive look at small and carefully selected samples of admittedly unrepresentative subjects. But the reader will find a host of stimulating ideas assailing him from every page whether he be a curriculum builder, test builder, or theory builder. The authors recognize the limitations of their method: "... we hold our findings not so much definitive as suggestive of further work" (p. 111). But their recognition of limitations does not dampen their enthusiasm for the insights they have developed. Like the creative subjects who are the focus of their investigation, they exhibit an 'openness in the encounter with the world' which leads to stimulating suggestions regarding the nature and nurture of creative talent. Characteristic of the tone of the report is the sentence beginning: "If we may overstate the case somewhat..." (p. 119).

THE METHOD of inquiry is clinical rather than experimental or normative. From a population of 449 bright adolescents (mean IQ of 132) for whom relatively complete data were available, four extreme groups were chosen for study: a group of 28 scoring in the top 20% on IQ but not so high on other measures, a group of 26 high only in *crea-*



tivity, a group of 30 high only in morality, and a group of 35 high only in adjustment. To the extent that the various measures succeeded in their intentions, the subjects of the study might be expected to exhibit unusual behavior. To the extent that interactions among qualities are relevant to effective personality functioning, one might view with caution descriptions developed on extreme deviates. At the outset, for example, one suspects he may be dealing with bookworms and beatniks as representatives of intelligence and creativity.

There is some suggestion of the beatnik in the creative group and some suggestion of the bookworm in the IQ group, but the responses of each do appear to throw light on the problem under study. One wishes, however, that the various tables comparing responses of the extreme groups with each other and with the total group also reported results for sub-groups high on two or more of the measures. As one reads, it becomes clear that these subjects have not been ignored. A number of them are subjects of case studies which round out the report of the study, and their responses are drawn on to a considerable extent in the section which relates the findings of the study to theoretical formulations. In fact, one develops the impression that some of the most convincing 'creative' responses are made by individuals who scored high on both IQ and creativity.

One finds himself wishing at many points that the basic data were available for further study. A table of inter-correlations of test scores suggests that the several measures of creativity are as different from each other as they are from the measure of IQ, but there are no means and standard deviations reported to permit further exploration of the idea. One is impressed by the contrast between the stories of seven of the 26 creatives and of six of the 28 IQ's, but he wonders how impressed he would be if he were able to examine all of the stories. Or how compelling is the case study reported by the authors when compared with the complete data on which it is based? Copies of the tests are included in an appendix, but one looks in vain for the test scores.

Perhaps it is inevitable that the ex-

perimentalist or the psychometrician will feel a bit uncomfortable with the kinds of data collected and interpreted by Getzels and Jackson. Rather than spending time teasing other interpretations from the data reported here, such psychologists might more profitably turn their attention to designing studies in which their own methods are brought to bear on questions raised by the report. For example, Getzels and Jackson use objective tests of achievement to measure school progress and find that their creatives do as well as their high IQ's on these measures. Yet they argue that such tests reward convergent thinking (the kind measured by the IQ test) rather than divergent thinking (the kind measured by their tests of creativity). At the same time they suggest that teachers' judgments (whether based on the reading of essay examinations or observations of behavior in class) are more likely to reward divergent thinking than is the usual objective test of achievement. Yet they imply that teachers' grades were higher for the high IQ's of this study than for the creatives. (Grade data are not reported.) Is it possible that a factor study would reveal high loadings on divergent thinking for well-made achievement tests?

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xviii + 264. 3,000 Lira. \$4.80  
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*The writer is a man overpowered by words, sentences, rhythms, ideas, the drama of ideas when there are lives moving in them, and the forms he can shape from his medium. Language haunts him. Words, sentences, rhythms are not things to him; they are presences. The presence of his medium makes him feel more than he really knows how to think or say. He knows that he is wiser, richer, more perceptive, more sentient when he is immersed in his medium than he can hope to be when he is high and dry in ideas and presences that he can identify and talk about with clear and pedagogical coherence. His medium is a gorgeous confusion upon him and a gorgeous flowering of all possibility. It is a house of great ghosts.*

—JOHN CIARDI



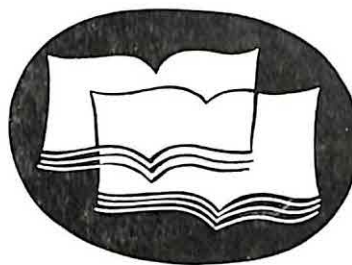


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Contemporary Psychology: A Journal of Reviews is published monthly. The subscription per year is \$10.00, foreign \$10.50, single number \$1.00.

Published by the American Psychological Association at 2210 W. Dallas, Houston, Texas and 1333 Sixteenth St., N.W., Washington 6, D. C.

Second-class postage paid at Houston, Texas and at additional mailing offices. Accepted for mailing at special postage rate stated in section 132.122, Postal Manual.

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# Contemporary Psychology

A JOURNAL OF REVIEWS

VOLUME VIII

March, 1963

NUMBER 3

## Moil in a Mainstream

Colin Cherry (Ed.)

*Information Theory: Papers Read at a Symposium on 'Information Theory'*

Held at the Royal Institution, London, August 29th to September 2, 1960. Washington, D. C.: Butterworth, Inc., 1961. Pp. vii + 476. \$16.50.

Reviewed by WILLIAM A. HILLIX

Edward Colin Cherry, who edited the present volume, was trained as an engineer and is now Professor of Telecommunication at Imperial College, University of London, where he has been since the end of World War II. His long list of earlier publications include the books *Pulses and Transients in Communication Circuits* and *On Human Communication*. The reviewer, William Hillix, is a Senior Scientist at the Navy Electronics Laboratory in San Diego, and also Lecturer at San Diego State College. His career includes Navy experience during WW II, teaching English, farming, fathering twins, completing a doctoral program with Melvin Marx, and a year as USPHS postdoctoral fellow. He went to his present position in 1959 and there he is. He has recently been collaborating with Melvin Marx on a book, *Systems and Theories in Psychology*, soon to be published by McGraw-Hill.

THE BOOK Dr. Cherry has edited is not wholly on information theory. He says:

"The papers now presented range over a wide field and, in spite of the title, are not confined to pure information theory. . . . At the same time, I have tried to select out of all the

papers offered those which seemed most likely to appeal to a common audience."

What is this common audience? I suspect that they have less in common than Dr. Cherry hopes, but let me try to deduce from the 36 papers the properties of the man to whom all of these papers could be addressed. He must certainly have an interest in coding and in the detection and correction of errors (papers 1-7). He must want to know more about communication systems than how to dial his telephone (8-10). He is fascinated by the logic of hypothesis-testing (11) and by human information-handling (12-18). Neurons, and especially neural models, are his cup of tea (19-21). Electronic learners interest him (22-28, 30), as does classification theory (31-33) and various linguistic problems (29, 34-36).

I, at least, have not met this man, but perhaps he can be approximated. The reason he can be, I believe, is that all of these diverse interests have a node at the data processing machine. Information theory is now just one of the threads leading to the node or, to put it another way, information theory has metastasized, and, through its change in form, has begun to invade much of the scientific body.

Because of this somewhat disorderly

growth, the authors of this collection represent medical laboratories, physical laboratories, electrical engineering and electronic departments, psychology departments, phonetics departments, and departments of neurophysiology, as well as the main stream in mathematics and communication. This "common audience" has grown less common as the field has grown, and we must bear in mind that this volume includes only a sampling of the available disparity.

The psychologist is not heavily represented in this fourth London Symposium on Information Theory. He will do a lot of pecking in Cherry's book for a few grains of corn. Of the 52 contributors involved in this volume's 36 papers, at least four are psychologists, and five or six others are suspect. The remaining forty-odd are clearly non-psychologists who are happy with their lot, and they have written papers whose relevance to psychology is remote at best. That is certainly no criticism either of the authors or the editor. In what follows, the reader should remember that I am considering this book only from the point of view of the psychologist; I am not at all competent to judge how well this volume achieved its primary goals.

The first ten papers on coding, detection theory, statistical theory, and telecommunication systems are least likely to interest the psychologist. Their tenor is very largely coding, with additional specialization on errors and error corrections. A. M. Andrew in his paper does make a side reference to coding in sense-organs, but that is as close as the first ten papers come to directly biological subject matter. I. J. Good's paper on the evaluation of evidence is of general scientific interest. The next 10 papers on human reaction



to information (12-14) and sensory information and biological models (15-21) are of most direct relevance (I will return to some of these later). The psychologist can afford to be mild rather than wild about the next seven papers despite the topic allegedly represented (learning mechanisms and other artifacts). "Learning in random nets" by Minsky and Selfridge is an exception to this, but their side-swipes at perceptrons can be found elsewhere. The last group of papers again present slim psychological picking, though here at least the topic (classification theory; syntactics and semantics) is of enough general interest to make reading worthwhile. The simplicity of Professor D. M. MacKay's concluding paper "The Informational Analysis of Questions and Commands" is a welcome relief after the (for your reviewer) fierce problems presented by the mathematics in much of the rest of the book.

I WOULD single out one paper from this volume as most clearly demonstrating the usefulness of information theory in unifying psychological data. It is E. R. F. W. Crossman's "Information and Serial Order in Human Immediate Memory." Dr. Crossman is able to reconcile findings from experiments using lists of varying length, composed of symbols of varying information content. Immediate memory capacity on the various tasks is shown to be more nearly constant than was supposed earlier. Dr. Crossman's contribution was noting that the serial order of symbols, as well as the identity of symbols, must be remembered.

The other papers of psychological interest generally either did not use information theory or measures, or did not really need to use them. Examples: Averbach and Sperling could get by as well in reporting on visual memory by using only the number of letters reported as units; they actually computed the short term visual memory capacity in bits, but might not have bothered if they had not been about to attend a symposium on information theory. Information theory really neither added nor detracted much from their results. The same could be said

of Julesz's interesting work on binocular depth perception. Its only relation to information theory was indeed remote; a computer was used to generate the visual presentations. Green and Swets used concepts from detection and decision theories in generating and discussing their experiment on deferred detection decisions.

IS THE promise of information theory for psychology being realized? The papers presented in this volume seem at least to justify the question.

I believe that information theory as a mathematical set of tools is presently ahead of the empirical observations to which it will eventually be applied. To say that the promise of information theory is not being realized is much like saying that the promise of calculus is not being realized. Both wait upon the collection of more data to which the models are appropriate.

It may have once appeared to the over-optimistic that information theory would revolutionize psychology. There was not enough data available to support a true revolution. The promise will be a promise for a long time, just as for the lover in Keats' poem. As often happens in a new theoretical endeavor, the abstraction has initially attracted too much attention to itself, at the expense of the world to which it must apply.

This does not mean that the psychologist now or at any time can afford to ignore developments in information theory. Information theory and its ramifications remain in active turmoil. Cherry's collection serves notice that the psychologist who ignores basic information theory as one of the accepted tools of his trade, and the new developments that are continually augmenting this tool, will soon find himself on the outside of one of the main streams of activity.

Two sidelights on the main issues deserve a passing comment. First, it is clear that animal (notably rat) psychologists have a new set of allies. Nine papers report on the behavior of machines; only eight give behavioral data on living animals. The authors of the first set of papers should now be available to reinforce their beleaguered col-

leagues who have for years been saying either, "I don't *care* whether people behave like my subjects," or "I don't see why people *shouldn't* behave like my subjects."

Finally, it has occurred to me that the proceedings of a symposium are sometimes like a burlesque show. Proceedings show the almost naked workings of science; the discussions reveal errors and animosities, corrections and courtesies, almost as though scientists were people. As in burlesque, the product is not always smooth, artistic, or well-connected, but it is revealed, and in that there is honesty.

## McNemar

Quinn McNemar

*Psychological Statistics: Third Edition.* New York: Wiley, 1962. Pp. v + 451. \$7.75.

*Reviewed by* BENJAMIN FRUCHTER

*The author, Quinn McNemar, the President-Elect of APA, needs little introduction and that is just what he will get. The reviewer, Benjamin Fruchter, received his PhD from the University of Southern California and has had the great good fortune of spending most of the time since then at the University of Texas, where, among other things, he is presently Professor of Educational Psychology, a position that involves, among other things, the teaching of statistical methods to graduate students. His publications include the book Introduction to Factor Analysis (1954).*

THIS BOOK is the third edition of a well known and widely used textbook on psychological statistics. It is strongest in the area of analysis of variance and hypothesis testing, although there is a balance of coverage, including materials on descriptive statistics and correlational methods.

Since the presentation of the material usually covered in an introductory course is somewhat condensed, its use as a textbook is most appropriate at



the "intermediate" level. The commonly used statistical methods, with the exception of factor analysis, are covered with varying degrees of thoroughness. The book is directed primarily to students, with good discussions of assumptions and uses of the statistical methods. Some topics such as discriminant function, which is covered in less than a page, are presented in a manner that is more suitable for the reference worker. The style of writing is for the most part as clear and concise as can be expected for such complicated subject matter.

New material in this edition includes a chapter on trends and differences in trends, and a summary of the empirical work that has been done on the effect of assumption violations on the  $t$  and  $F$  tests. Also some additional proofs and derivations of the formula for the standard error of the mean, for the difference between independent proportions, and of the connection between variance and chi-square are included. The relationships between some statistical methods, such as the  $\chi^2$ ,  $t$ , and  $z$  tests with the  $F$  test, reliability with analysis of variance, and interaction with correlation, are indicated. A section of exercises and discussion questions relating to the various chapters is included at the back of the book. The emphasis in the exercises is on reasoning and understanding rather than on computation and derivation.

Standardization of symbols is still a problem in psychological statistics. McNemar has shifted to the use of three symbols for the standard deviation,  $\sigma$  for the population,  $S$  for the maximum-likelihood estimate, and  $s$  for the unbiased estimate. He also uses  $\mu$  for the population mean, and  $M$  for the sample mean, with the  $x$  symbol being reserved for use only in the section on the analysis of variance.

With the exception of some additional distribution-free methods very few of the newer developments have been incorporated into the revision. There is, for example, no mention of decision theory, Markov processes, or the electronic computer.

In summary, this is a somewhat augmented and expanded (in length and price) version of the previous edition

with most of its strengths and weaknesses. The stress is on the assumptions and understanding of the statistical

methods, and there is relative freedom from inaccuracies, inconsistencies, and minor errors.

## Left Hand on the Right Side?

Jerome S. Bruner

*On Knowing: Essays for the Left Hand.* Cambridge: Belknap Press of Harvard University Press, 1962. Pp. 165. \$3.75.

Reviewed by RICHARD JESSOR

*Jerome Bruner, the author, is Professor of Psychology at Harvard University and Co-Director of the Center for Cognitive Studies at that institution. He has had faculty connections with Princeton University, with Cambridge University, with the Institute for Advanced Study. But mostly he has been at Harvard University and there has done so much, published so widely and received so many honors that he constitutes a problem for CP's biographer. The reviewer, Richard Jessor, might be said to have taken two doctoral degrees from Julian Rotter at Ohio State, one by effort and one by marriage. (His wife, Lee, is an OSU psychologist as well as a wife and mother.) He has inclined toward a phenomenological approach in his study of personality and is presently plying that approach in a research application of Rotter's social learning theory. In the last four years with his wife as a collaborator, he has worked on a study of deviant and conforming behavior in a tri-ethnic community in Southern Colorado. He is Professor of Psychology at the University of Colorado.*

IN THE day-to-day practice of our craft it is difficult to find a vantage point from which to gain perspective and to sense the course of history. What is ephemeral and what is lasting are matters of debate and there is no escaping the fact that, to some extent, each man writes his own history. Nevertheless, the deep ferment of the past decade in

psychology has become clear to many psychologists, and some of its causes and some of its consequences can at least provisionally be discerned. Most obvious has been the restless discontent over self-imposed constraints as to what constitutes our proper subject matter and what are the proper methods of scientific study. The emphasis on behaviors and on operations, both in the narrowest sense, had simply not yielded an image of man satisfactory to ourselves or to scholars in the other sciences of man and in the humanities. What seems to have been missing—and what has since emerged as perhaps the clearest consequence of the ferment—was a concern for man as an *experiencing* being, an organism with relatively unique capacities for language and symbolism, with deep sensibilities, imaginative, a thinker and a seeker after meaning.

It is these concerns which animate this beautifully written and eloquent set of essays. Revised and reworked from a talk or paper delivered on some occasion during the past five years, each essay ventures boldly yet sensitively into some realm of man's experience, frequently a realm about which psychology has had, till now, little to say. The essays range widely from a consideration of the essential structure of a creative work to an appraisal of art as a source of knowledge; and from a concern with the importance of personal acts of discovery in the educational process to an analysis of the role of knowledge in commitment and action. Despite their separate origin and the



sweep of their subject matter, the essays have a surprising unity and coherence—they all converge on some aspect or function or consequence of knowing.

There is another basis for connectedness in these essays. They are written for the left hand, the hand traditionally symbolizing imagination, feeling and intuition in contrast to the right which has been the representative of order, logic, and rational deliberation. The right hand in psychology, according to Bruner, “. . . has become too stiff with technique, too far from the scanning eye.” The essays, then, represent an attempt to explore the approach of the left hand, an approach perhaps best suited to the initial charting of unknown regions of man’s experience, “. . . an approach whose medium of exchange seems to be the metaphor paid out by the left hand. It is a way that grows happy hunches and ‘lucky’ guesses, that is stirred into connective activity by the poet and the necromancer looking sideways rather than directly.” Lest the reader become apprehensive at what might sound like utterly undisciplined inquiry, I can assure him that throughout these explorations the right hand knew what the left hand was doing.

THE BOOK is divided into three sections, the first entitled “The Shape of Experience,” the second “The Quest for Clarity,” and the last “The Idea of Action.” In “The Shape of Experience” the arena for discussion is the creative enterprise represented largely by literature and painting. Bruner is concerned with art in terms of what it can tell us about thinking, especially creative thinking, and how it provides a source of knowledge. To this end he explores with impressive skill the role of metaphor as a primary mode of concept-formation in art. He shows the way in which the device of the metaphor meets the cognitive needs for economy and organization by suggesting hidden likenesses in diverse aspects of experience and presenting us with surprising and powerful unities.

A not insignificant emergent from Bruner’s analysis of metaphor is the deep similarity of the processes of creative concept-formation in those tradi-

tionally sundered disciplines—art and science. One is reminded here of Bronowski’s profound essay on *Science and Human Values* in which he tells us how Kepler “felt for his laws by way of metaphors” and instructs us that the symbol and the metaphor are as necessary to science as to poetry. Such discussion is at a refreshingly far remove from that sterility we psychologists not long ago indulged in about the problem of “surplus meaning” in our concepts.

The essays in the next section explore the process of imparting and acquiring knowledge—the domain of education. Here, too, Bruner’s concern is for those kinds of conceptual structures which organize knowledge in economical fashion, permit easy retrieval from memory, and enable the learner to go beyond the available information. He shows how objectives such as these—which honor the connectedness of knowledge—are best achieved by an architecture of education that permits and encourages *personal discovery*. There are in this discussion the seeds of a theory of learning in a richer sense than psychology has thus far construed that term.

A fact which becomes abundantly clear from the discussions of education in this section and of esthetics in the preceding one, is that traditional models of motivation have little or no relevance to an understanding of knowing. Bruner finds it necessary to reject the “extrinsic grammar” of drive theory and to seek a congenial cognitive approach to motivation in Robert White’s concept of competence. With it he is able to suggest the nature of the satisfaction intrinsic to looking at a painting, listening to a quartet, or working out a mathematical problem.

THE FINAL set of essays follow out some of the ways in which ideas relate to action in the contemporary world. Though somewhat less coherent a unit than either of its predecessors, this section includes a perceptive analysis of the ways in which the “latent culture”—the guiding myths and values and socialization practices of our society—inevitably shape and take their toll of human experience. Drawing out the implications of the irreversible limits suggested

by the sensory deprivation literature, Bruner asks: “Are we mindful of what it takes by way of intensive exposure to certain forms of experience to unlock human capacities of certain kinds, whether for looking at art or for manipulating abstract symbols?” His answer, which cumulates throughout the book, is, of course, negative, and in this sense the essays represent a welcome form of social criticism, criticism illuminated by empirical knowledge in the science of man.

There is much more in this little book to catch the inquiring eye—the recurrent concern for the conservation of cognitive capacity; the provocative tracing of the role of effort in esthetic appreciation and in learning; the repudiation of such hoary ideas as “readiness” in education; and the development of the concept of effective surprise in creative work.

It is possible, obviously, to argue with many of Bruner’s views, but it seems inappropriate to the spirit of their presentation to do other than nurture them. Clearly Bruner has been successful in what he set out to do, to explore the range of the left hand in the realm of knowing. His fresh and often poetic mapping should make travel much easier for the right-handed workers who will follow behind.

In a remarkably penetrating Epilogue to the third volume of *Psychology: A Study of a Science*, Koch details the decline of the “Age of Theory” and notes the consequent openness of the contemporary psychological scene. He concludes: “The more adventurous ranges of our illimitable subject matter, so effectively repressed or bypassed during recent decades, are no longer proscribed.” Bruner’s book bears him out.



*In order that a man may stop believing in some things, there must be germinating in him a confused faith in others. It is curious to note that almost always the dimension of life in which the new faith begins to establish itself is art.*

—ORTEGA Y GASSET





# Views of Science. Microscopic and Very Otherwise

Arthur Pap

*An Introduction to the Philosophy of Science.* New York: Free Press of Glencoe, 1962. Pp. xiii + 444. \$7.50.

Reviewed by FELIX E. GOODSON

*The author of this posthumous book is Arthur Pap who at the time of his death in 1959 was a member of the Yale Department of Philosophy, and who, to quote Brand Blanshard, was "one of the ablest younger philosophers of his generation." He was born and brought up in Zurich, but came to the United States in the forties to earn his PhD at Columbia University and then to stay on in this country. The reviewer, Felix Goodson, is an experimental psychologist (University of Missouri PhD) who has always had an enduring interest in philosophy with particular emphasis on the problems inherited by psychology from philosophy. He is Associate Professor of Psychology at DePauw University where for a number of years he has taught the history of psychology. Always highly productive of journal articles, he is currently working on a book on psychological theory. In the planning stage is another book, this one to be called Themes in the History of Psychology.*

THE BOOK contains 420 pages (19 chapters) of intensive, at times laboriously minute examination of science—its language, methods, and problems. It is divided into five sections: (1) meaning and verifiability; (2) mathematics, logic and experience; (3) induction and probability; (4) causality and laws of nature and (5) explanation and justification. In his treatment of these topics, although such is not explicitly stated, the author has three basic orientations—logical positivism, relativity

theory, and phenomenology—which powerfully affect both his arguments and his conclusions.

One of the major tasks of the philosophy of science according to Pap is "to distinguish different kinds of 'proofs' corresponding to different kinds of propositions, and thereby to prevent a confusion of different standards of justification of beliefs, and to make explicit the criteria which scientists apply in evaluating the degree to which the known evidence warrants a given proposition." This comment sets the tone for the greater portion of the book. Thus in direct alignment with the tenets of logical positivism the author analyzes many of the statements and assumptions of science in terms of the various kinds of evidence one can accept as establishing their truth. Although syntactics, i.e., the rules governing the manipulation of symbols per se, is dealt with primarily in Chapters 5 through 8, where such topics as the nature of arithmetical statements, the laws of logic, and types of geometry are analyzed, the author uses logical analysis as a constant tool throughout the book in his search for the conditions necessary for the establishment of empirical truth.

The style and form of expression vary greatly from one topic to the next. Curiously, in those chapters dealing with formal logic, mathematics, and theory of probability the sentence structure is often so involved that one is prone to recall that Pap's first language was German and that one of his early fixations was on the Hegelian dialectic. The reader's task is made even more

difficult by the author's tendency to deal at unusual length and in exhausting detail with issues which from the standpoint of the day to day operations of science seem sterile and trivial. This immersion into minutiae most often occurs when the author seemingly becomes obsessed with the need to clarify logically the meaning of a given statement. It is not that such analyses are incorrect or without significance in the confines of logical positivism, it is simply that such detailed perusal seems alienated from the ongoing work of scientists and the development of their disciplines.

IN CRITICIZING Pap's style it must be remembered that the book was published posthumously, and that a number of the essays may have been presented without a final, critical scrutiny on his part. Further, it must be admitted that the unique character and one of the appealing aspects of the book is derived from the pervasive presence of the author's own personality which manifests itself not only in his curiously consistent reactions to issues but to the style within which these reactions are expressed. Indeed, as one follows the occasionally tortuous threads of the writer's arguments it becomes apparent that he (the reader) is participating in the creative thinking process with all its false starts and meanderings. Sometimes when the author becomes aware of the involvement of his discussion he begins the next statement with, "The point is . . ." and then an effort is made to recapitulate and clarify his thinking. Thus the reader follows Pap into numerous blind alleys and along many trivial tangents, from which at times he is rescued by recapitulation. On occasion, however, the argument simply disappears into omnidirectional ambiguity leaving the reader defending his own lack of insight with critical commentary.

It must not be assumed that the entire book is tedious and abstruse. This is not the case. Certain essays, for instance those on geometry, causality, determinism, and behaviorism, are remarkable for their clarity and coherence. Indeed, the entire book is marked by inconsistency of organization and expression. It fluctuates from the tedi-



ously ambiguous to the impeccably clear; from lucid coherency to almost complete lack of integration and organization; from pedantic hairsplitting to profound observation; from meticulous analysis to expansive generalization. Yet the many faults of the book are far outweighed by its virtues; the greatest of which is Pap's piercing insight into difficult issues and his ability to consider such issues with a depth of understanding rarely achieved.

Here are but a few of Pap's conclusions: (1) the major objective of philosophy should be an obstinate search for clear meaning; (2) so called private experiences are confirmable in terms of publicly observable stimuli; (3) the widely held belief that Euclidean geometry has been refuted by the development of consistent non-Euclidean geometries is completely unfounded; (4) an explanation of behavior in terms of inferred needs or motives is not only legitimate but highly desirable; (5) Hume was correct when he stated that causation is nothing more than a relation of uniform succession holding between observable changes; (6) relativity theory finds its roots in subjective idealism, and Newtonian mechanics in physical realism; (7) determinism of human action is incompatible with freedom—and a failure to understand this truism is due to the tendency to equate freedom with "rational choice" and determinism with compulsive or coerced behavior; (8) teleological explanations are clearly legitimate scientific devices; (9) phenomenal states of organism are important determiners of behavior and should be studied by psychologists.

These conclusions are in each case arrived at following an exhaustive examination of related issues and arguments. Admittedly, to remove them from context and qualifications makes them stand out in dogmatic relief; yet they give indication of the diversity of topics considered and suggest, which is actually the case, that the author has great confidence in his own rational processes and a capacity to state fearlessly the conclusions arrived at by their indulgence.

Although the book demands more than a casual grounding in symbolic logic, mathematics, and physics before

it can readily be digested, it is highly recommended for graduate students in psychology. Pap gives priority to the problems of physics but he also considers at length certain issues of interest to many psychologists, i.e.: mind versus behavior, free will versus determinism, teleology and emergence, and the status of value judgments. Furthermore, in spite of previously mentioned problems of style and expression Pap does a better than adequate job of treating the traditional issues of philosophy of science, and he presents a truly exceptional evaluation and discussion of the quasi-metaphysical issues and assumptions which lie at the outer edges of science.

## Talent Search

John C. Flanagan, John T. Dailey,  
Marion F. Shaycoft, William A.  
Gorham, David B. Orr & Isadore  
Goldberg

*Design for a Study of American Youth: 1. The Talents of American Youth.* Boston: Houghton Mifflin, 1962. Pp. 240. \$1.95 (paper) \$4.00 (cloth).

Reviewed by  
DONALD L. THISTLETHWAITE

*All authors of the present book are associated with the American Institute of Research. John Flanagan is the President of the Institute as well as Professor of Psychology at the University of Pittsburgh. John Dailey has been program director since 1958 of the Institute's Project TALENT, an enterprise from which the present book springs. Shaycoft, Gorham, Orr and Goldberg are his co-workers. The reviewer, Donald Thistlethwaite, did his doctoral work with Edward Tolman at the University of California. From Berkeley he moved to Western Reserve University and to the University of Illinois, and then served for a while as associate director of research with the National Merit Scholarship Corporation. In 1961 he moved to Nashville, Tennessee, to become Professor of Psychology*

*at Vanderbilt. His dual primary interests are in social and in educational psychology, and in line with these interests he has been conducting a study, under the sponsorship of the U.S. Office of Education, on factors influencing the recruitment and training of talented college students. He will produce a book on this matter.*

THE title of this highly readable book, the first of four initial reports planned for Project TALENT, is apt. It literally presents a plan, or scheme for a study of American youth. No substantive results are presented. Consequently proof of the values of this unprecedented undertaking must await future reports. We must be content to appraise prospects.

Project TALENT is an educational research program which began in March, 1960 with the testing of nearly 500,000 students in over 1,300 secondary schools. Like Terman's study of gifted children, the project aims to follow up examinees at periodic intervals—one, five, ten, and twenty years after graduation from high school. Like the wartime research on aptitude classification test batteries it seeks to discover formulae for predicting success or failure on the job. The task which Flanagan and his associates have set for themselves is much more difficult than either of these parallels, since a broader range of talents is to be related to success in hundreds of civilian careers.

Half of the book is devoted to describing sample items and the rationale for each test and inventory included in the two-day test battery. The battery included approximately 40 aptitude and achievement tests, a student activities inventory yielding scores on 13 personality scales, an interest inventory designed to assess 16 interest areas, and a 400-item student information blank. With two exceptions all of the tests and inventories were new. To insure that each test made a unique contribution to the battery it was required that a significant part of each test's reliable variance be free of overlap with any of the other tests.

It is hard to say how successful will be this attempt to discover better meth-



ods of identifying human talents. Certainly the authors' predictive battery is a model of careful workmanship and a highly sophisticated sample of the art. Of course, the hard-headed psychologist wonders whether the art will ever be equal to prediction of success in "hundreds of careers within many occupational fields." Even though the sights may be aimed too high there seems little doubt that Project TALENT will increase our information about the talents predictive of success in many occupations.

SOME of the questions posed in the authors' prospectus are of the causal variety: How can talent be developed? How can the talents of American youth be brought to a point of high productivity and usefulness? Thus a second aim of Project TALENT is to discover better methods for developing and utilizing human talents. The major focus of manipulable policies and events at this stage of the project is necessarily the high school. By comparing the products of high schools which differ in size, teaching staff, per-pupil expenditure, curriculum, guidance services, etc. it will be possible to identify school characteristics associated with the greatest payoff. Although the designs to be used in testing out the effects of different educational and counseling experiences in high school are only briefly sketched, they appear adequate for the task. For example, the authors state that with their IBM-7070 they can compute partial correlation coefficients holding constant up to 90 or 100 other measures.

One possibly serious omission in the data collection procedures is the failure to obtain measures of the personal and behavioral characteristics of teachers. Some teachers are more skillful at nurturing and developing talent than others. Knowledge of the characteristics which distinguish effective teachers should help us to select and train teachers. Such data might well have more important implications for developing talent than the school data collected. It is hoped that this omission will be remedied and that equal attention will be given to the student's educational experiences in college in the follow-up surveys on Project TALENT.

There are few psychologists who are not concerned with at least some of the prediction, counseling, and training problems posed in this important re-

search effort. Certainly this book is required reading for all persons wishing to follow the progress of Project TALENT.

## Forty Years a Revisionist

Franz Alexander

*The Scope of Psychoanalysis: 1921-1961.* (Selected papers of F. Alexander). New York: Basic Books, 1962. Pp. xix + 594. \$12.50.

Reviewed by STANLEY F. SCHNEIDER

*The author, Franz Alexander, came from Budapest to this country in 1930 to accept an appointment as Visiting Professor of Psychoanalysis at the University of Chicago. In 1932 he became the first Director of the Chicago Institute of Psychoanalysis. Since that period, both in Chicago and more recently on the West Coast, he has continued to work vigorously and to write prolifically in the psychiatric and psychoanalytic area. The three of his books he reports himself to remember most clearly are The Age of Unreason (1942), Fundamentals of Psychoanalysis (1948) and The Western Mind in Transition (1960, CP, July 1961, 6, 244). Stanley F. Schneider, the reviewer, received his University of Michigan PhD in 1953 and since that time has worked mostly in Ann Arbor, first in the Department of Psychology and later in the Department of Psychiatry in the Medical School. His interests and activities have involved the training and teaching of clinical psychologists, the psychiatric education of medical students, and both the public and private practice of clinical psychology. He has recently become chief psychologist at a psychoanalytically oriented training clinic in Los Angeles, and there his pattern of activities has moved somewhat away from the academic and somewhat toward a service orientation. He still, however, finds himself involved*

*with the development of students and with the large questions of where psychology is going and what can be done to see that it's not too late when it gets there.*

THE NEARLY fifty essays in this volume reflect the productive work of one of the most inventive and controversial figures in psychoanalysis. They show a remarkable diversity of content, although most of them are devoted to psychoanalytic theory and treatment. Alexander's earliest papers are products of an especially exciting period in the history of psychoanalysis, marked by Freud's development of the structural hypothesis. Alexander seemed kindled particularly by the complex network of mutual influences between the ego and superego, and by pathological states characterized by the intransigence of the superego as well as by the possibilities inherent in the modification of the severity of this newest psychic institution.

The author shows a rather striking consistency in the development of his ideas, predicated upon the *relative* neglect of the more immutable features of the personality, specifically those subsumed under the id. If we accept this core proposition, we may examine the way his rich contributions radiate, like the spokes of a wheel, to clinical understanding, theory, treatment, to profes-



sional education and training and to the state of modern man in society. The prevailing emotional tone of this volume is one of optimism, the prevailing attitudes those of flexibility, adventure, belief in change and modification, open-mindedness, respect for scientific rigor and an apparently boundless curiosity about what people and social institutions are like. It is easy to be caught up in Alexander's sheer intellectual zest and persuasiveness, not only out of respect for the man and his ideas, but also because he writes with a beauty and simplicity of style surpassed only by Freud himself among the more prolific psychoanalytic authors.

The early clinical papers deal with a gallery of maladaptations calculated to be the despair of most therapists—the need for punishment, moral masochism, character types bent on self destruction and criminality. Alexander's continuing interest in criminal behavior culminated in his re-evaluation of our entire concept of justice for the criminal, a re-evaluation based upon the notion that punishment remains ineffective because the criminal needs it as a salve for his pathological conscience, and then, having suffered, he feels able to justify new crimes. Alexander's distinctions between guilt and inferiority feelings, between structural and instinctual conflicts, and his delineation of the neurotic character contain insights of lasting value to both the clinician and the personality theorist.

One may follow the metamorphosis of Alexander's thinking on instinct theory in this volume, from his early doubts toward a tentative resolution of this still vexing problem. In place of a dual instinct theory (sexual and aggressive drives) he offers a view based upon the degree of organization of impulses; impulses vary from those discharged in an isolated fashion for their own sake (sexual) to those discharged in the service of the organism as a whole as constituent parts of a complex, organized goal structure (non-sexual). The assumption is made of a principle of surplus energy—energy that may be released in all manner of erotic activity, including play, exploration and creativity. The role of aggression is subordinated, since aggressive impulses may be discharged sexually (as in sadism) or non-sexually, in the service of self-preservation, and therefore aggression may be treated in the same fashion as any other type of discharge. Thus revised, instinct theory



FRANZ ALEXANDER

appears consonant with communication theory, the law of entropy and, ultimately, with general systems theory.

On the level of culture, the author contends that adventure, change, creativity and individuality are the results of the dynamic use of surplus energy, in contrast with the need for security, which is bound up with those energies required for survival and self preservation. He feels that man's greatest achievements accrue from the way leisure time is used, and he is dismayed by our current preoccupation with security, by our apparent loss of identity and by our other-directedness.

Alexander's flexibility, daring and optimism are nowhere more evident than in his innovations in the area of treatment and, contingent upon these, in his proposals for psychoanalytic education and training. Nor is any of his work more controversial. His restlessness with protracted treatment and with the therapeutic ineffectiveness of abreaction and intellectual insight in themselves was accompanied by his growing awareness of the influence of all factors, cultural as well as biological, upon the uniqueness of the individual child in the immediate family constellation. Alexander's conviction that reliving emotional experiences in treatment was of greater therapeutic importance than the recollection of repressed memories became the central issue in his struggle with orthodoxy. In order to attain this 'corrective emotional experience' Alexander was willing to

manipulate the transference, and even the environment, consciously and actively, to create that atmosphere most effective in undoing the results of the patient's early pathogenic setting. He firmly regarded the therapist as a real person in a real interpersonal involvement, rather than as a 'blank screen', and his educative philosophy eschewed technique in favor of research and psychodynamic understanding. Some observers feel that these changes have blurred the distinction between psychoanalysis and psychotherapy, a distinction without meaning to Alexander so long as any treatment is based upon sound psychodynamic principles. His ideas in this area richly deserve the clinician's attention, but they obviously cannot be detailed here.

Before turning to a somewhat more general evaluative appraisal, I would comment briefly on two papers which are outstanding, for quite different reasons. "Two Forms of Regression and Their Therapeutic Implications" (1956) is notably brilliant, not only for its masterful clarification of the manifold relations between pregenital and oedipal phases of development, but also because it provides a very excellent illustration of regression as a defense. The other paper, "A Note on Falstaff" (1933) is Alexander's sole excursion here into literature. The marriage of psychoanalysis and literature has produced some lamentable offspring. Alfred Kazin tersely observed how many psychoanalysts wanted to be writers and how many writers have tried to become analysts. He found it difficult to say which group had less to contribute to literature, since both wrote badly and reduced art to formulas in the process. But Alexander's little paper is a model of restraint and illumination, and can serve as an example to those with literary aspirations.

THE volume's title is misleading, since this collection hardly represents the scope of psychoanalysis. The reader will find nothing here, for example, about the major developments of the past two decades in ego psychology. Names like Kris and Erikson are given only the briefest mention, and Hartmann, Rapaport and Lowenstein are conspicuously absent. Alexander does not disregard the ego, but he seems almost exclusively concerned with synthesis and integration, its highest order functions. Again,



in his optimistic framework, he tends to bypass the scutwork of the ego in favor of a grand end-product, a tendency conceptually related to his underemphasis on instinctual features in general and aggression in particular. It is highly consistent with this orientation that his methods of treatment have become highly interpersonal in nature at the expense of being intrapsychic, stressing the present rather than the past. This relative neglect of the past is tangentially evident even in his sympathetic critique of existentialism, which he values as a revolt against dehumanizing influences in society. It seems unimportant to him that existential theory revolves almost *solely* around the concept of identity, or that existential therapy is ahistorical. Despite these things, the transference retains paramount significance in his treatment. But in his method it must be often abridged, and, since it is certainly actively manipulated, one wonders how fully it is allowed to develop. Indeed, Alexander's most extreme treatment modifications may be paraphrased as the encouragement of acting in the transference in place of remembering.

These features have made him *cogitatio*, if not *persona, non grata* in certain psychoanalytic circles, and while it may be difficult for the outsider to know just who still acknowledges whom as a sibling in the psychoanalytic fraternity, there can be no doubt they all claim the same father. Freud thought enough of aggression, for example, to make it central in his scheme of things, and Alexander certainly does not share his later pessimism. Perhaps we may understand Alexander's ability to diverge boldly from orthodoxy if we compare his presidential address to the American Psychoanalytic Association (1938) with Rangell's, given a generation later (1961). For Alexander, psychoanalysis had already 'come of age' in 1938, whereas Rangell is just now able to grant it its majority and feels that its identity is still in jeopardy. Long ago Alexander could get on with things that finally seem possible to his colleagues.

The thorny issue of psychoanalysis versus psychotherapy has been an enormous drain on the talents of a productive group. If it remains contentious, it

should be settled by a controlled study in which divergent therapeutic approaches can be compared in terms of their efficacy, for the issue will not be resolved emotionally. Alexander's current researches into the therapy process are an admirable step in this direction; however, it must be noted that he appears to have already answered for himself the questions he poses in this research. Psychoanalysis faces the same dilemma that confronts the virgin—how to remain pure in a world of compromise. Both may be notably enriched by allowing *certain* compromises, as psychoanalysts surely ought to know, since purity, after all, is a state of mind.

Alexander appears as the champion of the unorthodox, but, one may ask, what orthodoxies remain? In theory, the ego-psychologists are unorthodox compared with the orthodoxy of forty years ago, and they have their own brand of optimism, albeit cautious. In practice, it is hard to believe that most present day psychoanalysts still regard themselves as blank screens, that they do not take quite scrupulous account of themselves as real people in therapy, or that they do not modify treatment when it is appropriate to do so. And Freud, of course, was the most unorthodox of all. If he were not, he could not have fashioned his theory to begin with, nor could he have altered it so conspicuously during the course of his lifetime. Although I have no wish to minimize what may appear to some as fundamental differences, it is conceivable that Alexander may be fighting a few straw men.

The foregoing should not detract from Alexander's great contributions. Apart from his value as an imaginative and courageous maverick, from which any science can profit, he remains a humanist and a scientist in the best sense of these words, and he bears a more than slight resemblance to a modern Renaissance man. Any psychologist, indeed any person of intelligence and curiosity, will be rewarded by his journey through this volume, and he will be in the company of an original mind and a vital personality.



## Personality, With Care

Ralph Mason Dreger

*Fundamentals of Personality: A Functional Psychology of Personality.* Philadelphia: J. B. Lippincott, 1962. Pp. xi + 388. \$6.95.

Reviewed by ALFRED B. HEILBRUN, JR.

*The author, Ralph Mason Dreger, received his PhD from the University of Southern California and presently is Professor at Jacksonville University and Director of the Duval County Child Guidance and Speech Correction Clinic. His past includes more than a decade as a Methodist Minister and a period of teaching at Florida State University. Alfred B. Heilbrun, the reviewer, received his PhD from the State University of Iowa, has spent two years on the staff of Veterans Administration Neuropsychiatric Hospitals, and has taught for six years in the Department of Psychology of The State University of Iowa where he is now an associate professor. He is an ABEPP diplomate in clinical psychology and has authored or co-authored about forty research or review articles on personality measurement and functioning.*

It is difficult to imagine a more arduous task than authoring a basic text on personality when you consider the mountain of research evidence, theory, and nonsense which has accumulated to this date. Dreger has approached his assignment in an erudite and scientifically conservative manner and has added a sufficient number of naturalistic and clinical observations to enhance readability for the beginning student. To begin with a summary statement, I was favorably impressed by this book, and the reader would do well to temper the coming and inevitable criticisms with this fact.

The general plan of the book is a commendable one in light of its intent as a basic text; the reader is given a short-course on the principles of the scientific method in psychology followed



by a survey of the more visible definitions and theories of personality which have been proposed. Of no less importance, the author gives considerable attention to the question of whether there is sufficient evidence of behavioral consistency in humans to warrant a science of personality (he attributes a narrow verdict to consistency).

The core of the book resides in Dreger's presentation of a schema for conceptualizing personality, a schema which is better described as 'fresh' rather than 'new.' Human biological and psychological functions are classed into four areas: drive or tension, control, societally-induced control, and role. The author acknowledges a psychoanalytic framework to his formulation with the time-honored trinity of id, ego, and superego functions corresponding to the first three classes and with social role functions added. The reviewer wonders why Dreger felt it necessary to develop his construct of personality in a Freudian mold, since his tension-control model has heuristic value of its own without a great deal of tribute due to psychoanalysis for inception or formal congruence.

The chapters on the control functions, which include perception, motility, "binding" tension, judgment, synthesizing, and conscience, sometimes sag under the weight of demonstrating regulatory significance to such a broad spectrum of habit systems. However, the research literature is liberally consulted in presenting his point-of-view, and after a while the thesis becomes a believable one. It should be noted in this regard that Dreger conducts himself impeccably throughout the book by continually reminding the reader that construing personality as he has represents nothing more than a way of making sense out of the fact that simple and complex behavioral consistency occurs at all. No purveyor of unobservable demons, he!

The increasing importance of role theory in understanding interpersonal behavior is recognized and role functions are given equal status to drive and control functions in Dreger's description of personality. A rather limited space is allotted to the contribution of social, cultural and situational phe-

nomena in shaping role behavior, but the author's propensity for illustrating his points by anecdote, excerpts from literary works, and the like usually serves to clarify what might otherwise be unclear.

THE REMAINDER of the book serves two purposes. For one, the phenomena subsumed under the four personality functions are considered in terms of two additional dimensions which cut across each. Consciousness and unconsciousness as a dimension are dealt with in a more behavioristic than Freudian manner, although considerable attention is paid to the clinical implications of varying accessibility of our own behavior to awareness. The other cross-classification of personality is in terms of the central versus peripheral (nonobservable versus observable) character of the personality functions.

The final section of the book is employed to present the reader with an overview of adult personality. What are the 'units' of personality? Can personality be meaningfully ordered from more specific traits to the most general styles of living? Probably because of his training at the University of Southern California, the image of J. P. Guilford and the method of factor analysis loom large in the delineation of traits presented as basic to human behavior. By my predilections, this represents an overemphasis on a still controversial method of analysis, yet criticism must be tempered by Dreger's admission that the dividends of factor analytic precision are still in the future. Giving it a place of importance in the book is an act of faith.

The virtues of this book reside in a readable style, a logical organization of materials, and the scientific conservatism of the author which yet does not cripple the expression of viewpoints. Its outstanding weakness is inherent in the subject matter chosen. Personality is a nebulous construct and does not lend itself readily to either scientific or literary examination. Dreger has done a good job in pushing water uphill.



## Psyche and Disabled Soma

James F. Garrett and Edna S. Levine  
(Eds.)

*Psychological Practices with the Physically Disabled.* New York: Columbia University Press, 1962. Pp. xi + 463. \$8.75.

*Reviewed by W. E. FORDYCE*

*James F. Garrett and Edna S. Levine, who collaborate here as editors have collaborated earlier as authors to produce Psychological Practices in Rehabilitation (in press). Both of them did their graduate work at NYU and both went on afterwards to become leaders in the field of rehabilitation. Garrett as assistant director of the Office of Vocational Rehabilitation of the Department of Health, Education and Welfare, Levine as associate research scientist at the New York State Psychiatric Institute. The reviewer, W. E. Fordyce, is a doctoral product of the University of Washington, who, after a number of years with the VA programs in the states of Minnesota and Washington, has returned to Seattle where in recent years he has concerned himself with problems of rehabilitation and where he is now in the Department of Physical Medicine and Rehabilitation of the University of Washington Medical School. He has worked extensively with the blind as well as with other kinds of disabled individuals.*

THE EDITORS of this book are a pair of senior instigators to the development of psychology's role in rehabilitation. Having helped psychology find a place on the rehabilitation team, they seek now to help future members know what to do. They succeed.

This book is a symposium of chapters setting forth many of the important kinds of information about "... some of the special 'disability' influences and problems in terms of (1) medical-physical aspects; (2) psychological implications both in regard to the indi-



vidual, as well as to family, community and sociocultural and vocational milieu; (3) special considerations in psychological appraisal, management, and in rehabilitation; and (4) suggestions for research as well as for improvement in psychological management and rehabilitation" (p. x). Each chapter is authored by a psychologist experienced in dealing with the selected kinds of disabilities discussed.

There is here a wealth of information for readers newly arrived at a depth consideration of non-medical aspects of disability and rehabilitation, but it is not, basically, a book of ideas, theories, concepts. New journeymen in all the rehabilitation specialties, and, perhaps, grandfather-clause rehabilitation professionals will find concrete information about disability influences and problems of management. Psychologists and their graduate students will find detailed discussions of methods of assessment and of problems specific to the wide range of disabilities considered.

This book reflects the inconsistencies and uncertainties of much of present day clinical psychology, in or out of rehabilitation. Despite a sequential outline approach followed well in nearly every chapter, the loose reins of the editors and the scatter in self-concepts of chapter authors is evident. Nearly every author has something to say about selection of psychological tests for assessment of patients with the particular disabilities they discuss. Seidenfeld in his chapter on arthritis and rheumatism—certainly one of the book's most informative and clearly written chapters—calls attention to the dubious validity of the projectives. He calls upon Eysenck and others for documentary support of his position. Several of the other authors hold somewhat less doubt. An extreme seems to have been approached in this regard by Madan who says (p. 274), "Since the emotional life of the patient (he is dealing with facial disfigurement) is all important, there should be a concentration on the projective techniques." This book may burn in Minnesota!

Cobb, in a perceptively written chapter, focuses on counseling problems relating to cancer patients and their families. Aside from this chapter devoted specifically to counseling or psy-

chotherapeutic problems, most of the authors address themselves on a limited basis to comments and recommendations on treatment procedures. Those who comment to such matters, with an exception to be noted in a moment, seem quick to recommend psychotherapy. Had they shared Seidenfeld's familiarity with the publications of Eysenck they might have made different recommendations. Shontz in his excellent discussion of problems of severe chronic illness points out the limitations imposed by resources of time and staff on the recommendation of psychotherapy. He points to the virtues of working through other team members such as the occupational therapist, etc., in striving for therapeutic objectives.

CENTRAL to all of this is the question of the role of psychologists in rehabilitation programs. There are excellent comments about working with and through fellow rehabilitation team members. Several authors call attention to the important contribution the psychologist can make in helping to orient other team members to psychological aspects of patient behavior. Concern is shown frequently for helping other team members to understand their impact on the patient. Given all of this, the collective effect of many of the chapters gives this reviewer a disappointing perception of psychology's clinical role in rehabilitation as that of collector of test-type facts and inferences. There seems to be too little concern with what might be paraphrased as a kind of middleman role somewhere among the complexities of behavior of patients and their families, the complexities of skills and interpersonal interactions of the rehabilitation team, and complex forces in the community.

Rehabilitation, in the eyes of the present reviewer, is best understood as a social process involving the inter-play of a wide range of forces and of people: patient, family, and staff. The heterogeneity of kinds of people, kinds of training, and professional self-concepts of rehabilitation team members is ever apparent in the process. These forces are immediate to the patient's rehabilitation. At a more general, and though frequently also specific, level for the

patient, is the inter-play of extramural forces such as agencies sponsoring patient services, employer resistance to hiring the disabled, and the generalized and common tendency of people to over-react to patients—protect, or sometimes to over-protect, sometimes to upset these patients. These forces too are part of psychological practices with the disabled. As Shontz points out, "the psychologist rarely has time available to do (treatment practices) . . ." (p. 436). Putting aside the issue of the efficacy of psychotherapy, it can be argued that a reason the psychologist has so little time is because of the need for his skills and services in the interplay of the forces noted. If this book is a primer on psychology's role in rehabilitation (which could have been a well deserved though not listed sub-title) more attention ought to have been given to these considerations.

The present reviewer's plea is not for consideration in a book such as this of any wide sampling of these and similar issues. The plea is for more preparation of the graduate student in psychology to anticipate better the basically social-economic character of the rehabilitation process.



*Newton, in his Principia, deduces from the observed motions of the heavenly bodies the fact that they attract one another according to a definite law . . . In his Principia, he confined himself to the demonstration and development of this great step in the science of the mutual actions of bodies. He says nothing about the means by which bodies are made to gravitate towards each other . . . With that wise moderation which is characteristic of his investigations, he distinguished such speculations from what he had established by observation and demonstration, and excluded from his Principia all mention of the cause of gravitation, reserving his thoughts on this subject for . . . the Opticks.*

—J. CLERK MAXWELL







#### CP AND RULES OF ENGLISH USAGE

**I**N 1775, Samuel Johnson, in commenting on his own labors on his dictionary, had this to say:

"It is the fate of those who toil at the lower employments of life, to be rather driven by the fear of evil, than attracted by the prospect of good . . .

Among these unhappy mortals is the writer of dictionaries; whom mankind have considered, not as the pupil, but the slave of science, the pioneer of literature, doomed only to remove rubbish and clear obstructions from the paths through which learning and genius press forward to conquest and glory, without bestowing a smile on the humble drudge that facilitates their progress. Every other author may aspire to praise; the lexicographer can only hope to escape reproach, and even this negative recompense has been yet granted to very few."

No editor of an APA journal feels, in his saner moments, quite so put upon as did Dr. Johnson and none would dare, before an audience of psychologists, express so nakedly his feelings about his function. But editors do worry and fret about the general usefulness of the editorial role, about ways both graceful and satisfying to serve as "slaves to science" and, at a lower level of anxiety, about ways to handle the multitude of niggling little problems that arise when they find themselves making suggestions, often on the basis of mere intuition, concerning the cherished sentences and paragraphs of a writer. In the face of such concerns, editors can be expected to do a variety of things—some sensible, some purely defensive, all perhaps understandable—to bring their efforts in line with their standards and their aspirations. One reaction is to seek for rules. Rules lend

a sense, if often a false one, of neatness, safety, propriety, legality. Rules that are explicit and widely accepted also help contain an editor's arbitrariness, for an author can cite such rules to an editor as easily as the other way around, and everyone gains protection from unbridled idiosyncrasy. Editorial rules, then, have both clinical and communicative advantages. But there is no truly authoritative handbook for editors, no general code and perhaps not even a body of workable common law. And even if there were, no editor would be completely happy about any given set of regulations. Each editor would like to formulate his own rules, for he has the feeling, justified or not, that his problems with his authors are unduplicated in the history of the written sentence. And he may be right; but right or not he still wants to make his own rules, for he must cultivate the double impression that his job is important and that he knows what he is doing. Perhaps, too, he has the forlorn hope that consistency cast into rules will reduce daily dissonances even with respect to nit-picking editorial practices. When an editor forms his rules, they may be quite explicit or may remain only semi-articulated; they may become exceptionless and rigid or they may be such as to give only general guidance; they may be very extensive or relatively sketchy. But rules do happen.

**I**T SEEMS probable that any journal editor will, in time, evolve a set of editorial rules for himself. These rules may become so elaborate, so rigid and so fusty that every edited writer may end up saying exactly the same thing in the same style about whatever subject is under consideration. When that day comes, of course, the given journal will die, or will discover that it has

long been dead. It will have no articles, no contributors, no readers. Only rules. Rules can kill. But a journal, including *CP*, still needs rules—sensible rules sensibly applied. And any journal, including *CP*, does have rules. *CP* does not have any highly codified or detailed rules, but it has some. For example, Professor Boring stated and adhered to the rule for *CP*<sub>1</sub> that reviewers should not diagnose or characterize or make inferences about the personal attributes or motives of an author, but must deal instead with what the author says. *CP*<sub>2</sub> happily adheres to this rule, and so do most of its reviewers. There are other rules presently in force, but in line with the necessity, fully explained earlier, that every editor work out his own rules. *CP*<sub>2</sub> has begun the job and has formulated at least one of its own. It is one of less significance than Boring's rule against *ad hominem*s, but it is a rule that can be of some general utility in dealing with a number of problems. The rule says:

*Writers and editors should concern themselves with questions of proper English usage; readers should not have to.*

The last clause in the rule carries its meat: it means, of course, that the reader should be free to follow the writer's meaning, and in his seeking for meaning the reader should not be jarred or irritated by usages that either obscure meaning or grate upon his sensibilities. And if both the reader and writer have a sense of the sound and rhythm of words, the reader should be allowed the uninterrupted, nonirritated enjoyment of flow and cadences and patterns. When a reader encounters a jarring usage he loses, at least momentarily, both the train of thought and the pleasure of following it. So, you writer and you editor, protect the reader from any unnecessary and unintentional jars. Do not, for example, use data as a singular noun. The word may soon become singular, through the repeated use of it as such by psychologists, but most readers still prefer data to be plural. Do not split infinitives, unless there is no discoverable way to express your meaning without the split, or unless as a matter of vital principle you need to



insult schoolmarm rules; many people have learned, and painfully, to avoid splitting infinitives and are disturbed when anyone else gets away with it. Do not string out adjectival nouns. Not only is this a way to create puzzles for your reader, but it's a way to make him come down with an interrupting worry about why you are not able to express the same meaning a little more clearly, a little more conventionally, by giving in to the established expectation that adjectives modify nouns. See that pronouns at the front of clauses or sentences have clearly defined referents; no reader should have to search through a preceding paragraph to find the meaning of a free-floating "this." However great the clarity achieved, do not use the same word three times in the same sentence; the reader will wonder, perhaps with empathetic embarrassment, about the size of your vocabulary. And so on, from the stuffy beginning to the stuffier end of books on English usage.

No rule or set of rules, however meticulously followed, will make poor writing into good writing. But whatever is the nature of good writing—and this is a matter lying far beyond the world of ironclad certainty—it involves containment, control, discipline; linguistic proprieties can be insulted only by those who are licensed by greatness or genius to depart from conventionalities. Expository writing, even for the great and the gifted, is probably more demanding of discipline than is any other kind. Rules are necessary. And they can help, within limits, improve the quality of writing.

The general rule under consideration here may bring a bit of discipline into the writing of sentences; and it may bring explication and justification into the editing of some of them. But it has a built-in weakness. It can be applied only on the basis of the author's or the editor's intuition about the jarring effect any given usage will produce on how many readers. And the usefulness of the rule still depends upon the rigor of its application. If one should go so far as to say that any trauma of any magnitude to the sensibilities of any reader is to be avoided, he is surely on the road not only to great dullness but

to spineless other-directedness. If the rule is not applied at all, the way is open for that syntactical anarchy that makes good communication impossible. Between the extremes of rigidity and laxity of application, the rule leaves room for an author to find clear expres-

sion and in his own style. He can still find ways to jar the reader if he wants to, but adherence to widely shared expectancies concerning usage can give the reader the good and non-distracting feeling that the writer, in Robert Frost's term, is "moving easy in harness."

## Biology Back in the Saddle?

Paul H. Hoch and Joseph Zubin (Eds.)

*The Future of Psychiatry.* New York: Grune & Stratton, 1962. Pp. xii + 271. \$8.75.

Reviewed by OSCAR A. PARSONS

*The team of Paul H. Hoch and Joseph Zubin, editorially responsible for the present volume, is well known to the readers of CP. The pair has edited fifteen volumes of a series based on events occurring at the meetings of the American Psychopathological Association. The reviewer, Oscar A. Parsons, now plays a triple role involving medical operation and a single one in the Psychology Department at the University of Oklahoma. In the Medical School he is Professor of Medical Psychology, Head of the Division of Behavioral Sciences in the Department of Psychiatry, Neurology and Behavioral Sciences and is Psychologist in Chief at the University Hospital. In the Department of Psychology he is a professor. He has focussed a great deal of his energy since 1959 on developing for medical students and for residents in psychiatry a teaching program in the behavioral sciences. His research interests include perceptual processes and information processing in brain damaged patients and in chronic schizophrenics.*

AT THE 50th annual meeting of the American Psychopathological Association, sixteen investigators and clinicians undertook the difficult task of predicting "things to come" in psychiatry. As in the psychiatric interview, the question "What are your future plans?" evoked varied responses ranging from relatively conservative to expansive and optimistic predictions. Psychologists will

find three aspects of the book of interest: 1) some recent advances in the biological aspects of behavior; 2) the place accorded to psychology in the future of psychiatry; and 3) the manifest divergence of views as to the psychiatrist of the future.

The dominant impression conveyed by this volume is that the future of psychiatry lies in tying behavior to the biology of the organism. While at first glance this may seem to reflect the specific interests of the members of the American Psychopathological Association, there seems little doubt that this is an adequate representation of trends in the field of psychiatry today. Certainly had such a book been written ten years ago it would have had much greater emphasis on possible contributions from psychoanalysis, experimental psychodynamics, and from interpersonal and communicational approaches to behavior. Psychodynamic theories are here tacitly accepted as important but limited. There is a spirit of optimism about unraveling the relationship between mind and brain at a level which will anchor psychodynamic concepts in physiology and neuropsychology.

Apparently by recognizing that prognostications are meaningful only when compared to a base line, most of the authors provide an overview of some recent developments in their field. On the biological side such consideration is given to neurochemistry, neurophysiology, pharmacology, physiology, biochemistry, genetics, maturational and



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***CHILDREN’S DRAWINGS AS MEASURES OF  
INTELLECTUAL MATURITY: A Revision and Extension  
of the Goodenough Draw-a-Man Test***

By DALE B. HARRIS, *Pennsylvania State University*. A complete revision of the earlier text, **Measurement of Intelligence by Drawings**, by Florence L. Goodenough, including a restandardization of the Goodenough Draw-a-Man scale, a new standardization of a Draw-a-Woman scale, an extensive survey of the literature relating to children’s drawings, results of hitherto unpublished studies of personal and cultural influences on drawings, and a complete test manual. With supplementary materials. ■ Publication: May



general biological factors. Of particular relevance to the interests of many psychologists are papers by Marrazzi, Greenblatt and Rioch. Marrazzi focuses on the import of disruption of homeostasis at the synaptic level in the CNS for understanding psychoses and he imaginatively indicates directions that psychopharmacological research in excitatory and inhibitory processes is likely to follow. A sense of enthusiasm and excitement is conveyed by Greenblatt's citation of a number of areas (e.g. classical and operant conditioning, motivation, emotion, psychotherapy, social behavior) where bridges between biologic aspects and behavior patterns are being constructed. Rioch, in a much more cautious vein, declares that the phenomena within psychiatry are social phenomena and that only occasionally will psychologic data be relevant to psychiatric problems.

Of the three papers which are primarily concerned with psychologic approaches, Rado's most clearly provides a conceptual framework for synthesis of psychodynamics with biological functions. In contrast to Rioch, he sees the choice of psychodynamic theories ultimately being dictated "behavior physiologists." Appel finds psychoanalysis facing many problems and states that it cannot afford to remain tied to "repetition of intellectual formulae." Meehl's indictment of the overgeneralization of the concept of purpose in contemporary psychodynamics is challenging to all who profess allegiance to, and act upon, motivationally based theories of behavior.

A miscellany of other papers includes discussion of the future of epidemiological studies, contributions of psychiatry to education, and social organization of psychiatric services. The tenor of these pages suggests a disenchantment with individual psychotherapy as an answer to the larger problems of mental health and a growing awareness that psychiatry must become attuned to the social matrix and social philosophy within which it operates. A wise and scholarly paper by Nolan D. C. Lewis places the present and future of psychiatry in its historical perspective. His insistence on the need to avoid dogma, develop curiosity and to search

for answers to fundamental questions may well be applied to any discipline.

**W**HAT part does psychology play in the future of psychiatry? The present authors expect contributions from the experimental laboratories in the areas of learning, problem solving, memory and physiological psychology. Special emphasis is accorded studies involving classical and operant conditioning; indeed the presidential address by Gantt is essentially a tribute to Pavlov and his methods. In contrast, social psychology and personality development receive only passing reference. Recent experimental developments in areas such as person perception, impression formation, cognitive dissonance, determinants of yielding and group behavior, attitude change, parent-child interaction, and the like, receive scant attention. There are no papers directly concerned with a current issue of some importance to psychiatrists and psychologists i.e., who should do psychotherapy? However, the impression one gains from several authors is that as knowledge in this area accrues, responsibilities will be assumed by those individuals whose training and background best fit them to cope with certain types of therapeutic problems. Zubin writes "The battle for the possession of psychotherapy will persist only as long as ignorance about the nature of therapy and its efficacy remain".

The conclusion of most authors is that psychiatry's future is bright but there is disagreement about the attributes of the psychiatrist in the Brave New World. Will he be Marrazzi's "practicing clinical experimentalist," the socially oriented general psychiatric practitioner of Gruenberg, the mental health educator described by Funkenstein and Farnsworth, or the computer-guided pharmaco-psychiatrist feared by Greenblatt? While all these predictions will undoubtedly be off the target by an appreciable margin, the stimulus value of such an effort is impressive. Indeed this reviewer could not help but conclude that a volume devoted to the 'future of clinical psychology' would have many provocative similarities and differences.

## Narrow Training for Broad Functioning

Dorothy Mereness and Louis J. Karnosh

*Essentials of Psychiatric Nursing.*  
6th Edition. St. Louis: C. V.  
Mosby, 1962. Pp. 312. \$5.50.

Reviewed by W. K. RIGBY

*The first of the authors, Dorothy Mereness, is a widely experienced and broadly educated nurse. The second author, Dr. Karnosh, is a now retired psychiatrist with an equally broad and deep training in his field. W. K. Rigby, the reviewer, obtained his PhD at the University of Pittsburgh and has had a long and productive career, both in service and administrative capacities in the Veterans Administration program. Presently he is Chief of Psychology in the VA area office in St. Louis. In the past he has served as a lecturer at Washington University and presently holds an appointment as Clinical Research Associate in its graduate school. An ABEPP Diplomate, he is now chairman of the board of examiners of the Missouri Psychological Association.*

**T**HIS WORK was written as a textbook for students of nursing "who are having an initial experience with psychiatrically ill patients," and its aim is that of helping "the student to understand and function effectively in the therapeutic role of the psychiatric nurse." The sixth edition, it updates the material in the 1958 edition, shows a considerable change and improvement in format, and some reduction in length. The book has evolved vastly from the original edition by Karnosh and Edith B. Gage, published in 1940.

The material presented is written in a clear, easily understandable style and reflects the long and rich experience of the authors in direct work with patients. There are excellent passages concerning attitudes and roles of the nurse in a psychiatric setting and a great many practical recommendations and suggestions for making the nurse's efforts ef-



fective and productive. Organization of material is in three sections emphasizing the nurse's role and self understanding, understanding of patients, and an overview of some pertinent general aspects of psychiatry. The first section is likely to be the most interesting and perhaps the most useful for student nurses. It includes an excellent chapter, worthy of further emphasis and expansion, on nursing care for the physically ill. A weaker part of the book is the material on personality theory. While the need for brevity no doubt contributes to this result, a good deal of this material is not likely to be particularly meaningful or helpful to student nurses.

A DISAPPOINTING feature is the relatively narrow scope and somewhat too concentrated focus on nursing itself. At the end of each chapter there is a list of supplemental references which for the most part point to work of other psychiatric nurses, and little attention is given to contributions of other groups or to the potentialities of research. The book will indicate to student nurses that the treatment team is made up of a psychiatrist and a nurse; other disciplines are given very superficial attention. And perhaps a too rosy picture of psychiatric supervision is presented. Not all psychiatric institutions are completely staffed by the highly trained and sophisticated personnel which this book may lead students to expect.

As an introductory text, this book has much to commend it, the above limitations notwithstanding. Student nurses will find it readable, enlightening, and helpful in their everyday work. The first section could be reviewed with profit by members of co-professional groups who work with nurses in direct patient care. The many-faceted role of the nurse in a psychiatric setting is continuously evolving and requires continued evaluation and continual efforts toward progress. Advanced courses and in-service training are needed to supplement any beginning text, and frequent revisions of all texts will be necessary. This one has evolved well and, despite some limitations in breadth, is a valuable addition to the literature.

## Research Strategies of a Master

Samuel A. Stouffer

*Social Research to Test Ideas.* New York: Free Press of Glencoe, 1962. Pp. ix + 314. \$8.50.

Reviewed by KURT W. BACK

*Samuel A. Stouffer was, at the time of his death in 1960, professor in Harvard's Department of Social Relations and Director of that department's laboratory. The recipient of many honors from his colleagues in sociology and other disciplines, he was perhaps best known to psychologists for his role as principal author of the four volume work American Sociology in World War II, a series including the famous American Soldier (1949-50). Later he produced Communism, Conformity and Civil Liberties (1955). The reviewer, Kurt Back, as reported recently in these pages (CP, 1963), and as will be reported again in the subsequent issue in which CP reviews his 1962 book, Slums Projects and People: Social Psychological Problems of Relocation in Puerto Rico, is Professor of Sociology and Psychiatry at Duke University. He received his PhD in Group Psychology at M.I.T. and there, as well as later at the University of Michigan, he was associated with the Research Center for Group Dynamics. He is now involved in a study of work and retirement. CP will probably be hearing more about that, too.*

THE PAPERS in this book come from more than thirty years of research effort. They were collected by the author as a demonstration of how the social scientist, working with demographic and survey data, can reach demonstrable propositions. Short introductory pieces for the several sections explain the kind of setting and materials which led to a particular mode of analysis. The book is addressed partly to the general reader to show, without exaggerated claims, what social science can do, but it will be even more

valuable for the student, to inspire him to apply the same precision with the same ingenuity. The author died just as the compilation of the papers was completed and the volume was completed as a tribute to Samuel Stouffer by Paul Lazarsfeld, who also provided a discerning introduction presenting Stouffer's like work as practically co-extensive with the rise of empirical social science.

Most of the book is given to examples of empirical methods in different fields of sociology and social psychology. However, the best introduction to the general approach are two programmatic papers, "Some Observations on Study Design," and "Quantitative Methods in the Study of Race Relations." The first is a plea that sociology not become dull journalism, that it not settle for few data and much interpretation presented in an academic jargon. He discusses the ideal design to test a proposition, with data collected before and after the introduction of the critical variable, and having experimental and control groups; and he shows the consequences of deviation from this complete system. In this discussion the moderation of Stouffer's approach becomes clear. He derides the studies based only on "after" data in the experimental condition and that supply the other three cells through "brilliant analysis" which fill pages of social science journals. On the other hand, he realizes that not all research can conform to the canons of experimental design and that, through forethought and patchwork, some valuable conclusions can still be drawn. The second essay describes the application of these standards to research on race relations. Where do we have sufficient information to



make firm statements, where should we collect more, where is basic research needed? He mentions, for instance, that the consequences of Negro migration to the North have never been systematically studied, that the methodology of interviewing may give important clues to understanding the position of the Negro, that controlled laboratory studies of the main variables can feed into the research in natural situations, and that the predictable social change following the Supreme Court decision on school integration gives a natural laboratory of different communities to test different propositions. This program shows concretely what Stouffer means by a painstaking empirical attack on a problem. The first ten chapters of the book show some instances of this approach on limited objectives.

One of the striking features of all these research reports is their rigorous parsimony. Theory is not invoked beyond the relations shown in the data. Neither are statistical techniques used beyond necessity; the main approach is tabular analysis. Both these restraints combine to make for unpretentious and pleasant language. Both these features are marks of self-restraint, so Stouffer shows that he is an able theorist and statistician. As to the latter, some of the later chapters show some innovations which he has made in statistical technique. Characteristically, the most valuable is not his early work with partial correlation and case-study prediction, but his development of a simple scaling technique—the H technique—which permits the use of the maximum amount of data while adhering to the standards of Guttman scaling.

THE STUDIES themselves range over a variety of topics, from migration and fertility to role conflict and relative deprivation. They are not presented for their substantive interest, but to demonstrate similarities and differences in the method of reasoning. The differences depend on the type of material and the purpose, which determine the amount of elaboration of the data. If the use is an immediate decision, arrangement of the weight of the evidence from dif-

ferent sources may be best, as in advising about the possible changes in the point system of discharge after World War II. On the other extreme we have a precise mathematical model of migration, based on twenty years work of refinement and testing the model of intervening opportunities and competing migrants. The fitting of the style of presentation to the purpose at hand is again an example of elegant parsimony.

The common core of the approach is equally striking. One can almost outline the procedure step by step. First is the delineation of a question which can derive from several sources, practical and theoretical (note that the title of the book is *Social Research to Test Ideas*, not to "test theories"). Then comes the ascertaining of specific operational consequences of this idea. Next step is the search of data which can show these consequences. A study may be designed especially for this purpose. But equally important is the use of existing data, and even the study which is just being conducted. After all, the scientist does not stop thinking after he has stated his hypotheses and begun on his research operations (a position which rigorous adherence to the principle of hypothesis-testing may lead us to take), but he may get fresh ideas from the very data which he is collecting. Next comes the evaluation of the results and the refinement of the hypotheses and the search for new data to test this refinement. And there is no final step.

ONE may doubt whether the approach shown here is not too much of a reaction against the common theorizing, the "dull journalism" discussed in the paper on *Some Observations on Study Design*. Stouffer chooses as his motto Shakespeare's dialogue between Glendower and Hotspur: To Glendower's "I can call spirits from the vasty deep", Hotspur replies: "Why, so can I or so can any man; But will they come when you do call for them?" It may be well to remember that neither the theorist nor the empiricist was the victor, but Prince Hal, who could combine many approaches in the pursuit of his goal.

However, strictures of this kind should not obscure the real merit of this book. It does communicate the excitement of data analysis which is the core of social research, but slighted in most textbooks and courses. And it does so by giving the privilege of seeing a master at work. While the student is introduced to the art of reasoning from cross-tabulations, of questioning them and of designing further cross-tabulation and additional data to answer his questions, the expert will find delight in many examples of brilliance in the deceptive simplicity of research method. One example must suffice. It comes from the research memorandum on the family in the depression and is the more striking as it uses only available data. Stouffer reasons that the depression accelerated the loss of social function of the family. What would be objective indicators of this change? Stouffer finds three, which can be defined without relation to attitude measurements: marriage viewed only as legalization of sexual intercourse, called impulsive marriage; marriage across other social groupings, such as religion, nationality and class, called mixed marriage; and pregnancy before marriage, called forced marriage. The rate of mixed marriage can be obtained in some places from the tabulation of data given on marriage certificates; the rate of forced marriage by comparing birth and marriage records, compiling births with seven months of marriage. But how to define impulsiveness? Stouffer reasons cogently for three indicators: marriage outside the bride's and groom's home; marriage by a civil officer; divorce or separation within five years. While it is possible to give reasons why each of these indicators may be due partially to other causes, trends in all three give a reasonable indication of changes in impulsive marriages. Collection of data from Australia, Canada and several of the United States show consistent evidence on this question. This is just one example of ingenuity needed to test one armchair hypothesis.

The emphasis on rigorous empirical research is combined with a high sense of the responsibility of the social scientist and several essays, especially "So-





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## *New and Recent Texts*

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### 1963 Publications

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#### **DYNAMICS OF MENTAL HEALTH: The Psychology of Personal and Social Adjustment**

By James M. Sawrey and Charles W. Telford  
San Jose State College

This new text presents the development of adaptive behavior within the framework of learning and motivational concepts. The text provides sufficient descriptive material, at the outset, to serve as basic groundwork for understanding the dynamics of behavior. The authors include a systematic presentation of the adjustment processes, both normal and abnormal by means of the concepts of learning and motivation. The text is consistent in its theoretical approach and strongly emphasizes research findings.

#### **READINGS IN THE SOCIAL PSYCHOLOGY OF EDUCATION**

By W. W. Charters, Jr., Washington University and  
N. L. Gage, Stanford University

This unique new text of readings is the first book devoted explicitly to the emerging field of the social psychology of education. It has been sponsored by the Society for the Psychological Study of Social Issues, a division of the American Psychological Association. The Society also selected the editors. The readings included were chosen for theoretical soundness and readability, as examples of the empirically grounded application of social psychology to educational issues. The text may be appropriate for a variety of courses.

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### Other Recent Texts

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#### **THE CAUSES OF BEHAVIOR: Readings in Child Development and Educational Psychology**

By Judy F. Rosenblith, Massachusetts General Hospital  
and Wesley Allinsmith, University of Cincinnati

This 1962 text has been lauded as "the best collection of readings in psychology published in at least a decade." One of the most notable aspects of this comprehensive text is the recency of articles. Approximately one-half of the selections appeared originally in 1958 or subsequently, some as recently as 1961. Among the selections are readings by such eminent authorities as B. F. Skinner, Robert Sears, Jerome Bruner, David Ausubel, Samuel Kirk and Leona Tyler.

#### **PSYCHOLOGY**

By Allen Calvin, Frank McGuigan, Charles Hanley, Michael Scriven, James Gallagher, and James McConnell

This volume brings together the writings of a highly select group of specialists. The entire book has been carefully and thoroughly edited by Dr. Calvin to maintain a balance and continuity of style and content throughout.

#### **HUMAN DEVELOPMENT IN WESTERN CULTURE**

By Harold Bernard, Oregon State System of Higher  
Education and The University of Oregon

The text examines human development from multiple, rather than single (experimental, descriptive, or psychological) approaches. Emphasis is on the converging of psychological, cultural and individual aspects to show the global nature of development. The concept of developmental tasks which considers age, culture, physical, and psychological aspects of growth, provides the avenue by which the multiple approaches are presented.

#### **Student Workbook to Accompany PSYCHOLOGY**

By Paul J. Woods

This study guide is concerned entirely with the task of helping the student to learn and understand the material in the text. The book has two sections for each chapter; (1) an open-ended outline which is to be filled in by the student, (2) self-test multiple-choice questions.



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ciology and the Strategy of Social Science" reflect this concern. It is well to be reminded in a book on methods that commitment to social values goes well with rigorous technique, and Stouffer is a prime example. Besides its usefulness for researchers and students, this book is a fitting memorial to a great man.

## Italy's Delinquents

**Gino Faustini.** With the collaboration of Maria Teresa Conte and Santina Cortellesi

*Aumento o Trasformazione Nella Delinquenza Minorile?* Roma, Italy: Scuola per la formazione del personale per la rieducazione dei minorenni, 1962. Pp. 174.

*Reviewed by* PIER ANGELO ACHILLE

*Gino Faustini, the principal author, is a Consultant of the Department of Justice of the government of Italy and also a professor in a school for specialized teachers and educators of juvenile delinquents. Neither CP nor its reviewer could get information on his present collaborators. Pier Angelo Achille, the reviewer, is a senior resident at the Institute of Psychology at Catholic University in Milan, but at present holds the position of Assistant en Recherche at the University of Montreal, where he is conducting, under the direction of Father Noel Mailloux, O.P., a two-year study on the methods of treatment and reeducation of juvenile offenders.*

**H**AS juvenile delinquency decreased in Italy or have the patterns of Italian delinquency changed? The authors of the present volume try to reply to this question. From the beginning of the century to the present, statistics suggest a decrease at the rate of 3.5 per cent per year. In the same period the ratio between offenses against property and other offenses has changed, suggesting also a change in pattern. Larceny-

theft remains the most frequent offense but its relative percentage in the total occurrence of offenses is noticeably diminished.

A sharp analysis of all available statistical data is the method adopted by the authors. The Italian Code considers juvenile offenders young people between fourteen and eighteen who commit violations of established law. Offenses are grouped in four major categories of the Criminal Code, i.e. offenses against property, against family and morality, against persons and life, against the State and administration of justice. Statistical figures refer to young offenders recognized by a court to have committed a violation in one of the four areas. In the present volume a juvenile offender is a young person condemned by a court. Major and minor offenses are judged on a triple degree basis. Major or general courts judge major crimes or delicts. Minor or trivial offenses, namely "contraventions" in Italian terminology, are referred to correctional judges. Juvenile courts have been established since 1934 and deal with general matters of judging and rehabilitating juvenile offenders.

The number of youths condemned in major courts has noticeably decreased whereas there has been a marked increase in the occurrence of trivial offenses. This observation would tend to suggest that juvenile delinquents are possibly more inclined to behave in a more generalized antisocial manner than to commit definite major crimes.

**I**N the study of the general decrease in juvenile delinquency, the authors examine the disturbing effects of war and the geographical distribution of juvenile delinquency. It might have been assumed that the war was a disturbing factor on the younger generation of that period. Nevertheless indirect disturbing effects on those children born during the war might also have been expected during a later period. It is impossible to reconstruct a real situation from fragmentary records and inadequate statistics reported during the war, when disorganization was prevalent in civil life and subsequent division of the State made regular administration of justice

impossible. On examining the long term disturbing effects, an interesting fact comes to light; the increased rate of juvenile delinquency apparent in the years following World War I is completely absent in corresponding years following World War II.

On the whole, the study of the geographical distribution of juvenile delinquency confirms the decreasing trends. A general examination of available data shows a sharply defined decrease of delinquency in agricultural and underdeveloped districts in the South and a more regular diminution in industrial and more economically developed districts in the North. More traditional juvenile offenses such as larceny-thefts have diminished everywhere. Crimes against persons and life, especially in connection with traffic law violations, have increased in more developed regions. There is no correspondence between geographical distribution of general delinquency and juvenile delinquency.

General legal classifications on which reported statistics are based do not permit of reliable criminological conclusions. A large variety of motivations can be implied under the same legal classifications. The perusal of the volume brings to mind some intriguing questions, beyond the scope of statistical and legal bounds. How can the steady decrease in the incidence of juvenile delinquency records be explained? What influences have contributed to the limitation of the juvenile delinquency rate in recent years? Despite the absence of major manifestations of delinquency in young people, is it not possible that the war has had disturbing effects on other less observable aspects of behavior? What economic, sociological and general cultural factors underlie the particular distribution and patterns in juvenile delinquency? These and other problems require a complementary explanation from related disciplines interested in juvenile delinquency, namely psychology and sociology. In a comprehensive presentation the reviewed volume puts at the disposal of students interested in Italian juvenile delinquency a considerable sum of data, hitherto unavailable.



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# Indians and Ink Blots

Gardner Lindzey

*Projective Techniques and Cross-Cultural Research.* New York: Appleton-Century-Crofts, 1962. Pp. ix + 339. \$6.00.

Reviewed by BERT KAPLAN

The author, Gardner Lindzey, has done and continues to do a number of things at least as important as his consulting for CP. He is the sort of person to whom one refers books that fall between or across the usual substantive categories. A product of Harvard's Department of Social Relations, he is well known as a social psychologist, as a personality theorist and as a professor of psychology at the University of Minnesota. The present book, one of a number he has brought to the light of day, is an outcome of a fellowship year at the Center for Advanced Studies in the Behavioral Sciences. The reviewer is the Bert Kaplan who edited the recent book *Studying Personality Cross-Culturally* (CP, June 1962, 7, 210-212). He is likewise the Bert Kaplan who received his PhD at Harvard, who has been in the psychology department at the University of Kansas since 1953, and who has done a number of valuable things about his interest in cross cultural affairs, including a *Microcard* publication, *Primary Records in Culture and Personality*. In the same area are coming other publications to be based on a just completed three year study of mental illness among the Navaho. On the way also is a book, *The Inner World of Mental Illness*, a volume presenting first person accounts of the experience of mental illness. In the fall he is joining Rice University as Chairman of its new Psychology Department.

IT HAS become abundantly clear to most social scientists in recent years that personality processes constitute a



GARDNER LINDZEY AND FRIENDS

vital link in the functioning of socio-cultural systems. It has thus been more than a passing fad that a considerable body of research has been concerned with describing personality processes in a variety of literate and nonliterate societies in every part of the world. The question of how such studies are to be conducted constitutes one of the most interesting challenges that faces psychology today. Projective techniques have played perhaps the major role to date, a fact which because of continuing doubts about their validity in our own culture and even greater doubts about their applicability in radically different cultural settings, has led to a certain amount of concern. It was perhaps a consideration of this kind that led the Committee on Social Behavior of the Social Science Research Council to invite Gardner Lindzey to undertake a critical survey of the extensive dependence on these techniques in cross-cultural research.

Dr. Lindzey has provided what in many respects is a classical stocktaking; the only limitation to my enthusiasm is that his critical comments have been confined mostly to methodological issues. With wisdom, thoroughness and exemplary fairness he has reviewed the bulk of the significant work in the field, given an account of methods and findings and weighed values and faults. This is undoubtedly the best methodological review that these studies have received, and it is an invaluable resource for anyone planning cross-cultural research of any kind.

In preparing the groundwork for his critique, Dr. Lindzey has, incidentally, produced one of our most valuable introductions to the projective techniques themselves. Covering a span of 150 pages, his four preliminary chapters constitute both an excellent introduction to the study of projective techniques and a very sophisticated treatment of projective technique theory for advanced workers. This book then is destined to join the small handful of works that contribute significantly to the clarification of projective theory and practice.

THE MAIN focus of the book, however, is the critique of the cross-cultural use of projective techniques. In summarizing what he finds wrong with this body of research, Dr. Lindzey describes the following "modal flaws": 1) the doubtful independence of many of the personality inferences; 2) the lack of objectivity in relating projective technique to ethnological sources; 3) a peculiar failure of anthropologists to provide a full description of the circumstances under which the tests were administered; 4) a failure to explore the possible contribution of nonpersonality factors; 5) an apparent unawareness of the examiner's influence on the test performance; 6) a failure to select well-matched samples where cultural groups are being compared; 7) the tendency to take group averages and treat them as descriptive of the group as a whole; and perhaps most damaging of all, 10) the fact that there has apparently been little accumulation of sophistication and wisdom in carrying out such studies. Taken together these findings constitute a



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Appleton-Century-Crofts is pleased to announce that the first Century Psychology Series Award for a distinguished manuscript in psychology has been presented to

**DR. BERNARD RIMLAND of San Diego, California**

for his study of Kanner's syndrome of apparent autism. In this unusually significant monograph, which will be published probably in the summer of 1963, Dr. Rimland reviews early infantile autism, proposes a theory which accounts for early infantile autism as the consequence of a specific cognitive defect, and finally enlarges the theory by showing that the assumed brain damage is consistent with a broad range of findings in the fields of normal and abnormal personality and intelligence.

Dr. Rimland is the Director of the Personnel Measurement Research Department of the U. S. Naval Personnel Research Activity in San Diego.

Appleton-Century-Crofts, publisher of The Century Psychology Series, edited by Richard M. Elliott, Kenneth MacCorquodale, and Gardner Lindzey, offers an annual prize of \$1500 in cash and a favorable contract for publication to the author whose manuscript provides a significant original contribution to the field of psychology. The deadline for submitting entries for the 1963 award is October 1. Questions concerning the award or manuscript requirements should be sent directly to the publisher.

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damaging criticism of the whole body of research, a damage which is not mitigated by Lindzey's corresponding list of contributions.

As someone identified with cross-cultural personality research I would like to be able to controvert Dr. Lindzey's critique and defend the legitimacy of what has been done. In all honesty, however, I must say that his points seem to me to be essentially just and correct. If his demands are occasionally contradictory, as when he seems to ask for both independence from contamination by *and* full consideration of relevant ethnographic materials, or occasionally somewhat rigid as when he asks people to produce materials or analyses that are irrelevant to their particular problems, there can be little doubt that his charges are substantially correct.

In mitigation it can be claimed that some of these problems are inherent in the use of projective techniques anywhere, something Dr. Lindzey acknowledges, and that other problems have to do with the fact that cross-cultural personality research has been done largely by anthropologists who are relatively untrained in the more rigorous criteria of psychological research and, more significantly, are professionally unequipped to deal with the extremely difficult problems encountered in this kind of research. We are still awaiting the serious entry of the psychologist into this field and while Dr. Lindzey's critique correctly represents the psychologist's understanding of all that is wrong with what has been going on, it also represents our failure thus far to make any significant positive contribution to the solution of problems. We can expect the anthropologist to do more careful research work, but it is not reasonable that we expect him to solve our basic problems for us.



*We do not worry about being respected in the towns through which we pass. But if we are going to remain in one for a certain time, we do worry. How long does this have to be?*

—PASCAL



## Some Psyche for the Soma

A. H. Chapman

*Management of Emotional Disorders: A Manual for Physicians.* Philadelphia: J. B. Lippincott, 1962. Pp. vii + 259. \$8.50.

Reviewed by DUANE DENNEY

*The author, A. H. Chapman, is a physician who obtained both his undergraduate and medical degrees at Yale. He had psychiatric training at Washington University School of Medicine in St. Louis, and has published clinical articles on a wide variety of psychiatric subjects. Presently, among other things, he is Assistant Professor of Psychiatry at the University of Kansas School of Medicine. The reviewer, Duane Denney, is also a psychiatrist, now functioning as an instructor at the University of Oregon Medical School. His clinical interests have involved him primarily with psychiatric consultation services to the general hospital. His intellectual and research interests range from electrophysiology to social psychology. At the moment, in addition to teaching, he describes himself as engaging "in various pokings and probings of the thalamus and cortex of felix domesticus in an attempt to identify some of the mechanisms involved in classical conditioning."*

DR. CHAPMAN'S book should prove useful to all practicing physicians and to those behavioral scientists and other allied professionals who are interested in the role of psychological factors in illness. The author obviously found it necessary to limit the depth of discussion of many specific topics, but succeeded in his principal aim of helping the "...busy physician who is treating many patients," and to "stick to concrete suggestions and specific advice."

The book, in keeping with its subject, is extremely broad in its coverage. The

first and largest portion of the discussion is devoted to the treatment of the non-psychotic disorders as defined in the American Psychiatric Association's *Standard Nomenclature of Diseases*. Little attempt is made to discuss psychodynamic considerations except in those few areas where the physician is advised to attempt some exploratory psychotherapy. Even here the author wisely avoids psychiatric jargon and needlessly complicated formulations.

His discussion of the levels of dysfunction in psychosomatic illness is a good example of a simple model which not only helps the physician and the patient understand current thinking about these disorders, but almost automatically suggests a rational and often simple treatment program. For instance, peptic ulcer may be considered a disease which can be understood as a dysfunction at any one or all of six different levels: (1) the breakdown of the gastric mucosa in the presence of chronic hyperemia, hypermotility, and hypersecretion, (2) increased activity in the vagus nerve which produces changes in the gastric mucosa, (3) a dysfunction produced at the level of the brain stem where various autonomic functions are coordinated, (4) a dysfunction at the level of the symbolic and integrative activities of the central nervous system, (5) difficulties at the level of the interpersonal transactions between the patient and important persons in his life, or (6) distressing personality problems in the lives of important other persons in his interpersonal environment.

The last part of the book represents an attempt to help the physician with the enormous number of problems



which inevitably come to his attention but do not fit any particular psychiatric diagnostic category. Such problems include marital discord, sexual incompatibility, childhood and adolescent behavior problems, and difficulties inherent in aging. In this portion of the book the author has inserted also some comments on the types of psychiatric orientation usually encountered among psychiatric consultants, and some comments on interviewing technique. It is inevitable, because of the range of this discussion, that the book loses some of the succinct and clearly utilitarian style that marks the initial chapters, but the author's recommendations remain plausible and practical. For example, it is candidly suggested that, "... inadequate spankings are worse than none at all... An effective spanking should be administered on the bare buttocks with a short hard object... such as a hair brush... The child can be turned over one knee while the parent's other leg is used to clamp the child's struggling legs down... The spanking should be given with an attitude of firm execution of justice, with perhaps a touch of justified indignation."

One deficiency in the book concerns the discussion of the role of allied professionals (clinical psychologists, social workers, public health nurses, physical therapists, vocational rehabilitation counselors, legal aid advisors, and many others). They are mentioned only briefly as helpful in the rehabilitation of the mentally ill hospitalized patient. There is a considerable body of evidence to indicate that these groups can and must play a large role in the treatment of non-hospitalized patient if the goals of the Joint Commission on Mental Illness and Health are to be realized. It is time for the practitioner who is concerned for a large segment of the psychiatrically disabled population to begin working with these professional groups and learning from them.

Certain portions of the book may appear condescending to a number of physician readers. Examples of points which are self-evident to most physicians include statements such as "the deeper the depression, the more probable is a suicide attempt," "it is better to say that the 'patient has a problem

with alcohol' than to argue with him about whether or not he is an alcoholic," or that "when confronted with an organic brain disorder of unclear etiology, the physician should explore the possibility of exposure to medications, patent medical remedies, household cleaning substances, heavy metals, industrial fumes, etc."

Since the book will undoubtedly be read by physicians, it will be of interest to those psychologists and other behavioral scientists who are interested in the ways that emphasis on psychosocial factors in illness and health are being disseminated throughout general medicine.

## Many Gallic Elephants

Serge Moscovici

*La Psychanalyse, Son Image et Son Public: Étude Sur la Représentation Sociale de la Psychanalyse.* Paris, Presses Universitaires de France. 1961. Pp. xi + 650. 20 NF.

Reviewed by BOHDAN ZAWADZKI

The author, Serge Moscovici, "Chargé des Recherches au Centre Nationale de Recherche Scientifique," is a French social psychologist who since 1953 has been publishing in the *Bulletin de Psychologie* and *L'Année Psychologique* important articles on methodology of research in public opinion and attitude scales. The reviewer, Bohdan Zawadzki, is associate professor of Psychology at City College of New York, and was introduced in the October, 1961 issue of CP, as a man widely traveled and both broadly and deeply educated.

THIS book is good news for those concerned with the fate of European psychology. It testifies eloquently to the vigorous revitalization of social psychology in France.

This new volume in the *Bibliothèque de Psychanalyse et de Psychologie Clinique*, edited by Daniel Lagache,

professor at the Sorbonne, has an ambitious purpose: to open a new area of research by applying the concepts and methods of empirical social psychology to the problems of sociology of knowledge.

The central problem of the study is: "how does a science become transformed into its public image?"

To answer this question the author and his staff, after a pilot study, have conducted a public opinion poll in which 2265 persons answered at least fourteen questions each. The respondents constituted a representative sample of the Parisian population and several specially selected samples (professionals, university students, pupils of vocational schools, middle class people, and workers) stratified according to suitable criteria.

The questions asked aimed at disclosing 1. the degree of acquaintance with psychoanalysis and 2. the variety of evaluations with regard to several aspects of psychoanalysis. They included questions such as: "According to you, what is psychoanalysis?", "What does one tell his analyst?", "Which practice does psychoanalysis resemble most: conversation, confession, suggestion, narcoanalysis or hypnosis?", "Can analysis change personality, for good or for bad?", "How effective is psychoanalysis?", "Would you undergo analysis?", "What kind of people resort to it?", "Do more men or women resort to psychoanalysis?" and so on.

The major factual findings are presented on the whole in a clear fashion; whenever group differences are stated, the probability of null hypothesis is duly indicated. (This is still a rare novelty in most European psychological writings). The findings are not surprising: they reflect a state of utter confusion in the public's minds, all degrees of misinformation, and a wide variety of reasons for either acceptance or rejection of the doctrine and its applications. To the American reader one finding is of special interest: the great frequency with which psychoanalysis is regarded as an American invention and importation, harmful, because it serves the purpose of adjusting the patient to American way of life which is energetically rejected.



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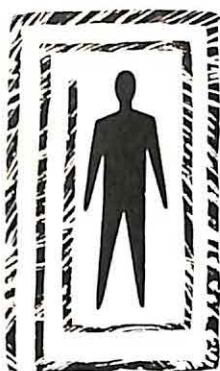
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The second part of the book is based on the content analysis of 1451 articles on psychoanalysis which appeared in 241 popular publications during an unspecified period of time after 1951. Again, the study of this mirror and agent of public opinion discloses most chaotic conditions; only the Catholic and the Communist press present a more or less consistent body of opinion.

The factual findings, which in themselves could have only limited local and temporal interest, serve the author as an empirical basis for developing an intricate network of conceptual distinctions and hypotheses, valuable in guiding future research. His theory is concerned with various aspects (individual, social, linguistic) of the formation of the "public image" on any issue, as well as with the processes by which such an image influences individual and group behavior. The theoretical structure erected by the author is truly impressive; in it lies the greatest value of the book. The scope of this review does not permit detailed presentation of the numerous concepts which the author introduces into his theoretical analysis. It must suffice to call the psychologists' attention to the chapter on the cognitive aspects of "la représentation sociale," in which the author makes an ingenious use of some of Piaget's ideas. The author's general orientation may be best described as an original synthesis of ideas coming from three main sources: from the French sociological tradition (Durkheim and Tarde), from Marxism and from the contemporary American empirical research on public opinion.

The empirical study which led to quite important theoretical results, paradoxically, suffers from one fundamental weakness. To study the distortions in public opinion of the true image of anything under consideration, one must state what this true image is. This the author refuses to do, although he implies a certain "model." Even the author of the preface, Professor Lagache, brings out this difficulty. Undoubtedly the popularity of psychoanalysis raises many questions which can be answered only by confrontation of popular misconceptions with the objectively ascertained state of affairs. The complete

social psychology of psychoanalysis is still to be written.

This shortcoming of the book does not diminish the value of author's penetrating insights and constructive sug-

gestions with regard to the theory and methodology of research in the area of public opinion. All interested in that area should read the book. The effort will be richly rewarded.

## Words, Music and the Brain

Karl Kleist. Translated by F. J. Fish and J. B. Stanton

*Sensory Aphasia and Amusia: The Myeloarchitectonic Basis.* New York: Pergamon Press, 1962. Pp. vii + 95. \$5.00.

Reviewed by RALPH M. REITAN

*The author, the late Professor Karl Kleist, was at the time of his death in 1960, Chairman of Neurology and Psychiatry, University of Frankfurt am Main and Director, Research Institute for Brain Pathology and Psychopathology. He is the author of Gehirnpathologie (1934) and has been described as the last of a great generation of famous German clinical neuropsychiatrists. The reviewer, Ralph Reitan received his 1950 PhD from the University of Chicago and stayed on there to teach for a while before moving to the Indiana University Medical Center where he still is teaching. His title is Professor of Psychology (Neurology) and Director, Section of Neuropsychology, Indiana University Medical Center. A primary interest, developed while he was working with brain-injured soldiers in World War II, concerns the psychological effects, including aphasia, of brain lesions. He has published extensively on the subject and has written on it for the 1962 edition of the Annual Review of Psychology.*

THE GERMAN tradition of detailed specificity in postulated relationships between aphasic symptoms and the location of critical brain lesions has received another contribution in this small volume. The author's interest is to correlate clinical symptoms of aphasia with areas of damage in the cerebral hemispheres and, more especially, with myeloarchitectonic regions, subregions, and areas of the temporal lobes.

The cytoarchitectonic structure of the cerebral cortex, in terms of the detail which has been claimed, has received serious criticism regarding the reliability with which morphological differences in cell bodies provide a valid basis for differentiation of specific areas (Bailey and von Bonin; Lashley and Clark). While similar cross-validation studies have not been done with regard to myeloarchitectonic areas, one would expect the results to be at least as equivocal because of somewhat more difficult staining problems with cell fibers as compared to cell bodies. Nevertheless, Kleist uses the myeloarchitectonics of the temporal lobe (a classification of differences in fiber structure into 7 regions, subdivided into 20 subregions, further subdivided into 60 areas) in his attempt to discern which areas were involved in patients with sensory aphasia.

Each of the patients used for illustrative purposes had extensive brain lesions, although in some they were principally restricted to the left temporal lobe. Careful myeloarchitectonic studies had been performed in every instance. Even if we were to assume perfect reliability, validity, and generality of these descriptions of the brain lesions, serious problems would still exist with respect to the clinico-pathological correlations. In some patients death occurred soon after the brain was damaged whereas in one instance approximately 20 years intervened. Changes in the brain lesions and in the clinical symptoms over long periods of time, uncontrolled with respect to temporal





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—Rudolf Arnheim, *Sarah Lawrence College*

1962; 828 pages; \$9.00 *text*

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*by* KENNETH R. HAMMOND, *University of Colorado*  
*and* JAMES E. HOUSEHOLDER, *Humboldt State College*

Enlivened by 31 numbered boxes that feed the student gems of historical interest or dramatic pronouncements about statistics, this text provides a pleasant introduction to the subject. For reasons easy to appreciate, I like the organization. It begins with its feet on the ground in the theory of measurement and goes all the way to the theory of decision.

—Stanley S. Stevens, *Harvard University*

1962; 431 pages; \$7.00 *text*

## Elements of Psychology

*by* DAVID KRECH *and* RICHARD S. CRUTCHFIELD  
*both of the University of California, Berkeley*

This is without a doubt a superior text in every way. It is probably the most coherently written and theoretically consistent book in the field today. It presents some innovations, e.g., the "box" presentation of actual experiments and the integrated treatment of physiology . . .

—Lee Sechrest, *Northwestern University*

758 pages; \$7.00 *text*

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correlations, constitute inescapable but extremely complex problems.

The clinical evaluation of aphasic symptoms of the illustrative patients leaves much to be desired. These evaluations appear to be based principally on casual interpersonal contacts of a number of physicians with the patients rather than upon standardized examinations administered to each patient. While this is quite understandable, since some of the patients were evaluated during the 19th century, the aim of establishing valid clinico-pathological correlations is hampered.

In the clinico-pathological correlations in individual patients Kleist frequently draws specific conclusions that appear to derive from his expectations rather than from fully adequate evidence. In discussing patient Pitt (p. 50), he says, for example: "There is only limited involvement of the second temporal convolution in the upper border of the territory of the subregio caudodorsalis. It is clear, therefore, that this region also has nothing to do with disturbance of the repetition of speech." This instance illustrates the problem of generalizing from specifically selected individual cases. A proper design should provide an equal sampling opportunity for inclusion of patients with coincidence of lesion location and behavioral deficit, of patients with damage of the area in question but without the behavioral deficit, of patients with lesions in other areas but with the behavioral deficit, and of patients with lesions in other areas but without the behavioral deficit. The consequences of disregard of such necessary comparisons lead Kleist to postulate what appear to be arguments of convenience to interpret his clinical findings. For example, he does not hesitate to postulate right temporal lobe dominance for speech comprehension and left frontal lobe dominance for speech expression in a particular patient when this suits his clinico-pathological rationale, even though no independent criteria for such a postulate are present in the case of this patient as compared with others in which such a postulate is not invoked. In the same area of investigation, *Speech and Brain-Mechanisms*, by Penfield and Roberts stands in striking contrast to this type of

permissiveness in interpretation.

Kleist offers many conclusions concerning the dependence of receptive language and musical functions upon the integrity of myeloarchitectonic regions, subregions, and areas. One may agree with Sir Russell Brain, in his brief introduction to this volume, that, "His

analysis of their (the patients) psychological disorders is always stimulating and his stress upon myeloarchitectonics may challenge those who do not accept his interpretation to provide a better one of a structural organization which may well possess an important functional significance."

## Into the Depths of Management

Chris Argyris

*Interpersonal Competence and Organizational Effectiveness.* Homewood, Ill.: Richard D. Irwin, 1962. Pp. vii + 292. \$6.50.

Reviewed by JACK R. GIBB

*The author, Chris Argyris, Professor in the School of Engineering at Yale University, has long been interested in the complexity of organizations. He is the author of Personality and Organization, and Understanding Organizational Behavior (CP, August 1960, 5, 272). The reviewer, Jack R. Gibb, has had at least an equally long and equally pervasive interest in group and organizational phenomena. He has kept his research interests alive while teaching at Brigham Young University and at the University of Colorado, and while holding appointments as Research Professor at the Fels Group Dynamics Center, and as Director of Research for the National Education Association's National Training Laboratory. Now a consulting psychologist in private practice, part of his time is presently occupied on a project, sponsored by the office of Naval Research, to study defensive behavior in small groups.*

THIS BOOK is likely to lead to productive controversy. Reported is a study designed to test a controversial theory and a controversial method of management development. The writing is incisive. The issues are clearly drawn and central to both managerial practice and to the development of managers. The conclusions are challenging. The reactions to the book are likely to be polar.

Argyris is one of an articulate group of behavioral scientists and management consultants who have formulated theories of management that challenge both comfortable practices and traditional assumptions in the field of management. His statements of theory have been inventive, imaginative, and bold. Always challenging, sometimes intemperate, his statements have induced bimodal reactions ranging from ardent acceptance to militant rejection.

Argyris' latest book is a report of an attack upon a critical problem: the relationships between interpersonal competence and organizational effectiveness. The book has many strengths, some limitations, and will be provocative to several varieties of reader.

Viewed as a contribution to theory, the book is provocative and helpful. Stemming from a tradition in psychology represented by such persons as Rogers, Maslow, Sullivan and Fromm, his model relentlessly and cryptically presents the implications of the developmental, interpersonal, and phenomenological psychologies for managerial practice. His model is heuristic, susceptible to empirical test, and places focus upon a selected few variables which an increasing number of theorists and practitioners think critical to personality and organization theory.

Viewed as a case study of an industrial consulting experience, the book



will be illuminating to a growing group of behavioral scientists who are building action and research relationships with industry. His diagnosis of the top management team of a large corporation is particularly penetrating and will have a valid ring to experienced consultants and to a growing body of theory-conscious executives. He gives informative and convincing protocols of a selected group of eleven executives who are undergoing T group training.

**V**IEWED as action research, the study suffers from the many methodological limitations that are perhaps necessarily incident to action research in large organizations, particularly until the client system is suitably developed to the point where conditions permit rigorous designs. Argyris makes a realistic compromise between the demands of a consultant-client relationship and the requirements of experimental design. As was indicated earlier, the model (derived from his theory) and the training method (the experimental treatment) are each controversial. Readers who are initially sympathetic to his growth-centered theory and who are favorably oriented toward the laboratory method of training will find much to corroborate their views. Critics who are unfavorably disposed toward the assumptions underlying his model or who have a negative orientation toward T group training, or both, may find the design and the findings less than satisfying.

The total situation required that the same person be the theorist, diagnostician, consultant, interviewer, and data analyst. The author is aware of this limitation and cites several safeguards taken to mitigate the possible effects of this personal variable. A colleague, Dr. Roger Harrison, was asked to make an independent administration of the Kelly Role Repertory instrument to the experimental and control group as a possible check on the observer bias. Harrison's findings are suggestive but equivocal. The members of the experimental group (those having T group training) did increase somewhat their tendency to describe each other in interpersonal terms after training, a finding that corroborates a prediction from the model.

However, the members did not transfer this tendency to descriptions of persons not members of the training group.

In major instances the results tend to corroborate earlier findings in studies assessing the effects of the laboratory method upon interpersonal competence and upon behavior within organizations. It would perhaps have strengthened the findings to have compared them with data from such studies as the Miles, Soar, Buchanan, or Weschler studies on T groups. Such a comparison would have been illuminating to the readers, would have added some corroborative strength to the findings, and would perhaps have influenced the interpretation of the data.

Because the book was written for a non-professional audience, many details of interest to the behavioral scientist are omitted. A complicated series of questionnaires, behavioral observations, and interviews are used at critical stages throughout the consultation, diagnosis, training, and post-training consultation. The data presented are interesting, often quite convincing, but necessarily fragmentary. The methodologist would find it helpful to examine in more detail the other items on the questionnaires, the protocols from the interviews, the statistical analyses of the data, and the detailed interview schedule.

The study has programmatic implications. Constructs such as trust, openness, and authenticity are not easily defined in satisfying operational terms. A model as suggestive and as significant as that presented in the book requires multiple measurements and rigorous empirical test. The study reported is a courageous exploratory test that opens the door to much research.

The book makes a major contribution to a problem of critical significance to applied behavioral science. It will be widely read and will create an impetus to further research and theory building.



*There is hardly a more direct way to encourage the grasp of the essence of a method than by following a recapitulation of its evolution.*

—ANATOL RAPOPORT



## Progressively Longitudinal

Margaret Willis. With a chapter by  
Lou L. Labrant

*The Guinea Pigs After Twenty Years.* Columbus: Ohio State University Press, 1961. Pp. xv + 340. \$5.95.

Reviewed by EVELYN WESTREM

*The author, Margaret Willis, is an historian who has been associated with Ohio State's University School since 1932. Evelyn Westrem, the reviewer, received her master's degree from the University of Southern California and now works as a social scientist in the Educational Research Division of the System Development Corporation. She has, and does things about, a special interest in remedial reading and in automated teaching.*

**T**HE BOOK, *The Guinea Pigs After 20 Years*, is a unique follow-up of a special educational program instituted at the University School at Ohio State University in the 1930's, a program following the philosophies for curriculum development of John Dewey and Boyd Bode.

The data on which the report is based were obtained from the entire graduating class of 1938. Fifty-two of the 55 class members returned biographical questionnaires, and extended personal interviews were conducted with the 45 students who had been in the program for three or more years. Since subjects of the study evidenced a range of I. Q.'s (71-170) and diversity in background, special education rather than selection seems to account for the results.

The thesis of the study can be stated best in the author's own words: "If basic high-school curriculum reorganization is worth the effort, it should have results which are apparent in the adult living of the students who experience it." The follow-up, then, was an attempt to associate reported aspects of the



adult living of the "guinea pigs" with their high-school experience.

The book is divided into three parts. Part I provides background information about the students, describes the educational program, and details the method of collecting and analyzing the follow-up data. Part II presents the data, classified and compared with other research studies. Part III describes the philosophy of the University School and summarizes the ways in which its values and goals were evidenced in the adult living patterns of the students.

The hazards involved in attempting such a treatment are obvious to anyone who has summarized questionnaire and interview data. Willis does not minimize the problems encountered. She explicitly declares her personal biases which might have influenced the interpretation and classification of responses. Frequent comparisons are made with other surveys. Many categories of responses of the University School "guinea pigs" are compared with Terman's gifted children and with a survey of the Princeton class of 1944; in every case the similarities and differences are noted so the reader is permitted to judge the appropriateness of the comparisons. Procedures and techniques are detailed sufficiently to enable investigators to collate future studies with her data.

It is impossible to summarize the data tabulated in some 63 tables in a short review. However, it seems clear that the "guinea pigs" compare favorably with groups reported in other research studies. The achievement of the educational "guinea pigs," as expressed by higher degrees and honors earned in college, surpassed Terman's gifted children. Their median income at the reporting date was equivalent to the Princeton class of 1944. In general, the data suggest that the "guinea pigs" are a superior group of adults. Proponents of the once popular progressive education movement will surely find comfort in the results of the study.

The value to educational psychologists of a longitudinal study of students participating in a special program is unquestionable. Too few studies such as this have been done. The literature abounds with reports of experiments, but follow-up studies are infrequent.

When a new program is instituted on an experimental basis, investigators are often anxious about future impairment of the student. Willis' examination of such students should help to allay this pre-experimental fear, since the "guinea pigs" apparently lost little, and perhaps gained much, from their service as subjects.

If educational goals are more than immediate, it is critical that educational programs be evaluated by succeeding studies of this caliber. The detail and rigor evidenced here make the book a genuine contribution to educational research. Many more of the same would certainly contribute to the needed re-examination of long-term goals of educational systems.

## For Educating Educational Researchers

Deobold B. Van Dalen. With two chapters by William J. Meyer

*Understanding Educational Research: An Introduction.* New York: McGraw-Hill, 1962. Pp. xi + 332. \$6.95.

Reviewed by ROBERT L. EGBERT

Deobold Van Dalen, the principal author here, received his doctorate in education from the University of Michigan and, since 1946 has been Professor of Education at the University of Pittsburgh. William J. Meyer, contributor to the book, is a Syracuse PhD who is now Associate Professor of Psychology and Education, also at Pittsburgh. The reviewer is Robert L. Egbert, who did his doctoral work in educational psychology at Cornell University. On his way from Cornell to his present position with the System Development Corporation, he taught at Utah State University, at Brigham Young University, and worked for a while with the Human Resources Research Office. His present title, held since 1961, is that of Head

of the Administrative and Support Section of Educational Research and Development in the Research Directorate at SDC. One may now refer to him, with both brevity and accuracy, as a human factors scientist.

PROFESSOR Van Dalen indicates two primary purposes for writing his book; it is to serve as a primer for the potential researcher and to help the classroom teacher acquire a better understanding and appreciation for research. In the opinion of this reviewer, the book will be successful in achieving the first objective but will be less successful in reaching the second. In writing for the researcher, it is essential to include details and depth of presentation that are beyond the interests and background of most classroom teachers. Van Dalen has done this and in so doing has written a valuable research methods text, but one which will probably have little appeal to the teacher.

Traditionally, educational research has not been adequately financed. In the past, this deficiency has been reflected by a shortage of good methodology texts and competent researchers. However, during the past thirty years, at least in part as a result of increased foundation and government financial support, real strides have been made in the quality and sophistication of educational research. A comparison of such research as reported in the journals today with that reported during the thirties reveals a marked improvement in control, design, and statistical analysis.

In a similar manner, texts describing methods of educational research have undergone a significant metamorphosis. From pedantic and superficial descriptions of surveys and questionnaires, these books have progressed to stimulating and significant discussions of educational research as a part of the general scientific endeavor and have stressed the importance of conducting research within a framework of theory.

Professor Van Dalen's book is another forward step and should be of considerable value as an aid in the academic preparation of research workers in education.



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*Understanding Educational Research* is of particular value in depicting educational research as a part of the general scientific enterprise. In chapters devoted to "Methods of Acquiring Knowledge," "General Concepts Concerning the Scientific Method," and "Nature of Observation," discussions of such topics as "deduction," "induction," "assumptions underlying the scientific method" and "the goals of science" are more complete than is typical in educational research books. Many references are made to examples from education.

The research novice will also find superior discussions of "printed resources," "library skills," and "patterns of historical research." He will find much less help in selecting or developing the "tools of research." Here the discussion is superficial. Furthermore, there is a lack of information concerning development of such recent research aids and techniques as the digital computer and simulation methods.

Two somewhat uncommon features are the two chapters on statistics, by William J. Meyer, also of the University of Pittsburgh, and a series of excellent appendices giving examples of such significant topics as "constructing a theoretical framework," in which the author utilizes Ryan's *Characteristics of Teachers*; "hypothesis construction;" "deducing the consequences;" and "criticism of a theory." The chapters on statistics, dealing with "descriptive statistics" and "inferential statistics" appear sound but are so brief that the discussion might better have been restricted to uses and limitations of statistics rather than to attempt presentation of computational procedures.

Several contrasts appear in the quality of the content and format of this book. Obviously a great deal of thought went into organization and the interpretation of this organization through meaningful headings in the text. However, this complete presentation of organization is not apparent in the very brief Table of Contents. On the other hand, the index is well done. The text itself runs the gamut from far too much detail, e.g., ten pages on "improving note taking," to rather striking omissions such as the absence of chapter summaries.

Despite some weaknesses, *Understanding Educational Research* is a fine addition to the field and should contribute significantly to the training of future educational researchers. However, it will probably accomplish less than is intended in improving the knowledge and attitudes of classroom teachers toward educational research.

## On the Ancient Art of Rating

Thomas Whisler and Shirley F. Harper (Eds.)

*Performance Appraisal: Research and Practice.* New York: Holt, Rinehart & Winston, 1962. Pp. v + 593. \$8.50.

Reviewed by WAYNE K. KIRCHNER

*The first author, Thomas Whisler, is a specialist in the field of industrial relations and is now Associate Professor of Industrial Relations in the University of Chicago's Graduate School of Business. Shirley Harper, his collaborator here, is a librarian presently in charge of the library of the Industrial Relations Center at the University of Chicago. Wayne K. Kirchner is an industrial psychologist who did his graduate work with Donald Paterson at the University of Minnesota and who, since 1956, has held both research and managerial positions, not concurrently, with the Minnesota Mining and Manufacturing Company.*

THIS is an excellent book, not only for people who know nothing about performance appraisals and ratings, but also for people who do. At first glance, the book appears to be another collection of readings, but in addition to the readings that have been collected, the authors also spend some time putting across their own viewpoint on performance appraisals and also present case history information about actual com-

pany experience with performance appraisals. It is a handy reference source, yet it is also a book that presents some interesting thoughts on the future of performance appraisals in industry.

The readings range from articles written by such stalwarts as Donald G. Paterson and Arthur Kornhauser back in the early 20's to modern critiques of performance appraisals such as Doug McGregor's. There is no particular quarrel on the part of the reviewer with the readings that were selected. It is interesting, however, while browsing through these readings to discover that many of the things that we talk about today in the rating and appraisal area were talked about forty years, and in some cases considerably longer, ago.

"Deja vu" operates very nicely in this area. Of course, as the authors point out, there is evidence of ratings being carried on as far back as 221 A.D. Unfortunately, some eighteen hundred year old problems of rating—are still nagging away today—problems of objectivity, for example.

In any case, the authors are highly positive toward performance appraisals and their use. In addition they make some suggestions as to how performance appraisals might be used more effectively and it is here that the serious psychologist is probably going to find his most enjoyment. For example, the authors have suggested very strongly that an appraisal plan or system has to have some kind of control feature built into it and they have suggested further that for non-managerial personnel the best control is seniority. In other words, it appears that the authors are suggesting that ratings or appraisals are not always necessary, particularly on jobs where there is little differentiation among the people doing the jobs. This view is probably correct but many personnel men will shake their head about it.

Again, then, this book is a well collected set of readings, with fairly well done case history materials, plus a thoughtful summary statement of the authors' thoughts and predictions concerning the general field of performance appraisal. This reviewer liked it; he thinks most industrial psychologists and most personnel people, in fact, will too.



**SCIENTIFIC CREATIVITY: Its Recognition and Development**

*Edited by Calvin W. Taylor, University of Utah; and Frank Barron, University of California, Berkeley. Selected papers from the Proceedings of the First, Second, and Third University of Utah Conferences: "The Identification of Creative Scientific Talent," supported by the National Science Foundation. In these papers, leading psychologists report on their pioneering efforts to define the criteria and characteristics of scientific talent. They provide a rich source of suggestions for future research and speculation 1963. 419 pages. \$7.95.*

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**THE MENTAL PATIENT COMES HOME**

*By Howard E. Freeman, Brandeis University; and Ozzie G. Simmons, University of Colorado. This book is the result of a large-scale research study of psychiatric rehabilitation by the Community Health Project at the Harvard School of Public Health, with the support of grants by the Professional Services Branch and the National Institute of Mental Health. It describes the posthospital experiences and levels of rehabilitation of 649 mental patients with the objective of determining the factors that affect their behavior and performance after they return home. 1963. 309 pages. \$7.95.*

**MEASUREMENT IN PERSONALITY AND COGNITION**

*Edited by Samuel Messick, Educational Testing Service, Princeton, New Jersey; and John Ross, University of Western Australia. Based on a conference held at Princeton, New Jersey, under the auspices of the Educational Testing Service, this book brings together the latest discussions of major issues in personality measurement. The contributors are specialists with diverse backgrounds, ranging from factor analysis to psychoanalysis. The book, therefore, clarifies diverse modes of approaching the problem of personality measurement, ranging from reliance upon clinical interpretation through developmental description and experimental manipulation to psychometric analysis. 1962. 334 pages. \$5.75.*

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*By William W. Cooley, Harvard University; and Paul R. Lohnes, University of New Hampshire. Multivariate statistical analyses have become powerful research tools in the behavioral sciences. This book provides a coherent introduction to the more useful multivariate techniques and their applications in psychology and education. It contains "ready-to-run" computer programs — and shows the reader how to use these techniques without confusing computational detail. 1962. 211 pages. \$6.75.*

**COMPUTER SIMULATION OF PERSONALITY: Frontier of Psychological Theory**

*Edited by Silvan S. Tomkins, Princeton University; and Samuel Messick, Educational Testing Service. The papers and discussions presented at a conference on computer simulation and personality theory held at Educational Testing Service and Princeton University in June, 1962. The purpose of the conference was to evaluate the potential of computer simulation for the field of personality. The papers and discussions analyze the problems of utilizing computers in general psychology, psychoanalytic theory, and cognition and affect. 1963. 325 pages. \$5.00.*

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# Folie en Masse

Hugh Mullan and Max Rosenbaum

*Group Psychotherapy: Theory and Practice.* New York: Free Press of Glencoe, 1962. Pp. vii + 360. \$5.95.

Alexander Wolf and Emanuel K. Schwartz

*Psychoanalysis in Groups.* New York: Grune & Stratton, 1962. Pp. v + 326. \$8.00.

Reviewed by WALTER G. KLOPPER

The authors of the two books are identified by the reviewer, who is Walter Klopfer, identified here as an expert on group therapy who received his PhD from the University of California and who first became interested in group therapy while working in Berkeley with Hubert S. Coffey. He has taught at Duke University and at the University of Nebraska College of Medicine. Between these two pedagogical stints, he served as chief clinical psychologist at the Norfolk, Nebraska, State Hospital. Presently he is Professor of Psychology and Director of Clinical Training at the University of Portland, Oregon. He is the author of *The Psychological Report* (1960, CP, July 1961, 6, 236), and is president-elect of the Society for Projective Techniques.

AMONG the many similarities between these two books is the fact that all four of the authors are psychoanalysts, and that each pair comprises one psychiatrist and one psychologist. Alexander Wolf, one of the medical authors, is currently Associate Clinical Professor at the New York Medical College and a prolific writer in the area of group psychoanalysis. He is associated with his co-author, Emanuel Schwartz, at the Post-Graduate Center for Psychotherapy where Wolf is Supervising Psychiatrist and Schwartz is Associate Dean. Both of them have many other positions in the New York area as well.

Hugh Mullan is a psychiatrist who practices privately in New York City and is also Medical Director of the New York Alcoholism Vocational Rehabilitation project. His co-author, Max Rosenbaum, is also in private practice in New York and serves as Education Director of the Association for Group Analysis, Inc.

Psychoanalytic group psychotherapy is evidently quite different from group therapy as practiced by other persons. Both the present books agree on encouraging the reader to focus his attention upon intra-personal phenomena, even though Mullan and Rosenbaum agree that the group may enhance the interpersonal efficiency of the patients as well. Relationships within the group are assumed to be false ones, as characterized by such terms as transference and countertransference. Wolf and Schwartz not only do not promote discussion of conscious feelings between group members, but actually discourage discussion of life problems and favor the discussion of fantasies and dreams. Thus, although both of these volumes are psychoanalytically oriented, the one by Wolf and Schwartz appears more doctrinaire and less flexible.

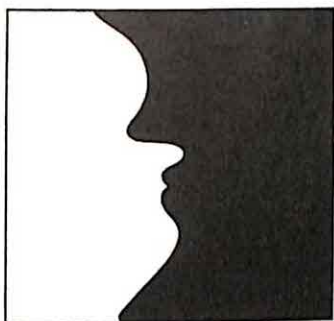
The intention of the authors differs from one volume to the other. In the book on psychoanalysis in groups, Wolberg states in the Foreword: "The same forces of bigotry and self interest that have blighted the field of individual psychotherapy have settled into this new

domain, filling it with a spirited babel. Amidst the din, a few eloquently rational voices have been raised . . ." It is presumed from what follows that the eloquently rational voices are those of the authors and that they propose a rather concrete carrying over of orthodox psychoanalytic principles into the group situation.

Mullan and Rosenbaum on the other hand describe their book as a "theoretical and practical treatise that describes in detail the selection, preparation, and introduction of patients, as well as the hour by hour work that group therapy entails." Thus, our second volume is an "all about" book.

IN THE book by Wolf and Schwartz there is considerable ambivalence expressed concerning the value of the group as a curative agent. On the one hand there appears to be considerable reluctance to give the patients their head and let them discuss what they wish. One patient's attempts to help another are considered a form of resistance against his own role as a patient. The patients are strongly encouraged to discuss their feelings toward the analyst who remains the focus of attention. Progress is gauged by dreams and other standard psychoanalytic means. The interaction among the patients is grist for the mill only in terms of the depth of response evoked. All difficulties that group members have with one another are assumed to be irrational. All problems can hopefully be reduced to sexual ones and thus become analyzable. The other side of the ambivalence is expressed by these authors in their encouragement of the 'alternate' session. This is considered desirable because it provides an opportunity for interaction. The reasons given seem plausible, including the benefit to the patient in gaining peer support and the enhancement of self-esteem implicit in helping someone else. Why these benefits could not be enjoyed equally in the regular session is not made explicit. The authors decry routine combinations of group and individual analysis, as well as reluctance to provide alternate sessions as both reflecting excessive dominance and dogmatism on the part of





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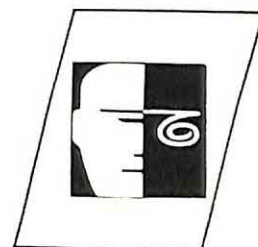
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## CONTEMPORARY RESEARCH IN PERSONALITY

By Irwin G. Sarason, University of Washington. 1962 Ill. 7 1/2 x 9 1/2 423 pages \$7.00

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the analyst. On the other hand, the term group dynamics is evidently regarded with abhorrence and it is claimed that group dynamicists are persons not interested in history or diagnosis, who ignore individual differences, and place all therapeutic responsibility on the group. The reviewer is led to conclude that Wolf and Schwartz, as judged from this book, are not at all unaware of the curative factors to be found within the peer group, but seem to find it difficult to incorporate this concept into their theoretical frame of reference. The style of the book is quite variable, sometimes being rather philosophical and rambling, and at other times being quite practical and dogmatic.

The book by Mullan and Rosenbaum is much more catholic than it is parochial. The basic practical suggestions for administering and carrying on with a group are pretty standard in this book as in the other. The group preferred is the small one, with the emphasis on heterogeneity of every kind, and with a further emphasis on very careful preparation of the patients for the group process over a period of months. The authors actually compare themselves to Wolf and imply that their approach is less concrete, and more general in applicability. Although they use the same jargon, the meaning seems to be more general. Thus, both sets of authors refer to the group as being in essence a 'family' configuration; but, whereas Wolf and Schwartz consider all relationships as having specific transference meaning, harking back to the original family, Mullan and Rosenbaum talk about persons in intimate situations where they can communicate fully and richly. The form of therapy being described in "Group Psychotherapy" is called regressive-reconstructive, and consists essentially of getting people to trust one another and express their feelings freely after which they are ready to understand their relationships more fully and utilize their intra- and interpersonal resources to a larger extent. This book contains good reviews of other people's work in each relevant area, and fascinating illustrative material. Even though Mullan and Rosenbaum, being psychoanalysts, express some guilt about their interest in actual

situations and interpersonal phenomena, they evidently do lean in that direction.

It seems obvious to this reviewer from both a reading of the current clinical literature and observations of practice, that this is indeed coming to be the decade of group psychotherapy. There seems to be a growing lack of faith in the efficacy of individual psychotherapy as a panacea, and a growing recognition of the realistic and economical nature of group processes for the treatment of emotional disorders. It seems highly encouraging that analysts with their historical proclivity for secrecy and privacy, are willing to emerge into the bright light of the group room and to consider exposing themselves to the cross-currents of real life. Hopefully, it is in this way that their wisdom can be separated from their folly. The two books discussed above are probably representative of the current thinking in this area, one of them representing a more traditional approach and the second being somewhat more avant-garde.

## To Catch Them Young

Douglas K. Candland and James F. Campbell

*Exploring Behavior: An Introduction to Psychology.* New York: Basic Books, 1961. Pp. vii + 179. \$4.50.

*Reviewed by* ROBERT H. KNAPP

*Douglas K. Candland received his PhD at Princeton University in 1959 and worked as a post-doctoral research fellow at the University of Virginia before moving to Bucknell University where he is now an assistant professor. He met with the second author, James F. Campbell, at the University of Virginia where Campbell remains as an experimental psychologist, specializing in the investigation of learning and motivation. Robert Knapp is a Harvard PhD who, after varied and strange experiences during World War II, settled at Con-*

*necticut's Wesleyan University. He has been there, except for relatively brief adventures elsewhere, for almost 15 years. Among his many publications are, with J. J. Greenbaum, The Younger American Scholar (1955) and, with H. B. Goodrich, The Origins of American Scientists (1953).*

It is probably true that all scientists, including psychologists, are coming to be recruited to their calling at an earlier age. If this be so, this publication has some real importance, for it is its clear purpose to present "scientific" as opposed to "popular" psychology at the high school level. This is not a textbook, though the first three sections are devoted to the range of psychological inquiry, the design of experiments and statistical techniques. Thereafter, however, there are some 12 chapters devoted to the review of experimental areas in psychology of the more classical type. Sensory process, perception and learning are heavily emphasized on the grounds that here "psychology more closely approaches the objective methods of the natural sciences." Descriptions of easily constructed experimental apparatus are given, while each of the experimental sections concludes with a short but useful bibliography. This book, therefore, should be particularly useful in inspiring psychological projects for "science fairs" and similar events. The contents of this book are more safe than imaginative, at the same time they are irreproachable and carry a convincing moral to the effect that psychology, too, may be scientific.

Many psychologists have expressed doubts whether psychology should be introduced into the high school curriculum. But this turns out to be an academic question. The plain fact is that psychology, in one form or another, is being taught at this level, mostly in conjunction with other courses but sometimes separately and in its own name. The present volume, therefore, does supply a thoroughly sound and respectable version of psychological research which should prove a most useful antidote to the careless introduction of "psychology" by non-professional persons at the high school level. Plato has



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## ☐ CONFLICT AND CREATIVITY

*Edited by SEYMOUR H. FARBER and ROGER H. L. WILSON, both of the University of California School of Medicine. McGraw-Hill Paperback Series. 348 pages, \$2.95 (paper), \$6.50 (cloth).*

This unique symposium on the community's demands for both creativity and conformity presents essays by twenty-eight well-known authorities in the arts, medicine and the social sciences. Each discipline encourages a different approach to the problems of conformity and diversity and methods for the resolution of differences. This interdisciplinary discussion of the forces acting on the mind clarifies existing thinking and raises new questions which can become the subject of future research. The challenge of discussion is maintained in these essays drawn from the proceedings of a second conference on *Man and Civilization: CONTROL OF THE MIND*, Held at San Francisco in 1962. The first such conference held in 1961 was reported in *Man and Civilization: CONTROL OF THE MIND* (McGraw-Hill Paperbacks, \$2.95).

## ☐ PSYCHOLOGY: A Study of a Science

Study II. Empirical Substructure and Relations With Other Sciences

Volume V. The Process Areas, The Person, and Some Applied Fields: Their Place in Psychology and in Science

Volume VI. Investigation of Man as Socius: Their Place in Psychology and the Social Sciences

*Edited by SIGMUND KOCH, Duke University, Volumes V. and VI. available in March, 1963.*

These are the fifth and sixth volumes in this vast, seven volume inquiry into the status and tendency of psychological science. Study II seeks an increased understanding of the internal structure of psychological science, and its place in the matrix of scientific activity. The first four volumes are also available.

## ☐ VIGILANCE: A Symposium

*By DONALD N. BUCKNER and JAMES J. McGRATH, both of Human Factors Research, Incorporated. McGraw-Hill Series in Psychology. Available in March, 1963.*

This volume has been developed from the papers and transcript of an ONR-sponsored symposium on Human Performance on "Vigilance Tasks." The eight participants of the symposium include the major theorists in the field and, as a group, represent a notable cross-section of the various approaches that have been taken to the study of human vigilance. Each chapter of the book represents a formal paper delivered at the symposium and the discussion and critique of the paper by the eight participants. Three general types of papers are included: those concerned with presenting new research findings, those presenting theoretical views and those dealing with methodological issues.

## ☐ ALCOHOL AND CIVILIZATION

*Edited by SALVATORE P. LUCIA, University of California, San Francisco. Available in spring, 1963.*

A collection of material taken from a symposium held at the University of California School of Medicine in November, 1961. The book presents an objective view of the historical, psychophysiological, and cultural uses of alcohol in society, bringing out the significance of fermented beverages and the role of alcohol in the structure of our society. Areas covered in the symposium fall into five major sections: the effects of alcohol on the body, alcohol and the mind, the social implications of alcohol, alcohol in our society, and the specific and interdisciplinary viewpoints of the symposium speakers . . . all experts in the field. Each chapter constitutes a section in which the assigned contributor emphasizes his specific interest in terms of historical and experimental evidence dealing with the topic.

## ☐ VERBAL BEHAVIOR AND LEARNING: Problems and Processes

*Edited by CHARLES N. COFER, New York University; and BARBARA S. MUSGRAVE, Smith College. McGraw-Hill Series in Psychology. Available in March, 1963.*

This volume represents the proceedings of the Second Conference on Verbal Learning held under the auspices of the Office of Naval Research and New York University in June, 1961. The majority of the contributors, all top scholars, are the same as those which appear in the book developed from the first conference: *VERBAL LEARNING AND VERBAL BEHAVIOR*, McGraw-Hill, 1961. Presents over-view of the entire field of verbal learning and its relation to verbal behavior. For the advanced student and professional psychologist.

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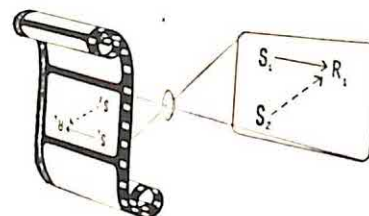
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## INSTRUCTIONAL MEDIA

Edited by A. A. Lumsdaine



### Teaching-Machine Melange

Robert E. Guild, Lecturer

*Teaching Machines. Part I* [30 min.]: *The Theory and Art to Date*; *Part II* [30 min.]: *Programming*. 16mm. black-and-white sound motion picture (kinescope). University of Washington Audio-Visual Services, Seattle, Washington. Rental: \$7.50 (each part); sale \$120 (each part).

Reviewed by DOUGLAS PORTER

Robert Guild, who created these kinescopes in what he describes as a "fairly casual local effort," is assistant professor of psychology and Coordinator of Institutional Educational Research at the University of Washington, where he earlier got both his MS and PhD. A BA from Wilamette University and World War II Navy veteran, he sandwiched in hitches with Rand and Boeing between his student and faculty days at the University of Washington. The reviewer, Douglas Porter is assistant professor of education at Harvard, where he also got his PhD. An experimental educational psychologist with training and predilections in the field of operant conditioning, his research interests are in the investigation of programming variables and evaluative garde whose publications have been closely associated with the Skinnerian sally into education throughout the past five or six years.

INFORMATION on teaching machines and programmed instruction is available in just about any communication medium at just about any level, from programmed booklets to elementary "primers," from strip films to movies, from research reports to superficial descriptions. The above films were prepared as a two-part television show with the intended audience of "... intelligent laymen and non-informed educators ... ." The source of the films strongly recommends the use of both parts of the series, or Part I if only one film can be used. However, there are strong reasons to believe that an uninformed audience can be served better by showing Part I only, even if Part II is available.

In Part I, the field of teaching machines is sketched as an active commercial and academic enterprise that must be taken seriously. Several teaching machines are displayed, and a teaching machine is defined as a "... device which controls presentation of subject matter, requires a response, and provides knowledge of results ... ." Pressey is represented by a quotation that gives his provocative conception of teaching machines as efficient labor-saving devices. Skinner is presented as one who would apply "... simple learning principles (reinforcement) ... more rigorously and broadly than most of us do." This is followed by "real life" examples of reinforcement and a short look at the ineffectiveness of reinforcing contingencies in the classroom. The most compelling section of the film is a brief case history that describes how a child failed to learn the division of fractions through the study of standard textual

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materials, and how a controlled presentation by teaching machine would have prevented the slip-ups that led to failure. The usual claims for teaching machines are given (interest is maintained, individual differences are catered to, etc.), and the film ends with rhetorical questions on some of the controversial issues usually aroused by a discussion of teaching machines.

Except for the case study, the film is plainly descriptive and speculative in tone rather than analytical. Reinforcement is the only clearly psychological concept presented, but reinforcement principles go essentially undeveloped, making it impossible to give a plausibly complex analysis of instruction. This leaves the psychologically trained viewer with an impression of shallowness, and must leave the uninitiated with doubts that such "simple principles" can be applied fruitfully to complex behavior. However, to develop reinforcement principles adequately in thirty minutes is no mean task, and failure to do so is an understandable and perhaps forgivable shortcoming of most of the popular literature on teaching machines. Lack of analysis also shows in the treatment of claims for teaching machines. To state, for example, that "The machine has infinite patience; it doesn't scold or resort to sarcasm," is going to leave the layman unimpressed and the psychologist frustrated unless the effects of punishment are considered. Again, this is a general shortcoming of the popular literature.

**A**PPROXIMATELY the first five minutes of Part II is devoted to the display and operation of one commercially available teaching machine and associated programs, then emphasis is shifted to principles and the practice of programming. The core material of Part II consists of two samples of "programmed" instructional material: Socrates' dialogue with the slave boy from *The Meno* and some elementary physics material alleged to represent "... Skinner's conception of a program." Woven into this material are quotations and paraphrases from Skinner and others on general procedures for constructing a program. Quoted almost in their en-

tirety are ten rules of programming from a 1959 APA paper of Gilbert's, in which it is suggested, among other things, that the programmer cast aside all teaching machines until the program is made. Justifiable emphasis is given to the empirical process of program development by trial and revision, and the film ends with an optimistic statement on the success of teaching-machine instruction to date.

Despite the importance of the material in Part II and some excitement engendered by dramatization of the Socrates dialogue, the presentation is marred by a number of unfortunate aspects. Most unfortunate is the choice of samples of programming. The physics material attributed to Skinner is not his own, but was recast from his sample materials published in *Science*, and suffered in the process. Although the Socrates dialogue is lively and appealing it is also misleading. The film makes it clear that teaching machine programs are not based upon a theory of innate ideas, but that leaves the dialogue with the slave boy hanging in the air, for it is not clear where the boy's responses *do* come from or exactly what discriminations he is making in order to answer Socrates' questions. The other brief samples of programming shown are outright test questions that do not reveal the subtle item-writing techniques of programming. Altogether, the uninformed viewer is probably left with the hazy impression that a program is pretty much like a conventional test.

Although Gilbert's rules are picturesque and well taken, in many instances, they are not suitable fare for a general audience in which they could engender considerable misunderstanding. Finally, one is left with the distinct impression that the first five minutes of the second film constitute a commercial plug for the products of one teaching-machine manufacturer.

Summing up, Part I of these films gives a creditable general introduction to the field of teaching machines without sufficient samples of programs themselves, without discussing programming procedures, and without sufficient psychological depth to stimulate a sophisticated audience. Although Part II is concerned with programming proce-

dures and gives a clear idea of the importance of experimentally revising a program (important matters not covered in Part I), the examples of programming are not apt and some of the materials presented require discussion in order to avoid misunderstanding by an uninformed audience. In comparison to other films (NEA/DAVI's *Teaching machines and Programmed Learning*, and the American Institute for Research's *One Step at a Time* [see below]), Part I of this series probably provides the best available general introduction, while Part II falls short of the AIR film in describing programs and program development.

## Back to Page One?

Robert W. Mager

*Preparing Objectives for Programed Instruction.* Pp. 62. \$1.75. San Francisco: Fearon Publishers, 1961.

Reviewed by LASSAR G. GOTKIN

The author, Dr. Mager, is senior research scientist for Varian Associates and has recently, along with other activities in the programed-instruction field, devised a "walk-in" teaching machine to provide a controlled experimental learning environment. A 1954 Iowa PhD, he spent a number of years with Army's HumRRO and has taught at Ohio University and Sacramento State College. The reviewer, Lassar Gotkin, is an EdD from Teachers College, Columbia University. He is now director of the Reading Improvement Service at the Center for Programed Instruction in New York City, and is working on a program for junior high school students who read two or three years behind their grade level. He is particularly interested in extra-program variables (e.g., teacher, classroom environment) that influence programs' effectiveness.



THIS book is based on the reasonable premise that in order to prepare "instructional programs which help us reach our objectives, we must first be sure our objectives are clearly and unequivocally stated." If the objectives of this book (about preparing objectives for programed instruction) are achieved the reader will, he is told, be able:

- "a. Given one or more instructional objective . . . to select those stated in performance terms.
- b. Given a well-written instructional objective . . . to identify the portion of it which defines minimum acceptable performance.
- c. Given one or more performance (test) items . . . to select those appropriate to the evaluation of the objectives." (p. 1)

The material is presented in a Crowder-like program format. After reading a few paragraphs about some concept, the reader is required to choose the correct answer to a question from alternatives (usually two). On the basis of his response, the reader is "branched" to a particular page. When an incorrect response is made, the reader is led through an explanation of his error and is then instructed to return to the original page to make another choice. To assess the adequacy of the learner's grasp of the text, a self-test is provided at the end of the book and the reader is told that for the author to have reached his objectives (stated on page 1), you can only make 7 errors or less out of 44 items."

Dr. Mager has written clearly about stating objectives meaningfully, identifying terminal behavior, and defining criterion performance. In this text he has stated his own objectives, provided a Crowder-type program to achieve them, and supplied a criterion test (along with a standard of performance) for the reader to assess his own learning.

The potential consumer has the responsibility of judging the programmer's objectives, critically analyzing the program, and questioning the standards—the latter two can even be explored empirically. This reviewer finds (1) the stated objectives trivial and out of line with the title and content of the book, (2) the branching items at the end of

each explanation out of accord with the explanatory material and requiring such gross discriminations that intelligent readers can make correct choices without reading the text, and (3) the test standard attainable without reading the text. Dr. Mager has provided an illustration of the difference between merely writing about programs and actually producing effective programs.

THE only behavior required in this program is that of making choices between two (or occasionally more than two) statements. However, the learner's ability to make correct choices, though necessary, is not a sufficient indication that he can prepare meaningfully stated objectives.

In a branching program the learner's path is determined by the choices he makes. Six secretaries who had not read the text were given the branching items. Analysis of their responses reveals that these "naive" subjects were able to make correct selections without benefit of the explanatory text. The subjects had a mean of 11 out of 13 (85%) correct choices without having read the text. Not only did these "naive" subjects make almost no incorrect choices, but, the only item missed by the majority was one in which the author erroneously maintains that the ability to "derive the quadratic equation" does not indicate that the individual can "solve quadratic equations." Many programmers overshoot an objective in order to assure achieving it. Deriving the quadratic equation requires performance of a set of operations which can be used to solve all possible quadratics. The data indicates that the items which the author believes measure understanding of the text involve such gross discriminations that most readers can make correct selections solely by reading the answer choices.

Criterion-test performance is established as 7 or less wrong, or 84% or more correct, which happens to represent performance at least one standard deviation above expectation (if the respondent were merely guessing on a test of two-choice items). The six "naive" subjects averaged 7 errors (criterion performance) on the self-test without

even going through the program. Four of the six reached criterion performance of 7 or less errors.

What Dr. Mager has written about objectives is well worth reading. Most readers, however, by making correct choices, would omit some of the best parts, e.g., a diagram and explanation differentiating prerequisites to a course, the course, and its objectives. Dr. Mager appears to have been confined by the method of programing he has chosen; both his objectives and the behavior required of the learner in the program are shaped to fit within the boundaries of the method. Furthermore, his choice of a standard of performance on the posttest is statistically naive and functionally misleading.

In this review I have raised three of the questions which the consumer should ask of every program: (1) Is the content appropriate? (2) Is it well programed? (3) Does it achieve its objectives? For the latter two questions, I have demonstrated a few of the crude but powerful empirical checks that can be used to evaluate a program with only a few test subjects. Consumers, especially school personnel, have the responsibility to test programs even when the publisher provides statistics.

At the end of the terminal test the student is asked, "How well did you do?" If he has not achieved the author's standard, i.e., seven or fewer errors, the learner is instructed to return to page 1. The evidence suggests that Dr. Mager, not the student, ought to heed this advice.



*A dream is a strange thing. Pictures appear with terrifying clarity, the minutest details engraved like pieces of jewelry, and yet, we leap unawares through huge abysses of time and space. Dreams seem to be controlled by wish rather than reason, the heart rather than the head—and yet, what clever, tricky convolutions my reason sometimes makes while I'm asleep! Things quite beyond comprehension happen to reason in dreams!*

—DOSTOYEVSKY  
1877





# Convergent and Divergent Excellence

Jacob W. Getzels and Philip W. Jackson

*Creativity and Intelligence: Explorations with Gifted Students.* New York: Wiley, 1962. Pp. vii + 293. \$6.50.

Reviewed by WILLIAM E. COFFMAN

The first author, Jacob W. Getzels, is a Harvard PhD who during World War II was a clinical psychologist for OSS, who after the war served for a time as counselor at Harvard University and, who, since 1951, has been at the University of Chicago where he is now Professor of Educational Psychology. The second author, Philip W. Jackson, received his PhD from Columbia University. Before and after that he has taught in the Newport, New Jersey, public schools, at the University of Puerto Rico, at Columbia and Wayne Universities, and now at the University of Chicago, where he is Associate Professor of Education. He obviously does things other than teaching. The reviewer, William E. Coffman, has his EdD from Columbia University's Teachers College. He has taught in the public schools, in the US Army, at Horace-Mann-Lincoln Institute Teachers College and at Oklahoma A & M. Since 1952 he has been associated with the Educational Testing Service, where he is currently Director of Research and Development in its College Board Programs Division. His interest in testing goes back to his undergraduate days as a student of education and psychology at Wittenberg College but it received its major impetus from his experience as a doctoral candidate with Robert Thorndike at Teachers College.

IN THIS book, Getzels and Jackson are reporting the results obtained when they ask themselves some cogent ques-

tions about the nature of excellence and then apply their professional skills to the search for answers. Their skills are impressive, particularly those involving the application of creative imagination to the development of a network of relationships between their own data, the data reported by other investigators, and theoretical formulations regarding the nature of the creative process. In addition, they write a fluid line. The book is stimulating in content; it is also pleasing in form.

The central focus of the search is on an examination of the nature of two aspects of cognitive behavior, that which leads to high scores on a typical IQ test (*intelligence*) and that which leads to high scores on some unusual, and frankly experimental, tests requiring responses of a novel or speculative type (*creativity*). By using common terms to convey their specialized meanings, the researchers avoid the pedantry of scientific jargon; at the same time they introduce a hazard for the reader, for it is not easy to keep track of the transitions as the discussion moves from a consideration of the research data (where the technical meanings are intended) to theoretical considerations (where different meanings apply).

The problem under investigation is an important one. It is important for anyone designing an educational program for cultivating excellence in all its variety. It is important for college admissions committees trying to choose from among a flood of applicants those most likely to profit from educational

opportunities. It is important for psychologists trying to understand the etiology of behavior. And it is important for the psychometrician responsible for developing tests for the identification of talent. If the ability to adapt to the new is different from the ability to comprehend the old, we need to know the nature of the differences. If we are directing the spotlight on one kind of excellence to the neglect of another of possibly greater importance, we need to learn how to correct our deficiencies.

The reader searching for definitive answers to his practical problems will be disappointed. The authors warn that their book will be of little use to those searching for standardized tests of creativity for immediate use. Nor will those interested in generalizations regarding the extent of creativity among American adolescents find an answer to their questions. The study has been geared to the development of insights through an intensive look at small and carefully selected samples of admittedly unrepresentative subjects. But the reader will find a host of stimulating ideas assailing him from every page whether he be a curriculum builder, test builder, or theory builder. The authors recognize the limitations of their method: "... we hold our findings not so much definitive as suggestive of further work" (p. 111). But their recognition of limitations does not dampen their enthusiasm for the insights they have developed. Like the creative subjects who are the focus of their investigation, they exhibit an 'openness in the encounter with the world' which leads to stimulating suggestions regarding the nature and nurture of creative talent. Characteristic of the tone of the report is the sentence beginning: "If we may overstate the case somewhat . . ." (p. 119).

THE METHOD of inquiry is clinical rather than experimental or normative. From a population of 449 bright adolescents (mean IQ of 132 for whom relatively complete data were available, four extreme groups were chosen for study: a group of 28 scoring in the top 20% on IQ but not so high on other measures, a group of 26 high only in *crea-*



tivity, a group of 30 high only in morality, and a group of 35 high only in adjustment. To the extent that the various measures succeeded in their intentions, the subjects of the study might be expected to exhibit unusual behavior. To the extent that interactions among qualities are relevant to effective personality functioning, one might view with caution descriptions developed on extreme deviates. At the outset, for example, one suspects he may be dealing with bookworms and beatniks as representatives of intelligence and creativity.

There is some suggestion of the beatnik in the creative group and some suggestion of the bookworm in the IQ group, but the responses of each do appear to throw light on the problem under study. One wishes, however, that the various tables comparing responses of the extreme groups with each other and with the total group also reported results for sub-groups high on two or more of the measures. As one reads, it becomes clear that these subjects have not been ignored. A number of them are subjects of case studies which round out the report of the study, and their responses are drawn on to a considerable extent in the section which relates the findings of the study to theoretical formulations. In fact, one develops the impression that some of the most convincing 'creative' responses are made by individuals who scored high on both IQ and creativity.

One finds himself wishing at many points that the basic data were available for further study. A table of inter-correlations of test scores suggests that the several measures of creativity are as different from each other as they are from the measure of IQ, but there are no means and standard deviations reported to permit further exploration of the idea. One is impressed by the contrast between the stories of seven of the 26 creatives and of six of the 28 IQ's, but he wonders how impressed he would be if he were able to examine all of the stories. Or how compelling is the case study reported by the authors when compared with the complete data on which it is based? Copies of the tests are included in an appendix, but one looks in vain for the test scores.

Perhaps it is inevitable that the ex-

perimentalist or the psychometrician will feel a bit uncomfortable with the kinds of data collected and interpreted by Getzels and Jackson. Rather than spending time teasing other interpretations from the data reported here, such psychologists might more profitably turn their attention to designing studies in which their own methods are brought to bear on questions raised by the report. For example, Getzels and Jackson use objective tests of achievement to measure school progress and find that their creatives do as well as their high IQ's on these measures. Yet they argue that such tests reward convergent thinking (the kind measured by the IQ test) rather than divergent thinking (the kind measured by their tests of creativity). At the same time they suggest that teachers' judgments (whether based on the reading of essay examinations or observations of behavior in class) are more likely to reward divergent thinking than is the usual objective test of achievement. Yet they imply that teachers' grades were higher for the high IQ's of this study than for the creatives. (Grade data are not reported.) Is it possible that a factor study would reveal high loadings on divergent thinking for well-made achievement tests?

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*The writer is a man overpowered by  
words, sentences, rhythms, ideas, the  
drama of ideas when there are lives  
moving in them, and the forms he can  
shape from his medium. Language haunts  
him. Words, sentences, rhythms are not  
things to him; they are presences. The  
presence of his medium makes him feel  
more than he really knows how to think  
or say. He knows that he is wiser, richer,  
more perceptive, more sentient when he  
is immersed in his medium than he can  
hope to be when he is high and dry in  
ideas and presences that he can identify  
and talk about with clear and peda-  
gogical coherence. His medium is a gor-  
geous confusion upon him and a gorgeous  
flowering of all possibility. It is a house  
of great ghosts.*

—JOHN CIARDI

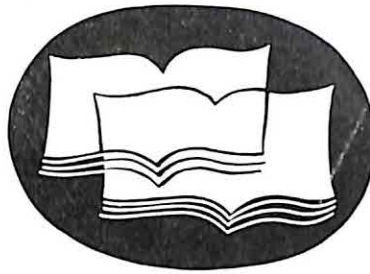




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*Contemporary Psychology: A Journal of Reviews* is published monthly. The subscription per year is \$10.00, foreign \$10.50, single number \$1.60.

Published by the American Psychological Association at 2210 W. Dallas, Houston, Texas and 1333 Sixteenth St., N.W., Washington 6, D. C.

Second-class postage paid at Houston, Texas and at additional mailing offices. Accepted for mailing at special postage rate stated in section 132.122, Postal Manual.

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# Contemporary Psychology

A JOURNAL OF REVIEWS

VOLUME VIII

April, 1963

NUMBER 4

## Biotropic Search for Substructures

Sigmund Koch (Ed.)

*Psychology: A Study of a Science. II: Empirical Substructure and Relations with Other Sciences. Vol. 4: Biologically Oriented Fields: Their Place in Psychology and in Biological Science.* New York: McGraw-Hill, 1962. Pp. v + 731. \$12.50.

The editor of this fourth volume in the monumental series *Psychology: A Study of Science*, is the same Sigmund Koch who edited the first three and who will a) edit the coming fifth and sixth and b) write a seventh to serve as a postscript to the series. Since 1952 he has been engaged with these volumes (all growing out of what was termed *Project A of APA's Study of Psychology as Science and Profession*), and all during that time he has been handling academic duties at Duke University. His chief interest continues to be in the areas of motivation, learning, and psychological theory.

### Mostly Brain-Behavior Patterns

*Some Interrelations between Psychology and Genetics*, by PAUL R. DAVID and LAURENCE H. SNYDER

*How Man Looks at His Own Brain: An Adventure Shared by Psychology and Neurophysiology*, by ROBERT B. LIVINGSTON

*Brain and Mind*, by GERHARDT VON BONIN

*Interrelations of Psychology and the Neurological Disciplines*, by KARL H. PRIBRAM

*Biological Psychology*, by I. T. DIAMOND and K. L. CHOW

*Experiment and Theory in Physiological Psychology*, by R. C. DAVIS

Reviewed by LAWRENCE I. O'KELLY

who reports himself comfortable with the role of a physiological psychologist who is happily able to sustain the view

that psychology is a biological science. He received his doctorate at Ohio State University and after a brief spell at the University of Colorado and another in the Army, went to the University of Illinois where he is now Professor of Psychology. His protracted work on thirst motivation and water regulation has carried him into a number of substantive areas not usually traversed even by biotropic psychologists.

IN MANY ways the best review is already written and prepackaged, for Professor Koch's introduction to this volume is an excellent model for all editors of multiple-authored, multiple-subject works. His "anticipations" of what the contributors to Study II will be saying about psychology is a succinct summary. At least three important points are made. First, "it becomes fairly evident that those committed to interdisciplinary work are not in general combining plural disciplines into single skulls . . . but tend to be crossing field boundaries rather adventi-

tiously in the pursuit of problems originating in the home territory. One gets little impression of a concern with the broader environment of interrelationships which might condition the significance of the specific cross-field variables under study." Secondly, Koch sees a lessening of concern about epistemological strictures, which hedged concern about subject matter, and in its place a lessening of "sophisticated and hygienic imagery of variables and functions, data languages and construct languages, along with the presumption that only fools could find the confines of the linguacentric predicament chafing." Thirdly, there is a renewed interest in the analysis of *process*, and a renewed faith in "a psychological universe to which research must be adequate."

It is thus obvious that, if Koch's evaluation of Study II is valid, these volumes will sensitively trace for us a new emergence that might well mark the achievement of maturity. The present review can only pass the most tentative and incomplete judgment of this hope.



since it is limited to the first six contributions to Volume 4, the remainder being done by two other reviewers, and Volumes 5 and 6 are still to be seen.

The first contribution is by Paul R. David and Laurence H. Snyder: "Some Interrelations Between Psychology and Genetics." Having previously stated that there are major differences between the kind of variation studied in laboratory animals and those of significance in natural populations, so far as genetic variables are concerned, human populations resemble wild-living groups rather more than the laboratory colonies from which so much of our knowledge of genetics is derived. Thus, genetic influences may be extremely subtle and, because of the combinatorial possibilities inherent in polygenic cumulative effects, much more difficult to identify than the dramatic mutations of major genes. "The genotypic constitution is but one of a large number of causal components whose dynamic interactions ultimately establish the pattern of the individual's psychosocial responses. Unless genotypic deviations from the norm are extreme . . . we would not expect them to be in any important way *determinative* of psychosocial phenotype." What a long way removed from the impression of genetic omnipotence against which Child and Watson argued! The major problem is how variability arises, and a true union of genetics which the other life sciences still awaits its Einsteinian conceptual revolution.

The second paper, by Robert B. Livingston, is entitled: "How Man Looks At His Own Brain: An Adventure Shared By Psychology and Neurophysiology." The first major part of this paper contains an informative and well-written precis of aspects of modern neurophysiology most relevant to behavioral study. The last part, however, might be disturbing to a modern psychologist who had confined his professional reading to psychological journals. On the question, "How do we consider mind and the brain?" the "new" neurology seems to be replaced by a slightly confused nineteenth (or sixteenth) century mental philosophy. After saying that ". . . dualism is undesirable also because it fosters professional, intellectual and conceptual isolation among

scientists who are trying to understand the whole existence" Livingston says: "We do know, however, that perception is definitely a mental act," and asks, "Where can we divide the nervous system to hedge off the limits of mind?" In his preface, Koch remarks that "biologists always were naive!" An effective comingling of neurophysiologist and psychologist could well be lubricated by requiring of the former a reading of Ryle's *The Concept of Mind*.

A similar contribution, disappointingly brief, is the chapter by Gerhardt von Bonin, the distinguished neuroanatomist. In fifteen pages he comments on character and the brain, consciousness, brain and mind, speech, attention, emotions and free will. He is not hopeful of ultimate anatomization of mind. "In a sense, there is really no intelligent question that one can ask about the relation of the two sciences of physiology and of psychology."

FROM both Livingston and von Bonin, one gains no feeling of deep or meaningful contact with the literature of modern psychology. The same cannot be said of Karl Pribram, whose essay is next. Under the title "Interrelations of Psychology and the Neurological Disciplines," he discusses many of the same "mind and brain" problems within the framework of his hypotheses about brain function which have appeared elsewhere within recent years. Methodologically he proposes a "psychological, subjective behaviorism" with psychological concepts derived from two sources: observation of behavior and introspection. When these are collated, it then is apparent, according to Pribram, that "concepts that result from observations of behavior are likely to be trivial unless they are initially close to those derived from introspection." Pribram here seems to confuse the natural history phase of scientific thought with its later formal structure. It could be argued that a reason for so much that is not only trivial, but misdirected, in psychological theorizing and research has grown out of just this procedure of going from introspection to problem definition. One contemplates with fascinated horror Pribram's recommendation that "the scientist . . . should ex-

plicitly follow a path hitherto reserved for the artist: i.e., enactment and re-enactment of his internalized representation of the world . . . the horizons hitherto reserved for the poet and the humanist become the frontier of behavioral science."

THE LONG look at biological science from within the field of psychology is more than adequately introduced by I. T. Diamond and K. L. Chow in their chapter on "Biological Psychology." Staying close to empirical findings, they discuss biological factors involved in the study of learning. For these working physiological psychologists a major problem of biological psychology is not, as it appeared to be for the neuroanatomist and neurophysiologist, to find the chain binding mind to body, but rather to study physiological variables in behavior at whatever level appears appropriate. "Reduction" or "level of description" should not be made an issue, since in biological science as a whole, the study of processes at different levels has been unifying rather than diversive. "Function is not reduced by being related to structure."

The paper of R. C. Davis, "Experiment and Theory in Physiological Psychology" really deserves a review of its own. Davis finds it difficult to define boundaries for physiological psychology, either within psychology or in its relations to other fields of inquiry. "The outcome might have been foreseen since, without some recognized discontinuity in the universe, there can be no clear subdivisions in the science which describes it; the investigator may well let his thoughts follow wherever questions lead . . . psychologists may be too much concerned with the protocol of science."

The best statement of the dominant theme of the whole of Volume 4 is the following quotation from Davis:

"This very principle of *nil obstat* may be the trademark of physiological psychology. Among psychologists there is a difference of creed on this point; if one likes, it can be taken to separate the physiological psychologist from others. Rather par-



adoxically, it may be said that the non-physiological psychologist is one who looks upon his subject as a distinct and independent science. The physiological psychologist, on the other hand, does not feel that he is clearly separate from scientists in general or confined to any special territory. Apparently without reflecting on the matter very much—or at least without being very articulate about it—he follows his strong and simple faith that an event, like a piece of machinery, is best understood by taking it apart. His inclination is to dissect and analyze everything into smaller components—reflexes, nerve impulses, impinging forces, chemical reactions, and so on—and he finds his work mingling with that of other disciplines. This analytical trend is certainly a common one in science, and one finds in each discipline a tendency to move downward: The physiologist turns to biochemistry, the chemist turns to physics, and the sciences all spill over at their lower edges . . . Far from being a subsection of general experimental psychology, in this view, physiological psychology simply becomes the less-circumscribed way of viewing the whole subject.”

### Mostly Physiology and Sensation

*Psychophysics and Neurophysiology*,  
by BURTON S. ROSNER

*A Quantitative View of Neuroelectric Events in Relation to Sensory Communication*, by W. A. ROSENBLITH and EDA B. VIDALE

*Sensory Processes and Their Relation to Behavior: Studies on the Sense of Taste as a Model S-R System*, by CARL PFAFFMANN

*Some Interrelations among Physics, Physiology, and Psychology in the Study of Vision*, by FLOYD RATLIFF

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NELSON YUAN-SHENG KIANG

who is Staff Member of the Research Laboratory of Electronics at MIT and Director of Research at the Eaton-

*Peabody Laboratory at the Massachusetts Eye and Ear Infirmary. He has been actively engaged in research in neurophysiology since 1950 when as a graduate student he joined W. D. Neff's laboratory at the University of Chicago. He is considered by his colleagues to be either a neurophysiologist or biophysicist but he still considers himself a psychologist, for it is behavior that interests him. Since moving to Boston in 1955 he has been trying to obtain quantitative relationships in the auditory system, with particular emphasis on stimulus coding mechanisms.*

THESE four chapters represent sincere efforts to characterize the sensory psychology of today. The main frame of reference chosen by all four authors is neurophysiology in general and electrophysiology in particular.

Far more important than minor differences in emphasis is the essential agreement in viewpoint among the various authors. To all appearances, there is consensus that:

1. The study of the senses is now a multi-disciplinary activity.
2. The study of the senses is no longer merely a science of receptors but of the brain as well.
3. Sensory performance and neurophysiology must both be studied quantitatively in order to establish their relationship.
4. The relation of sensory performance to other behavioral functions is now open to attack by experimental means.
5. Fully as important as the explosive development in techniques are the exciting reformulations and revisions of classical concepts.

Perhaps this harmony is a valid commentary on the maturity of this field of psychology. Perhaps it only reflects the fact that all five authors are based in the Northeastern part of the United States.

All experimental scientists collect bits and pieces of prejudices, generalizations, and even philosophies that determine their intellectual attitudes toward their respective fields. It is truly a formidable task to make explicit the many loose, unsystematized, often half-baked, notions that form the frontier

of systematic knowledge. One great difficulty is that cogent observations frequently look like platitudes when in print. For example, Rosenblith and Vidale point out in their cautionary summary:

“Regardless of the particular S-R relations under study, there is little reason to assume that we have discovered neural substrates of the organism's ability to handle sensory information so long as we merely examine neuroelectric events ( $R_\phi$ ) that occupy only a small fraction of the total time required by the organism for the actual processing ( $R_\psi$ ).”

Who could possibly disagree with this warning? and yet how few recognize its significance. Again when Pfaffmann points out that cognitive and informational functions are not the only behavioral functions of sensory systems, every sensory psychologist would agree. And yet the literature in this field is such that many students might well think otherwise.

The danger in reading these articles is that, like most good advice, the simple statements seem trite. The deeper meanings lie well hidden and may be accessible only through personal experience. One can only hope that the unquestioned scientific competence of the authors will stimulate reading that is more than cursory. Certainly the articles were not intended to be light reading. The reader finds very little humor to relieve the didactic dryness of unrelenting exposition. For the authors this is apparently a serious subject and is so treated.

Rosner gives a tortuous description of his view of the relations between psychophysics and neurophysiology. This chapter will benefit those who are well educated in one or the other field but who are relatively unacquainted with modern ideas in the other. Rosenblith and Vidale bring fresh views of both neurophysiology and psychology from the direction of communications biophysics. Although this is an area that may well supply many of the ideas and workers in future studies of complex systems, it is still too early to predict the ultimate influence of these ideas on psychology. Pfaffmann and Ratliff both choose the illustrative approach. Pfaff-



mann's chapter is a deceptively straightforward description of the way in which psychologists have arrived at ever-broadening views of the taste system. Ratliff presents a rambling, somewhat impassioned, plea for the breakdown of barriers between conventionally separate fields. He uses studies on the visual system to point out the pertinent contributions from many sciences.

It is encouraging that none of the authors felt that sensory psychology could be described without the inclusion of much data from experiments. This is an implicit comment on the state of the science. Because of the close similarity in orientation of these authors, it might have been more edifying to select from a broader range of views. The editor might also have eliminated some of the overlap in these articles such as the short reviews of electrophysiology in two separate chapters.

The bibliographies are useful for the further orientation of readers and appear to have been carefully selected. For graduate students and nonspecialists, these four chapters constitute fair and representative reviews of certain aspects of modern sensory psychology. The nature of the material and presentation is such that readers without background experience in experimental work will be severely handicapped in attempting to understand these chapters. However, for serious workers in sensory physiology or psychology, these chapters contain many points for further reflection and may well reward concentrated study.

### Mostly Perception

*Notes on Some Interrelations of Sensory Psychology, Perception, and Behavior*, by C. H. GRAHAM and PHILBURN RATOOSH

*Perceptual Experience: An Analysis of Its Relations to the External World through Internal Processings*, by KARL ZENER and MERCEDES GAFFRON

*Perception and Related Areas*, by FRED ATTNEAVE

*Perception and Transactional Psychology*, by WILLIAM H. ITTELSON

Reviewed by JULIAN HOCHBERG

*who is Professor of Psychology at Cornell University and who, since his graduate student days at Berkeley with Tolman, Brunswik and Warner Brown, has been productively busy in the area of perception. In addition to his many journal articles, he has produced one book and is working at the moment on another. The completed one, due to appear at about the same time as the review, will deal, naturally, with perception, and will be one unit of the Prentice-Hall series Foundations of Modern Psychology. The book in progress is Perception, Attention and Communication.*

THE LAST section of this volume (ca. 31%) comprises four chapters on perception, highly diverse in length and quality. According to Koch's introduction, "... perception is by way of becoming the 'basic' field of psychological interest and the foundation field of its conceptualizations—indeed, that has by now almost certainly supplanted learning in these respects . . . [this should] be viewed both in celebrant mood (there is much to celebrate in the return of the repressed) and critically" [p. xxxii]. Unfortunately, the official return to popularity after half a century of exile reveals less clear recognition of the interrelationship of purpose, problem and procedure than one might have hoped for in this 'basic' field, in which the goals of the entire enterprise have so repeatedly been questioned.

C. H. Graham and Philburn Ratoosh, in "Notes on some Interrelations of Sensory Psychology, Perception, and Behavior" asserts the methodological desirability of studying sensation and perception in terms of the general behavioristic relation,  $R = f(a, b, c, \dots n \dots t \dots x, y, z)$ , where  $R$  refers to the overt response;  $a, b, c, n, t$ , to the conditions of stimulation; and  $x, y, z$ , to the conditions of the subject. The aim is to predict behavior. Thus, they object to Stevens' supraliminal scaling because the procedure of averaging absolute judgments loses sight of the actual behavior: when subjects say "red"

or "nine," these are only responses to predict—not measured numbers to manipulate mathematically. However, they do not oppose all constructs of "rational accounts" which (like Hecht's theory of brightness-discrimination) provide a framework into which different  $R$ s might be fitted. Thus, tachistoscopic thresholds in word-recognition experiments are held inappropriate because, in the ranges employed, increasing durations do not affect discrimination of the number of dots. What permits such generalization about the very different  $R$ s used for discriminating words and dots? Knowledge of those sensory laws which limit 'perceptual discriminations' (e.g., failure of the Bunsen-Roscoe law past 0.05 seconds) allows us to relate one set of  $R$ s to the other, and such relationships presumably show that differences between sensory and perceptual discriminations are operationally meaningless.

However, the distinction between sensation and perception seems to be abandoned here, at the cost of restricting our attention almost wholly to sensory variables. This distinction was not merely a matter of 'operations' to the Structuralists, in the days before perception was banished; it provided a program, as well: a relatively small number of sensory elements were to be found from which all of our perceptions of the world could be constructed. This gave both a goal to the study of sensation, and a finite task to the study of perception. Does the "behavioristic" formula also provide a finite program for perceptual research? In fact, it contains no program at all, by itself, nothing to tie together all of the possible  $R$ s, or reduce the infinite number of possible terms to the right of the equal sign.

KARL Zener and Mercedes Gaffron on the other hand, make no concessions to the intervening behaviorist decades, in "Perceptual Experience: an Analysis of Its Relations to the World through Internal Processing," a report can never simply be considered as behavior, and years of attempting this restriction has left us with oversimplified descriptions of experience. We must attempt to



identify phenomenal dimensions more adequate to actual situations outside the sensory laboratory; addressed to this point, apparently, are 42 pages of the phenomenological differences between normal and reversed photographs and etchings. Some of these reported effects, and the asymmetries of the perceptual system to which they point, are very provocative, and suggest innumerable experiments.

Here again, however, the purpose of these methodological prescriptions is not clear. What makes *phenomenal reports* so important? What should we do with them? Are they more predictive of adjustive behaviors than are, say, 'cognitive judgments?' If this were true, as this paper asserts (and, conversely, if looking *behaviors* can simply be determined from phenomenal reports about them, as is assumed here), this would be extremely important—and would provide an objective correlate of phenomenal experience. William Ittelson, too, in emphasizing the 'creative' nature of 'perception' and its dependence upon unconscious assumptions ("Perception and Transactional Psychology"), simply declares that we 'necessarily' act in terms of what we perceive. No evidence, however, presently supports this relationship of behavior to phenomenal report.

Perception is treated as a construct, *inferred* from behavior, in Fred Attneave's occasionally brilliant essay on "Perception and Related Areas." This treatment is diametrically opposed to that of Graham and Ratoosh. To Attneave, for example, Stevens' scales provide an occasion for 'calibrating' the response as an index of the percept: we must penetrate past the phenotypic scale to the underlying appearance. No aggregate of unidimensional psychophysical scales is adequate, however, to encompass the perceived world of objects and events in a set of indirect measures. Attneave compares the alternatives of analyzing perceived objects by the intersection of psychophysical variables, and by classes of *elements*, and concludes in favor of the latter (wisely, I believe: in terms of variables, the perceptual world is analyzable in an infinite number of ways; in terms of elements, the task is limited).

Elements (called 'psychons' in an apologetic footnote: the lack of euphony should not obscure the proposal's merit) demand combining laws, and hence association models are invoked. This last is somewhat too pat, I think, since "The facts of perception are nearly all Gestalt facts" (p. 648), and the only 'hard' attempt to face up to the Gestalt destruction of the Structuralist associational program has been the quasi-informational approach which Attneave, a founder, has—curiously enough—ig-

nored in this paper, and which must surely shape the nature of the appropriate combining laws.

Of the four perception papers in this section, I can discern the outlines of a coherent, substantive programmatic purpose only in this last one. Let us hope that the field of perception will fare better now that it is in favor; surely, its return to popularity cannot be due to any great change in its own accomplishments.

## A Measure in Search of a Theory?

H. A. Witkin, R. B. Dyk, H. F. Faterson, D. R. Goodenough and S. A. Karp

*Psychological Differentiation: Studies of Development.* New York: Wiley, 1962. Pp. v + 418. \$7.95.

*Reviewed by* EDWARD ZIGLER

*All five authors of this book hold positions in the Department of Psychiatry in the College of Medicine of the State University of New York, where Herman Witkin is Professor and Director of the Psychological Laboratory, and where Dyk, Faterson, Goodenough and Karp, each coming from a different background and direction, now work with him. Witkin is best known for his earlier work on the 1954 book Personality through Perception. The reviewer, Edward Zigler, is now Assistant Professor in Yale's Department of Psychology and Chairman of the Child Development Program. He did his graduate work in clinical psychology at the University of Texas; an internship connected with that training carried him to Worcester, Massachusetts, where he came in contact with Heinz Werner's approach to developmental psychology and with Leslie Phillip's developmental approach to psychopathology.*

FOR a number of years Witkin and his collaborators have been investigating the correlates of a field-depend-

ent-independent dimension in perception, a dimension originally defined by the individual's penchant for utilizing either visual or bodily cues on such perceptual tasks as the rod-and-frame test and the tilting-room-tilting-chair tests. Employing the view that this dimension is indicative of the person's basic orientation and therefore underlies his functioning in a number of areas, these investigators have correlated scores obtained on these perceptual tests with scores obtained on a variety of other perceptual, cognitive, and personality tests and on certain non-test behaviors as well.

The typology constructed on the basis of this work has been heralded both for the simplicity of its categorizing principle, i.e., a set of highly reliable scores obtained on standard perceptual tests, and for its considerable predictive efficacy in respect to a variety of behaviors. If valid, such a typology would be of great value. It would be marvelously efficient to administer a few simple perceptual tests and then be able to predict with a respectable degree of confidence the examinee's body



image, degree of dependence, problem-solving behavior, impulse control, major psychological defenses and cognitive controls, activity level, attitude towards and interaction with other persons, as well as the type of pathology to which he is susceptible. Furthermore, a system which makes possible the prediction of such a variety of behaviors must by necessity illuminate some essential aspect of human functioning. There has been the undeniable suggestion in Witkin's work that his instruments are somehow tapping the very wellsprings of behavior and that a careful "job analysis" of these instruments will result in the construction of a comprehensive theory of human behavior.

It is clear that an evaluation of this effort must involve both an assessment of the efficacy of the predictor variables employed and an evaluation of the theory ultimately constructed on the basis of the empirical relationships discovered. The first possibility for such an evaluation came some years ago with the publication of *Personality Through Perception*, the bulk of which dealt with studies involving the relationship between the field-dependency measures and scores obtained on several projective tests. Although a number of interesting relationships were discovered, the authors' interpretations of them were rendered questionable by their failure to deal adequately with the issue of validity in respect to the projective tests employed. Furthermore, the theoretical superstructure built on these relationships left much to be desired. The explanatory schema finally advanced was more characteristic of clinical case construction than of rigorous psychological theorizing. In this schema Witkin and his collaborators ultimately mediated all empirical relationships discovered with two rather amorphous constructs, "active coping" and "passive submission." Nevertheless, the sheer number of empirical relationships reported was impressive, the plea for an holistic approach appealing, and the promise of new findings impelling.

It is in *Psychological Differentiation* that the recent empirical findings of Witkin, his immediate collaborators

and others, are presented and a new theoretical formulation advanced. This book is both important and disappointing. Its importance lies in the fact that it contains both a report of an extensive research project and also a scholarly summation of the findings of many investigators whose work was derived from or related to Witkin's position. The reviewer can hardly envisage that any serious worker in the areas of perception, personality development or child psychology will not pay attention to this most recent statement by an active and dedicated group of researchers. Nevertheless, many of the empirical relationships reported in this book remain questionable, interpretations of them are often unconvincing, and the new theoretical formulations have little to recommend them over those advanced in the earlier book.

In the study reported in this book, much of the effort is centered about correlating 10-year-old boys' field-dependence measures with a variety of other measures—with conventional intelligence subtests, with Rorschach, TAT, and Draw-a-Person scores, with the boys' interview behaviors, with the boys' mothers' interview behaviors. In most instances the hypothesized relationships were found, with the authors buttressing their findings further with the positive results obtained by independent investigators. However, as in the earlier book, many of the reported relationships are suspect because of the failure to deal with the validity of the personality measures and interview scores that were utilized. The little evidence on validity presented is based more on clinical folklore than on any sound validation studies of the particular measures in question. One wonders whether a variety of human-figure-drawing scores, strikingly reminiscent of those employed to assess intelligence, do indeed reflect the child's perception of his own body. In this same vein, one wonders whether the response to a single TAT picture actually measures "assertiveness," whether responses to another single TAT card measure the child's characteristic mode of handling aggression, and whether the stance taken by a child while being photo-

graphed is a valid measure of his relative activity or passivity.

In addition to the validity problem, the reviewer cannot help but feel that many of the relationships reported were highly inflated due to the contamination of certain scores by the experimenter's familiarity with the relationships previously discovered between the field-dependence scores and other behaviors. For instance, one prediction stated that mothers with particular characteristics would have field-dependent boys, while mothers without these characteristics would have field-independent boys. The presence or absence of these traits was determined and a final global assessment of the mothers was made on the basis of an open-ended interview with the mother in which the behavior and characteristics of the son were discussed at length. Regardless of the boy's perceptual scores, the interviewer could hardly help but know through the interview alone whether the boy was field-dependent or independent. It is not surprising that when this procedure was used the prediction was confirmed with point-biserial correlations as high as .85. Many of the scores employed in this study were highly subjective and global in nature and thus readily lent themselves to this type of contamination.

Despite such shortcomings, the very number of relationships discovered, their consistency and patterning, and the supporting evidence of other workers leave little doubt that there is something meaningful here. The question arises as to exactly what it is. The only safe conclusion that may be derived is that if one seines psychological waters with the net provided by Witkin's perceptual measures, a number of correlational fish will be caught. However, the very nature of the net remains elusive. In the earlier book, its success was attributed to its being an indicator of the degree of active coping. In the present book, Witkin sees the central process in the field-dependence dimension as being the ability to overcome an embedding context. On the other hand, Gardner and his colleagues, fishing these same waters, attribute the predictive success of the field-dependence dimension to a selectiveness-of-



attention factor and offer evidence in support of such an interpretation. Witkin specifically rejects such a view and offers some evidence in support of this rejection.

It is the reviewer's view that the empirical relationships found between field-dependence measures and many of the scores employed by Witkin are due to the common relationship between all these scores and general intelligence as defined by standard intelligence tests. Such a view is supported by the relationship repeatedly found between field-dependence measures and full scale intelligence quotients. Many of the TAT, Rorschach, figure drawing, and behavioral measures found to be related to the field-dependence scores are obviously related to general intelligence and in some instances have been employed by others as tests of general intelligence. Witkin attempts to negate the importance of this issue by demonstrating through factor analytic techniques that in his groups of boys the field-dependence measures are significantly related to certain subtests on the WISC (Block Design, Picture Completion, and Object Assembly) and not to others (Vocabulary, Information, and Comprehension). Witkin then combines the three subtests found to be correlated with his field-dependence measures into an "intellectual index" and the three tests found not to be positively correlated into a "verbal index." The peculiar names given to these composite scores reflect Witkin's repeated effort in this book to characterize as nonintellective any measure unrelated to his field-dependence measures. Once these two indices were formed, they appeared in correlational matrices presented throughout the book. Not once does Witkin present the relationship between his many measures and his subjects' total intelligence scores. It also appears that Witkin's verbal index returns to haunt him. We discover (p. 193), contrary to Witkin's theorizing, that the verbal index is more highly related to certain personality measures than is the intellectual index. Even more embarrassing is the finding that in one of his groups a highly significant correlation was found between an index of the

field-dependence measures and the Stanford-Binet Vocabulary score.

This discussion is merely to assert that if all one wants to do is obtain correlations he may do better with a general intelligence test than with Witkin's field-dependence measures. However, psychology's goals involve more than the dredging up of significant correlations, and one must note that our standard intelligence tests have never adequately been conceptually related to any major theory of human behavior. The reader might still prefer Witkin's approach, if it were true that the theory extrapolated from the empirical relationships truly illuminated the nature of human functioning and, by extension, the nature of intelligence. But it is precisely here, in their theoretical efforts, that the Witkin group promises so much and delivers so little. All of the relationships found are ultimately explained with that venerable developmental concept of differentiation. Differentiation to Witkin is nothing more nor less than Werner's orthogenetic principle stating that development proceeds from a state of relative globality and lack of differentiation to a state of increasing differentiation, articulation, and hierarchic integration. Werner has been explicit in treating this principle as an untestable assumption having the status of an heuristic definition. Its value lies in directing thinkers towards the examination of a variety of circumscribed developmental processes, which could then be represented in the theoretical matrix by a number of constructs of the middle range. In this book, differentiation is treated as an explanatory concept, and in an effort to make it explain everything these investigators succeed in making it explain very little. The only lower order construct clearly emanating from the differentiation notion is decontextualization, and with it Witkin seems to feel he has presented an imposing explanatory edifice. Thus, the field-dependence measure is related to the Object Assembly subtest because both require the individual to overcome an embedding context, and this ability increases as the individual becomes more differentiated. There is really no need to employ the concept of differentiation, since

all of its meaning seems to be exhausted by the concept of decontextualization. This equation of differentiation and decontextualization leads Witkin to the completely untenable conclusion that a variety of verbal behaviors are at best only minimally related to developmental processes. Such a conclusion runs headlong into the impressive work of Werner and Piaget.

This reviewer believes that our understanding of cognitive or perceptual styles would be enhanced by placing them within a developmental framework. Indeed, many investigators are making just such an effort. The concept of differentiation could have been used as the bridge to join Witkin's interesting empirical work with that of such investigators as Piaget and Werner. We would then have had a real synthesis of independent research efforts and certainly an enrichment of the developmental viewpoint. However, there is no real point of contact between Witkin and contemporary developmentalists, and because of this the concept of differentiation loses the strength that the work of other investigators has given it. The truth of the matter is that Witkin wears awkwardly the mantle of a developmentalist. He is much more interested in the stability of mediational structures than he is in their change over time or in the construction of a theory capable of encompassing such change. More than the loose application of the differentiation concept is required before Witkin's findings can be incorporated adequately in a developmental framework. One can only conclude that over the years the theorizing surrounding the field-dependence dimension has moved a surprisingly short distance away from the global and toward the articulated.



*It has often been said that, to make discoveries, one must be ignorant. This opinion, mistaken in itself, nevertheless conceals a truth. It means that it is better to know nothing than to keep in mind fixed ideas based on theories whose confirmation we constantly seek, neglecting meanwhile everything that fails to agree with them.*

—CLAUDE BERNARD





# Russian Individuals Differ

B. M. Teplov (Ed. and Co-author)

*Tipologicheskie Osobennosti Vysshei Nervnoi Deyatel'nosti Cheloveka* (*Typological Characteristics of Higher Nervous Activity in Man*). Moscow: Academy of Pedagogical Sciences, Vol. 1, 1956, Pp. 411; Vol. 2, 1959, Pp. 228.

The author, Boris M. Teplov, is a Professor at the Psychology Laboratory of Individual Differences of the Psychology Institute, Academy of Educational Science in Moscow and holds the title Honoured Scientist, bestowed by the Soviet Union. The first reviewer, Richard Lynn, graduated in psychology from Cambridge University and stayed on there for graduate work, doing his PhD dissertation on anxiety. Since then he has been teaching at the University of Exeter. The second reviewer, Dr. E. I. Boiko, is director of the laboratory of higher neuro-dynamics at the Institute of Psychology in Moscow and the editor and co-author of two collections of experimental studies: *Materials on the Study of the Conditioned Reflex Basis of Man's Psychological Activity* (1954) and *Questions of the Study of Higher Neuro-dynamics in Connection with Psychological Problems* (1957). He has also written a monograph, *Reaction Times in Man, History, Theory, Contemporary State, and the Practical Significance of Chronometric Studies* (in press).

Reviewed by RICHARD LYNN

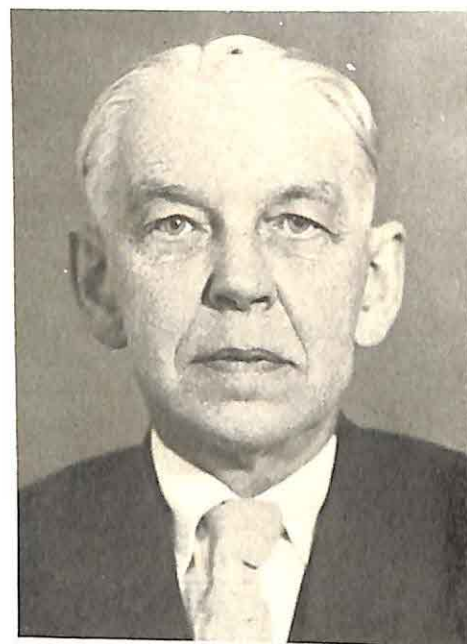
THESE two volumes present a series of experiments which have been carried out over the last decade at the Institute of Psychology in Moscow, under the direction of Professor B. M. Teplov.

In his introduction, Teplov presents an historical review of Russian work on individual differences and a discussion

of general methodological issues. Assumptions about the existence and nature of the hypothetical variables of strength, equilibrium and mobility, underlying individual differences in behavior, are taken over from Pavlov. Briefly, strength means the degree to which the nervous system can withstand stimulation without reacting and is essentially a matter of sensitivity and thresholds; individuals with weak nervous systems have low thresholds. Equilibrium refers to the relative predominance of excitatory or inhibitory processes. And mobility means the speed with which the individual can make shifts from excitatory to inhibitory states and vice versa.

The general methodological approach will be acceptable to western psychologists working on this type of problem. Teplov states a preference for studying responses that are both involuntary and as simple as possible, arguing that in complex and voluntary tasks too many variables enter into the experimental situation. Drugs are also used.

A good example of the kind of simple response with which the Russians have been working is the conditioned photochemical reaction, used as a measure of the "strength" of the nervous system. The experimental procedure is as follows: The subject is first dark adapted and his absolute visual threshold measured; a flash of light is then presented, which has the effect of lowering the subject's sensitivity; this lowering of sensitivity is conditioned by presenting a neutral stimulus before the flash, but with continued reinforced trials the



B. M. TEPOV

conditioned stimulus has less and less effect. Individuals with weak nervous systems show this effect more markedly, since they are more prone to generate protective inhibition. These individuals also have low absolute thresholds, show a rise in threshold when a distracting stimulus of different modality is presented (strong subjects show a fall in threshold), and are more susceptible to the effects of caffeine.

The conditioned photochemical reflex is also used for the investigation of the equilibrium of the nervous system, i.e., the degree to which the excitatory or inhibitory processes predominate. Essentially the procedure here has been to build up a conditioned photochemical reaction and then measure the speed with which subjects extinguish the reaction with non-reinforcement, discriminate the conditioned stimulus from a second similar stimulus, and generate conditioned inhibition to this second stimulus. The theory behind these experiments is that the excitatory processes are used in conditioning and the inhibitory processes in discrimination, in conditioned inhibition and in extinction. The individual differences supported this theory by showing that those subjects who conditioned fast had great difficulty in extinguishing, discriminating and in forming conditioned inhibition. Some other characteristics of subjects with strong excitatory proc-



esses are that they have long negative after-images, have difficulty in acquiring a motor skill because of their over-reactivity, and show errors of over-estimation in drawing lines.

The experiments on mobility have been somewhat disappointing. A number of possible indices of mobility have been investigated, such as the speed with which the subject can alternate positive and negative responses to a pair of stimuli, form delayed conditioned responses, and show both short latencies for after-images and short after-images. The measures of mobility do not inter-correlate and the conclusion is drawn that mobility probably does not stand up as a unitary construct. One investigator of mobility reports a correlation between critical flicker fusion and the latency and length of a negative after-image, but it would seem that the CFF could be interpreted as a further measure of equilibrium.

It is well known that Russian psychologists have, at any rate until recently, been somewhat innocent of statistical knowledge and this is true of the papers in these two volumes. Data which would most conveniently be reported in the form of correlations is somewhat laboriously presented in tables from which the reader has to make up his mind about the plausibility of the claims. However, there is no doubt that if correlations had been computed they would in many cases have been quite strikingly high, since in many cases subjects differentiated on one criterion are differentiated on another with virtually no overlap at all. On the other hand, since the experiments for the most part compare the reaction of subjects on measures of the same hypothetical nervous process, it is not clear how far strength and equilibrium are independent parameters of nervous activity. Some of the measures of weakness have something in common with measures of the predominance of excitatory processes, and caffeine is sometimes used to make the nervous system weaker and sometimes to alter equilibrium by increasing the excitatory processes. To the psychologist trained in the west the whole enterprise seems

to cry out for a factor analytic study.

It should perhaps be remarked that someone in Teplov's laboratory has recently been teaching himself (or herself—nearly half of this team are women) how to do correlations and factor analysis, using Thurstone's and Fruchter's books. The first experiment using these statistics has now been published (Rozhdestvenskaya, Nebylitsin, Borisova and Emolaeva-Tomina, 1960) the investigators reporting a number of correlations of the order of 0.6 and 0.7 between measures of the strength of the nervous system. There will be few workers on individual differences in the west who will not envy correlations of this size. Factorial analysis confirms the theory that there is a general factor of the nervous system's strength.

The interesting question remains: how far do these constructs of strength and equilibrium correspond, under different names, to constructs formulated independently in the west? The only western work discussed at any length by the Russians is that of Spence and his associates on the variables affecting individual differences in conditionability. V. D. Nebylitsin shows a good knowledge and understanding of this work and suggests tentatively that the Russian construct of strength-weakness may correspond to the western construct of anxiety, weak individuals being those who are anxious. Another possible correspondence which Teplov's associates are at present considering is that between equilibrium and Eysenck's personality dimension of introversion-extraversion and the postulated differences in excitatory-inhibitory balance which is assumed to underlie it. There are many striking similarities in the two theories, such as the connection between speed of conditioning and duration of visual after-effects, which would seem to demand further investigation.

In conclusion it is the reviewer's impression that the Russians are on to something in these findings and experimental techniques and that the west has something to learn from them. Reading these books strengthens the impressions of recent visitors to Russian laboratories that a good deal of careful, competent and sophisticated experimen-

tal work is going on in Russia and it is to be hoped that the work will be replicated and extended in the west.

*(This review was prepared in the frame of activities supported by National Science Foundation grant G 19469, awarded to Dr. Josef Brozek.)*

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*Reviewed by E. I. BOIKO*

ONE of the most important aspects of contemporary psychology is the complex of problems that relate to psychological personality, including questions connected with the study of temperament and character, inclinations and abilities. Differential psychology, that is, the study of individual psychological differences, has a special place in this context because of its great theoretical and practical importance. From the theoretical point of view, for example, it is very important to know the causes for the variety of human character and inclinations, and also of the various and, most important, changing potentialities of individuals in different external and internal conditions. The practical importance of these problems hardly needs any special comment, but here we cannot omit to mention such important general tasks as the working out of scientifically based criteria for personnel selection and the individual approach to training and education.

Professor B. M. Teplov and his colleagues approach the study of individual psychological differences by the experimental investigation of the typological properties of the nervous system. In this respect they have followed in the traditions of the work of Pavlov on animals, but they have tried to work out their own special system of experimental methods through which it has been possible to study the typological features of the higher nervous activity of man. The results of this vast work are published in these two collections of experimental investigations.

In the first of these is a long article



by Teplov ("Some Questions of the Study of the Higher Nervous Activity in Man and Animals"), in which the history of the investigation of the typological properties of the nervous system in the Pavlovian school is examined in detail and certain new hypotheses are put forward for experimental study. The path from physiology to psychology is regarded as not the only possible approach, but as one of a number of possible approaches. But on this path, in Teplov's view, the following fundamental problems inevitably arise. In the first place, it is essential to understand as clearly as possible the physiological content of the properties of the nervous system, and also to work out an adequate number of experimental methods. One of the most important techniques of such an investigation which has a laboratory character is generally acknowledged to be the comparison of data attained by different methods on the same subjects.

In the second place, it is important to study the psychological manifestations of each of the properties of the nervous system not only separately, but also in their typical combinations.

The first of these problems obviously has to be solved experimentally and the second through the study of various kinds of life indices, by such methods as systematic observation, interviews, natural experiments, and so on. Naturally, here we come across a series of difficulties, since the typological properties of the nervous system, as well as their combinations in the form of habits and abilities that each man acquires in the course of his life, are hidden and marked by innumerable conditioned reflex associations. It is precisely for this reason that Teplov holds the following to be indispensable conditions for the successful typological interpretation of life indices. First, the examination of these indices as a whole in comparing one with another; second, the study of the intimate process of the formation of these life indices and not only the final result. "It would be a mistake to think," says Teplov, "that the path of non-experimental determination of types of nervous system is the easiest path. It is easier only in the sense that it does not require the presence of a labo-

ratory and special equipment, but it makes quite as many demands on the investigator, if not more, than laboratory study, namely creative thought, prolonged and thorough collection of material and the ability to treat that material scientifically."

As an explanation of the way in which life indices can be used to find out the basic properties of the nervous processes (i.e., of their strength, equilibrium and mobility in Pavlov's sense), one of Teplov's colleagues, N. S. Leites, has conducted a special investigation of a number of senior school boys over the course of two years. The boys were under constant observation in school, at home, at games, meetings, concerts, stadiums, etc. As a result the author arrives at a probable psychological characterization of the basic properties of their nervous system and basic types (temperaments).

But for Teplov the experiment remains the chief method for the study of types of nervous system. The nature of the properties of the nervous system generally speaking is only revealed through laboratory investigation and so in the last resort all life indices have to be founded on experimental data.

To set out here the full contents of the experimental work of Teplov's laboratory is quite impossible. So we shall take a fleeting glance at only the two following questions. First, the direction which B. M. Teplov's work takes in developing the Pavlovian study of the types of nervous system; and second, what new theoretical problems arise in the course of this work?

**B.** M. TEPOV makes a detailed examination of the two different meanings of the term 'type,' which are often confused. The first is type as a characteristic picture of behavior, while the second is type as a complex of the properties of the nervous system. With Pavlov there was a gradual change from the first usage to reliance on strictly experimental measures of the properties of nervous processes. In this respect B. M. Teplov firmly follows Pavlov and sets his colleagues the task of working out experimental methods for the study of typological properties.

Following Pavlov, there are first the strength of the nervous processes in their capacity to sustain highly intense stimuli and prolonged stimuli without destroying the normal working of the large hemispheres; mobility of the nervous processes as the capacity of the cells of the cortex to transfer with greater or less ease from a state of excitation to inhibition and back again and in a wider sense as the general characteristic of quick mobilization, irradiation, concentration, etc.; thirdly, equilibrium as a derivative property consisting of the predominance of one process or the other in strength and mobility.

In Teplov's laboratory about ten different methods have been evolved for the experimental study of these fundamental properties and their various manifestations. A specially difficult problem was the study of the strength of the nervous processes in man. At the present time Teplov's colleagues V. I. Rozhdestvenskaya, V. D. Nebylitsyn, M. N. Borisova, L. B. Ermolaeva-Tomina and others are conducting an inter-comparison of data on the strength of the nervous system with the help of methods they have worked out. The basic experimental indices emerging as a result of this study are: first, the capacity to endure frequent, repetitive and concentrated excitation (the threshold of transmarginal inhibition), measured by the method of extinction with reinforcement of the conditioned photochemical reaction with a visual and a conditioned auditory stimulus; second, the threshold of transmarginal inhibition in relation to one short stimulus in conditions of exhaustion and excitability heightened by caffeine, measured by the ergographic method; third, the degree of concentration of the center of excitation in the visual analyser with stimuli of low and medium strength, measured by the inductive method (the interaction of weak stimuli in the visual field); fourth, the threshold of irradiation of excitation in the visual analyser measured by the inductive method with the administration of caffeine; fifth, the threshold of irradiation of excitation in the visual analyzer measured by the inductive method with the use of a technique for assessing the total excitation in the



(cortical) center elicited by an additional stimulus; sixth, the same method with the exhaustion of the nervous cells on which the main stimulus is acting; seventh, the change in visual sensitivity with simultaneous auditory stimulation and in auditory sensitivity with simultaneous visual stimulation; eighth, visual and auditory sensitivity; ninth, change in visual and auditory sensitivity under the influence of caffeine.

In the study of the properties of the nervous system of man the problem arises of so-called partial properties, which are typical of one analyzer or one of the acting functional structures, and the question of the relative advantages and disadvantages of the various properties and types. In Teplov's laboratory special techniques have been discovered which make it possible to discover the relative strength of the nervous processes in the visual and auditory analyzers and also to measure the relative predominance of one or other of the two signal systems (called by Pavlov the artistic and reflective types). Then we must not forget Teplov's hypothesis that the weak nervous system has not only the negative property of low working capacity of the cortical cells, but also the positive property of high reactivity and high sensitivity. This hypothesis is getting more and more support from the work of V. D. Nebylitsyn.

In conclusion, the transfer from the general properties of the nervous system and types of nervous activity to the study of special human types is one of the most complex and difficult problems on which the laboratory on individual and typological differences, directed by Professor B. M. Teplov, is working. The Soviet Government has shown its high regard for B. M. Teplov's work by conferring on him the distinguished title Honoured Scientist.

*(This review was prepared in the frame of activities supported by the National Science Foundation grant G19469, awarded to Dr. Josef Brozek. The Russian text of Dr. Boiko's review was translated by Dr. R. Lynn.)*



*The imposing arguments of science represent the highest degree of intellectual certainty yet achieved by the mind of man.*

—JUNG



# Roaming his Fief

George Casper Homans

*Sentiments and Activities: Essays in Social Science.* New York: Free Press of Glencoe, 1962. Pp. 326. \$6.50.

Reviewed by EDWIN PAUL HOLLANDER

*The author, some of whose many phases and facets are identified in the review, is Professor of Sociology in Harvard's Department of Social Relations. E. P. Hollander, the reviewer, is a Columbia trained social psychologist who has served as a research psychologist in the Navy, who has taught at such varied places as Carnegie Institute of Technology and at Istanbul University and who now is Professor of Psychology and Director of the Social Psychology Graduate Program at the State University of New York at Buffalo. Besides his recent Buffalo efforts to build a center for social psychology he has co-edited, with Raymond G. Hunt, a forthcoming book Current Perspectives in Social Psychology (Oxford University Press).*

GEORGE HOMANS is perhaps best identified as the author of two established contributions to required reading in social psychology, *The Human Group* (1950) and the recent *Social Behavior: Its Elementary Forms* (1961). He is also known to be a man of uncommon style who possesses a special deftness that conveys the otherwise abstruse in a disarmingly readable line, often with considerable punch.

The seventeen papers reprinted in the present volume are divided into three groups reflecting the historical development of Homans' interests from history and social structure, to anthropological analysis, and then finally to small groups, the present focus of his attention. Social science is his fief and he relishes the roaming of it, poking at this and that sprout, and calling off the

landmarks. Entirely capable of moving from one interest to another with remarkable adroitness, he is represented here by rather wide attainments.

It is in his autobiographical introduction to this collection, however, that he is at his supreme best. For forty-nine lively and tightly drawn pages, he offers trenchant observations, revelations, and some surprises. His pungent assessments of intellectual developments and of associated personages in his own and related fields are illuminating in several ways: we see how, as a Harvard College graduate in English Literature, he became a sociologist; we become a bit acquainted with men like Elton Mayo, Bernard de Voto, and Lawrence Joseph Henderson, who shaped Homans' early training; we are carried along into his later relationship with colleagues in sociology and social psychology; and, with his frank assistance, we learn something of his biases and predilections. Among other features of this introduction, the reader will find an especially searching discussion of "functionalism," a central school of thought in sociocultural study. Also there is here an account of Homans' growing affinity for Skinnerian psychology. Not least of all is his incisive commentary on his Harvard colleague, Talcott Parsons, a section noteworthy for its unusual balance of grace and candor.

There is abundant humor to be found in his writings, especially in "Giving a Dog a Bad Name," a BBC lecture on the state of English sociology. And there is consummate scholarship, as in "Marriage, Authority, and Final Causes," originally published as a small book with David M. Schneider. Both humor



and scholarship characterize his commentary on naval leadership in "The Small Warship," a paper based on his war-time experience as a ship's captain. Here, after disclaiming any special talent a sociologist might bring to a leadership function, he goes on to a number of quite penetrating observations on features of naval leadership.

Two papers represent the seedlings for Homans' newest venture, *Social Behavior: Its Elementary Forms*, a book containing the lessons with which he is most likely to be identified. These papers, "Status Congruence" and "Social Behavior as Exchange," together with two other inclusions covering his noteworthy study of clerical workers in a utilities firm, the "cash posters," afford a good introduction to his formulation of interaction in terms of an exchange of rewards as against costs in the prevailing social circumstances. It is here that he finds the notions of operant conditioning congenial and draws upon them.

Since his inclinations lie in the direction of field observations and interviews, it is not surprising to see these presented in a more favorable light than are the matters of attitude assessment and experimentation, topics he sees as characteristic of social psychology. Especially in the paper, "The Strategy of Industrial Sociology," does he push this distinction quite far, though he is not grudgingly parochial about it. Indeed, a continuing thread throughout this volume is his commitment to universal principles governing social behavior. "The field of investigation in the social sciences that has come to be called the study of small groups is not, in my view, properly so called. Small groups are not *what* we study, but *where* we often study it . . ." It is essential, he says, ". . . to show the relation between the results of experimental work done under laboratory conditions and the results of quasi-anthropological field research on what those of us who do it are pleased to call 'real-life' groups in industry and elsewhere. If the experimental work has anything to do with real life—and I am persuaded that it has everything to do—its propositions

cannot be inconsistent with those discovered through the field work."

Arguing for a combination of approaches, he feels no method can go far wrong that puts human behavior under scrutiny and he adds, impishly, that we should be wary lest social scientists ". . . come to do any mad thing in order to avoid being at the pains of studying men."

## 198 Brains

R. S. Allison

*The Senile Brain: A Clinical Study.*  
Baltimore: Williams & Wilkins,  
1962. Pp. vi + 288. \$10.00.

Reviewed by LISSY F. JARVIK

*The author is identified in the review which was composed by Lissy Jarvik, a native of the Hague who came to these shores to take her MA and PhD in psychology from Columbia University before going into medicine. At present she is a mother, a doctor's wife, and Assistant Clinical Professor, Department of Psychiatry, College of Physicians and Surgeons at Columbia University. Her numerous research articles have dealt primarily with gerontology and psychiatric genetics.*

IN *The Senile Brain* the author shares with his readers the wealth of clinical knowledge acquired in nearly 30 years of devotion to the field of neurology. As early as 1938 Dr. Allison was appointed to the staff of the Royal Victoria Hospital, Belfast, and in 1947 he was named the physician in charge of the newly opened Department of Neurology. His local experience was supplemented by his wartime service at a neuropsychiatric center of the Royal Navy where his interest in psychiatry was aroused. Indeed, he pursued studies in this field and was awarded the diploma in psychological medicine in 1943. In addition to his clinical practice Dr. Allison has given clear evidence

of a scholarly bent, as shown by his co-authorship of Whitla's *Dictionary of Treatment* (8th edition 1938 and 9th edition 1957). He is a foundation member of the Association of British Neurologists and the recognition of his accomplishments by his colleagues is reflected in his election to the presidency of the Section of Neurology of the Royal Society of Medicine for the current year (1962-1963).

Thus, it is a privilege to read the discussion of the 198 cases which form the basis of Dr. Allison's book. The author uses the rich case material to present his views and to introduce the student to the details of the physical, psychological, and neurological examination of various types of patients afflicted with mental disturbances. He devotes a considerable amount of attention to the distinction of various symptoms and attempts to relate clinical symptomatology to underlying pathophysiological processes. He lays particular stress upon his observation that specific lesions can frequently be found to account for diffuse mental impairment and cautions against attributing such impairment to generalized senile changes. The book should prove to be useful as a text to students in medicine, neurology and psychiatry, while students of psychology may, upon perusal, become aware of the organic pathology often underlying mental disturbances, either intellectual or emotional.

Though less than one-half of the patients included by Dr. Allison were either over the age of 60 or suffering from presenile psychoses, the title of the book serves to highlight the attempt to compare symptomatology due to cerebral tumors and cerebrovascular accidents in younger age groups with that of the presenile and senile disturbances. The character of the treatise under discussion can be described no better and in no briefer terms than those employed by the author in the opening sentence of his preface: "The work is principally a record of personal experience." The reviewer should like to add that this personal experience has been remarkably rich and recorded by an unusually astute observer.



# Much Ado About Much

Harold Borko (Ed.)

*Computer Applications in the Behavioral Sciences: The Role of the Computer as a Research Tool in the Behavioral Sciences.* Englewood Cliffs, N.J.: Prentice-Hall, 1962. Pp. 633. \$8.75.

Reviewed by DONALD W. TAYLOR

The editor, Harold Borko, received his PhD from the University of Southern California in 1952, served for four years in the U. S. Army as a clinical psychologist, worked a year as a training specialist at the Rand Corporation and, in 1957 joined the System Development Corporation, where he is a human factors scientist. The reviewer, Donald Taylor, is a Harvard PhD who taught at Harvard and at Stanford before moving to Yale where he is now Professor of Psychology. During 1961 he served as Sigma Xi National Lecturer for the Pacific Area, speaking on "Psychological Studies of Thinking" on 25 campuses from San Diego to Seattle and from Pocatello to Honolulu. During 1961-62 he was Research Fellow in the Center for Cognitive Studies at Harvard University. For the past twelve years, both at Stanford and at Yale, he has been engaged in research on thinking, including problem solving, decision making and creativity, with support both from ONR and the Ford Foundation.

SOME years ago, an acquaintance predicted: "The computer will prove to be as important a tool for the social sciences as the microscope has been for biology." Those who find this prediction surprising or even absurd may, if they can make their way through this book, begin to understand why at least some find the prediction quite plausible.

Those lacking prior acquaintance with computers, however, are unlikely to find

this book the best introduction. *Digital Computers in Research* by Bert F. Green, Jr. would probably provide a better introduction for two reasons. Both perhaps stem from the fact that 23 individuals have contributed to the book edited by Borko. First, the level of difficulty of the individual chapters is quite varied. The first seven by the editor himself, for example, should be accessible to the naive but interested reader. But a number of chapters, such as that by J. H. Ward, Jr. on "Multiple Linear Regression Models," or that by L. A. Hiller, Jr. & R. Baker on "Computer Music," or the one by J. T. Culbertson on "Nerve Net Theory," are likely to require rereading even by an individual with considerable experience in using computers.

The second reason for preferring Green's book as an introduction is that it is an integrated presentation limited to those concepts and procedures fundamental to an understanding of the use of computers in research. The present book presents a much wider variety of information. In part, this may be quite desirable even for the naive reader. At many points, however, such a reader may be reminded of the apocryphal child who wrote a one-sentence review of a book on turtles: "This book tells more about turtles than I care to know." If so, he is of course free to skip to the next section.

Given its possible limitations as an introduction, this book will nevertheless be a valuable addition to the library of any behavioral scientist seriously in-

terested in computers. Indeed, the book is likely to be most useful as a reference for individuals who already have some knowledge of the field. Although the quality of the chapters by individual contributors does vary, it is in general high. There are occasional statements to which exception may be taken. Indeed, views expressed in one part of the book are sometimes contradicted in another. For example, there is the impression given by Borko (pp. 52-53) that machine translation from one language to another is highly developed; this contrasts sharply with the view cited by D. G. Hays (p. 395) in his chapter on "Automatic Language-Data Processing" that machine translation "should be discussed in hypothetical sentences." Nevertheless, the book as a whole presents a broad and competent view of the ways in which computers are being used in the behavioral sciences.

Several chapters, including the one on "Data Processing in Psychological Research" by E. L. Kelly & J. C. Lingoes or on "Canonical Analysis" by P. B. Koons, Jr., make clear that analyses of data which would not otherwise be feasible, become quite practical with a computer. But a computer has other uses. The larger part of "Studies in Perception" by B. W. White is devoted to the discussion of generation of stimuli. At least eight chapters deal with what may be the most important use of the computer for behavioral scientists, namely, the simulation of behavior. Included among these eight are the chapter by W. R. Ashby on "Simulation of a Brain," by R. F. Simmons on "Synthesis: Toward Computer Synthesis of Human Language Behavior," and by S. C. Rome & B. K. Rome on "Computer Simulation Toward a Theory of Large Organizations."

Some of the chapters on simulation provide a kind of catalogue of activities in the area; an example is the rather unsatisfactory one by O. Benson on "Simulation of International Relations and Diplomacy." Others are devoted almost exclusively to the description of a particular simulation. J. Feldman, for example, limits his discussion of "Computer Simulation of Cognitive Processes" to binary choice behavior. In this



## COMPUTER SIMULATION OF PERSONALITY - Frontier of Psychological Theory

By SILVAN S. TOMPKINS, *Princeton University*, and SAMUEL MESSICK, *Educational Testing Service*. This book contains the papers and formal discussion presented at a

conference on computer simulation and personality theory held at Educational Testing Service and Princeton University in June 1962. 1963. 325 pages. \$5.00.

## SCIENTIFIC CREATIVITY: Its Recognition and Development

Selected Papers from the Proceedings of the First, Second, and Third University of Utah Conferences: "The Identification of Creative Scientific Talent," supported by the

National Science Foundation. Edited by CALVIN W. TAYLOR, *University of Utah*, and FRANK BARRON, *University of California, Berkeley*. 1963. 419 pages. \$7.95.

## SIX CULTURES: Studies in Child Rearing

Edited by BEATRICE B. WHITING, *Harvard University*. The first volume in a projected series by social scientists from Harvard, Yale, and Cornell Universities, this book studies the degree to which the treatment a child receives in the first years of life determines his behavior, perception of the

world, philosophy, religion, and code of ethics. It is based on reports from mothers in an Okinawan village, several Philippine hamlets, African homesteads, a Rajput courtyard in India, a Juxtlahuacan courtyard, and a New England town. 1963. 1017 pages. \$12.50.

## MEASUREMENT IN PERSONALITY AND COGNITION

Edited by SAMUEL MESSICK and JOHN ROSS, *University of Western Australia*. Based on a conference held at Princeton University under the auspices of the Educational Testing

Service, this book brings together research papers on personality measurement by specialists with diverse backgrounds. 1962. 334 pages. \$5.75.

## A GUIDE TO PROGRAMMED INSTRUCTION

By JEROME P. LYSAGHT and CLARENCE M. WILLIAMS, both of the *University of Rochester*. This book explores "how" and "why" to program. The guidance presented is

the result of experimentation by the authors who have integrated and synthesized their knowledge and skill into a practical process for programming. 1963. 130 pages. \$3.95.

## SAMPLING TECHNIQUES, Second Edition

By WILLIAM G. COCHRAN, *Harvard University*. This book gives comprehensive, up-to-date coverage of the theory underlying modern sample survey methods (for both small-

and large-scale surveys) with proofs of the results. It underscores the relation between sample survey theory and the main body of statistical theory. 1963. 413 pages. \$9.95.

## THE MENTAL PATIENT COMES HOME

By HOWARD E. FREEMAN, *Brandeis University*, and OZZIE G. SIMMONS, *University of Colorado*. Presenting findings from a lengthy research study, this book reports on 649 mental patients in the year following hospitalization. Based

on data gathered by social workers in interviews with the patients' family members at two points in time, the book documents the success and failure of patients in the community. 1963. 309 pages. \$7.95.

## MULTIVARIATE PROCEDURES FOR THE BEHAVIORAL SCIENCES

By WILLIAM W. COOLEY, *Harvard University*, and PAUL R. LOHNES, *University of New Hampshire*. An eminently useful book, *Multivariate Procedures* considers the applica-

tions of multivariate statistical analyses to research in the behavioral sciences. It contains "ready-to-run" computer programs for multivariate analysis. 1963. 211 pages. \$6.75.

## PERSONALITY AND SOCIAL SYSTEMS

Edited by NEIL J. SMELSER and WILLIAM T. SMELSER, both of the *University of California, Berkeley*. This collection of articles in personality and social systems analyzes the

empirical interaction between these two systems. 1963. Approx. 632 pages. Prob. \$7.95.

## THE CORPORATION AND ITS PUBLICS: Essays on the Corporate Image

Edited by JOHN W. RILEY, JR., *The Equitable Life Assurance Society of the United States*. This series of papers by a group of distinguished authors was originally presented at

a symposium at Gould House, Ardsley-on-Hudson. They explore the content of the "corporate image" concept. 1963. Approx. 208 pages. Prob. \$4.50.

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latter instance, the unfortunate result is that the reader receives no adequate impression of the extensive work of Allen Newell, J. C. Shaw, and Herbert Simon, or of a number of others in this general area. In fact, for this reviewer, who believes that the most interesting and fruitful work on simulation has been that on cognitive processes, the absence of an adequate general discussion of the area represents the most serious omission in the book. One could wish that the chapter on "Can Computers Think" might have shown more understanding of the problems involved in answering this question.

One final word. Make sure that your dean or provost reads the chapter by Charles Wrigley on "The University Computing Center." This step may do much to facilitate the local use of computers. In any case, the editor and contributors deserve credit for what should be a very useful book.

## Computers: Servants or Simulators?

Martin Greenberger (Ed.)

*Management and the Computer of the Future.* New York: Wiley, M.I.T. Press, 1962. Pp. xxxvi + 340. \$6.00.

Reviewed by MELVIN R. MARKS

Martin Greenberger, editor of this volume, is Associate Professor of Industrial Management at M.I.T. Earlier, he co-authored *Microanalysis of Socioeconomic Systems: A Simulation Study*. The reviewer, Melvin R. Marks, is Professor of Business Administration at the University of Rochester. He holds a degree in law as well as a PhD in psychology, the latter from Tulane University. His primary training was in experimental psychology but now he seems to be a member of the diminishing tribe of general psychologists. His professional work has been mostly in the area of industrial psychology, first

as a personnel research man in the Adjutant General's office of the Army, then for Psychological Research Associates. His return to the academic setting is recent.

IT HAS been alleged that some rat psychologists are more concerned with teaching their charges to act human than they are in discovering the general laws of learning. In the series of lectures and discussions reported in *Management And The Computer Of The Future*, some participants allege that computer specialists are more concerned with making computers simulate human behavior than they are with adapting computers for human use.

Like most analogies, this one has its flaws. Rats differ from computers in some respects. It seems unlikely that the vaudevillian approach to comparative psychology will generate either significant new knowledge of behavior or new techniques of training rats for the stage. On the other hand, one cannot overestimate the rate of propagation of the blast from the technological explosion. It was the last century that someone argued for the dissolution of the Patent Office on the ground that everything had been invented.

It seems that, although computer specialists may be more interested than they should in demonstrating what computers can do rather than what they should do, they tend to discover, perhaps serendipitously, that computers are appropriate for tasks which men do poorly, or perhaps not at all. For example, it may be that the world chess-champion may never be beaten by a computer; however, techniques developed in the course of programming a computer to play chess suggest new ways of understanding problem solving behavior, or ways of creating mathematical models which simulate—and can be used to regulate—the flow of city traffic.

*Management And The Computer*, representing as it does many points of view, takes both an optimistic and a pessimistic view of computer usage in the future. Only two of its chapters are directly relevant to this problem, the balance dealing with science-fictioning (soon to be true) about the libraries of

the future, advances in programming, the simulation of human thinking, the computer in the university, etc.

On the pessimistic side, it will take very competent managers to use adequately the computer of the future. It may be that the harassed executive is not the proper person to manage a computer. If this be true, how can appropriate status and rewards be given to the person who can? The increased use of computers poses problems in centralization and decentralization, time-sharing, economics, storage and retrieval methodologies, etc. Computers should not necessarily do the things that they can do. At any moment in time, what a computer should do is a function of the nature of the problem, the capability of the computer, and the ingenuity of the programmer. It is probably more true with computers than with men that the results are no better than the data. It will always require a human being to plan the investigation and interpret the results.

Not all would agree with this cautious philosophy from which emanates the injunction to avoid jumping on the automation bandwagon. A year or so ago, I learned that computers were good for processing large amounts of data along routine procedural lines, e.g., generating statements of account for all of the customers in Smith's Department Store; however, they were ill-adapted to search all of the records to produce the debit balance for Mrs. Jones whenever she happened to request it. Last week, when I reported this gem to a class, one of the students told me that today there are computers for which Mrs. Jones' request is entirely appropriate and economical.



The first impression that one gets of a ruler and of his brains is from seeing the men that he has about him. When they are competent and faithful one can always consider him wise, as he has been able to recognise their ability and keep them faithful. But when they are the reverse, one can always form an unfavourable opinion of him, because the first mistake that he makes is in making this choice.

—MACHIAVELLI





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## A Look-in on Drop-outs

Solomon O. Lichter, Elsie B. Rapien, Frances M. Seibert and Morris A. Sklansky

*The Drop-Outs: A Treatment Study of Intellectually Capable Students Who Drop Out of High School.* New York: Free Press of Glencoe, 1962. Pp. xiii + 302. \$5.50.

Reviewed by MERLE HUGH ELLIOTT

All four authors of the present volume are members of the staff of the Scholarship and Guidance Association, a Chicago counseling agency that has been in existence since 1911. Lichter and Rapien are social workers, Seibert is a clinical psychologist and Sklansky is a psychoanalytic psychiatrist. The reviewer, M. H. Elliott, did his doctoral work in the late twenties with Edward Tolman at the University of California and since then has worked in a wide variety of intellectual fields and geographical settings. A major intellectual and professional focus has centered on the public schools; he carries credentials as teacher, supervisor, administrator and psychologist. He is past president of both the California State Psychological Association and the California Educational Research Association.

THERE is increasing concern about the large percentage of pupils leaving high school before graduation. Many of these may become the delinquents, the maladjusted, the marginally employable in the community.

The project described in *The Drop-Outs* was intended as one approach to this problem. The book reports an intensive effort to study and counsel a selected group of adolescents. Potential drop-outs were referred to the agency by schools. Services included regular interviews, assessment, and diagnostic evaluations of child and parents. Other supplementary help such as financial assistance, psychological testing, vocational counseling, employment place-

ment, and psychiatric consultation was available. The project was an all-out effort to see what could be accomplished under maximal conditions with a particular group of troubled and troubling adolescents. An experimentalist would wish for a more rigorous research design, an untreated control group, etc. The book is written from the point of view of the caseworker and the conceptual framework is that of the psychiatrist and psychiatric social worker steeped in the Freudian tradition.

It is easy to praise this report for its enlightening analysis of the problems of individual children and to criticize it for the limited sampling of students upon which it is based. Findings such as the following are presented:

"The girls managed relatively well in elementary school and became rebellious and aggressive about the time of high school entry" (p. 62-63).

"Unsatisfactory school experience and concomitant discouragement are often used by the youngsters as excuses for leaving school but these are excuses only and not the real reasons" (p. 65).

"The typical girl under study is rebelliously 'acting-out.' Her standards are fundamentally sound. She attaches value to school and genuinely wishes to graduate. She appears to be 'sicker than she is' and 'sexier than she is'" (p. 133 ff.).

These brief statements quoted out of context, and lacking the rich background of discussion fail to do justice to their source, but they do illustrate the kinds of material presented. There



is no question of the significance of some conclusions for the understanding and treatment of certain adolescents. However, one might ask whether workers with a different theoretical orientation would arrive at the same characterizations of potential drop-outs. And would the generalizations be different if based upon a more representative group of school-leavers?

THE AUTHORS of the book are fully aware that they were dealing with a highly selected sampling of potential school drop-outs. They are quite explicit in describing the sample. The study "was designed to be a systematic, clinical analysis of a group of intellectually capable high school students who were potential school-leavers." It is a study of one very special group and not of a representative sampling of potential drop-outs. In fact, it is possible that the segments of the population contributing the greatest numbers of drop-outs were omitted from the sample. Since the title of the book could lead the unwary or uncritical reader to assume that drop-outs in general were studied, it seems worthwhile to specify further some of the sampling limitations. All 105 pupils in the original study group were white, predominantly from middle-class homes, and of average or better academic ability. Over half came from unbroken homes. The majority had not been involved in 'actionable offenses.' Further selection came about from the fact that one-third dropped out of counseling before the fourth interview. Seventy pupils were left in the Treatment Group on which most of the statistics are presented.

Does the study discover whether intensive casework can improve the adjustment of potential drop-outs? Some pupils do improve in their adjustment and the authors throw some light on which these are likely to be. On the specific matter of continuation in school it is reported that out of 56 pupils who were sixteen years of age or older (i.e., over the legal school-leaving age) 26 were still in school or had graduated when the treatment project was closed. Trained caseworkers under favorable circumstances can be of assistance with some children. Training and great skill

are obviously required, and even these are not always sufficient. An educator looking at the outcomes cannot be very optimistic about the possibility of a substantial lowering of the drop-out rate in his school.

An obvious conclusion of the study is that many potential school drop-outs also drop out of treatment. The thirty-three percent loss in the original sample has already been mentioned. On page 83 we find the further discouraging note that seventy-one percent of the remaining *Treatment Group* left treatment too soon.

The book presents interesting material, indicates some of the emotional problems of selected adolescents, and shows the varying meanings of the school as stage, battlefield, friendly shelter, or enemy camp. Illustrative case material is well presented in a separate chapter. The chapter, "Collaborative Work with the Schools," deserves special mention for its practical and useful discussion of working relationships.

## Beclouded African Ids

J. C. de Ridder

*The Personality of the Urban African in South Africa.* New York: Humanities Press, 1961. Pp. xvi + 180 + 16. \$5.00.

Reviewed by ROBERT A. LEVINE

The author, J. C. de Ridder, studied at Rand University, South Africa, where he received his PhD in 1957. He is at present engaged in research on African leadership and on executive training methods. The reviewer, Robert A. Levine, is a psychological anthropologist primarily interested in research on personality in African societies and in other cross-cultural settings. He is presently an assistant professor of anthropology in the Committee on Human Development at the University of Chicago. He has

done a great deal of field work, the first in connection with the Harvard-Yale-Cornell study of socialization in six societies. His field work has taken him to Kenya (1955-57), and Nigeria (1961-62).

SOUTH AFRICA has produced the largest proportion of worthwhile psychological investigations of Africans, primarily because it is the only place on the continent where a considerable number of professional psychologists are at work. The National Institute of Personnel Research in Johannesburg, under the direction of Simon Biesheuvel, is well-known for its systematic studies of intelligence, aptitudes, and attitudes, and university psychologists such as S. G. Lee (now in England) and R. C. Albino have made creditable contributions in the field of personality research. This study by de Ridder, however, is below the usual standard of South African psychology, lacking the methodological rigor of Biesheuvel's group and the theoretical insight and sophistication of a personologist like Lee.

Mr. de Ridder designed a T.A.T. of nine cards to help a Johannesburg bus company select drivers; it was administered to 163 drivers and the results were analyzed blind to predict which men were good accident and disciplinary risks. Against objective criteria of accident liability and disciplinary record, the T.A.T. predictions were (we are told) successful. No details of this study are given in the book. Subsequently, the T.A.T. was administered to 2500 African job applicants. In this book Mr. de Ridder purports to summarize the common personality characteristics of these 2500 male subjects in relation to their cultural environment and social system. This could have been a most interesting and significant study. However, it is so unsystematic—with an alternation of anecdotal material and obscurely founded global generalizations—that it could not be judged successful in the most modest definition of its purpose.

Among the more serious flaws of the volume are: the complete absence of quantitative evidence, the journalistic quality of the account of urban African life, the failure to relate ethnographic



background meaningfully to test results, the lack of any mention of previously published T.A.T.'s designed for use with Africans in South Africa viz., those of Lee (1953) and Sherwood (1957).

Eighty pages are devoted to illustrative excerpts from the T.A.T. protocols, with comments which point out gross behavioral themes in the social life of Johannesburg Africans: gang violence, racial discrimination, striving for money, the use of American slang, sexual promiscuity. Rarely does the author attempt to analyze in any depth the personalities of the individuals in this social setting, and when he does he produces a spate of ethnocentric clichés: the urban African is "immature," "exhibitionistic," "easily led" (p. 111), and "retains within himself a great deal of the uninhibited, uncontrolled primitive" (p. 158).

Although he does not cite any previous psychological studies of Africans, there is evidence that Mr. de Ridder has been influenced by one of them. In the 1959 Hoernle Memorial Lecture in Johannesburg, Biesheuvel applied Riesman's typology of character structure to urban Africa and coined the term "id-directed self" to characterize the transitional state of township dwellers. Mr. de Ridder concludes a year later:

"In this cultural reformation, the tradition-directed society of the rural areas . . . is being supplanted by what may be called an individually-directed society with a predominating id-complex" (p. 154).

How unfortunate that his analysis of 2500 T.A.T.'s did not enable the author to go beyond the sweeping, surface formulation of Biesheuvel!

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## Psychosis with Statistics

William Goldfarb

*Childhood Schizophrenia*. Cambridge, Mass.: Harvard University Press, 1961. Pp. vii + 216. \$4.50.

Reviewed by IRA M. STEISEL

*The author, William Goldfarb, holds both a PhD and an MD degree, the former from Columbia Teachers' College, the latter from Cornell Medical College. He is presently Associate Clinical Professor in Psychiatry at the Psychoanalytic Clinic of Columbia's College of Physicians and Surgeons. Also he is Director of the Henry Ittelson Center for Child Research in NYC. He is co-author with Marilyn Dorsen of Annotated Bibliography of Childhood Schizophrenia (1956). Ira M. Steisel, the reviewer, received his PhD from the State University of Iowa in 1949 and for the next eight years carried out multiple and assorted functions at the University of Washington. There he held positions in the Department of Psychology, in the Department of Psychiatry and in the Psychiatric Clinic for Children. His research activities have focused on the behavior of children who have been given the schizophrenic diagnosis; he has sought clarity concerning those factors common to schizophrenic children and absent from the behavior of normal ones and ones who have brain damage. He is now Chief Clinical Psychologist at St. Christopher's Hospital for Children in Philadelphia.*

THE TITLE of this book is somewhat misleading in that it implies a treatise on some aspect of the topic or perhaps a comprehensive review. It is in fact a summarization of studies that have been ongoing for about a decade under the guidance of Dr. William Goldfarb. Some of the results have been

published separately and still others are due to appear in print.

Dr. Goldfarb is particularly suited for this work in light of his training both as a psychologist and psychiatrist, his previous publications on childhood schizophrenia, and his earlier research on the effects of institutional deprivation on children, work which earned him the Devereaux award in 1943 from the American Psychiatric Association.

In its conception and execution the current undertaking is indeed pioneering and holds forth a model which makes many of the publications in this area pale by comparison. The robustness of the work is revealed by its breadth and depth, by the laudable attempts to reduce bias and contamination, by the fact that it is replicable and by the quantitative treatment of subjective impressions. Statistical tests are applied, and those of us who prefer the use of more than a small number of case histories to investigate phenomena, will take comfort in a report on a clinical topic in which there are 26 childhood schizophrenics and 26 normal controls equated for age and sex.

The first goal of the investigators was to compare psychotic with normal children to see in what ways they could be differentiated. This was undertaken with the keen awareness that in many instances clinical impressions might be formed in the absence of adequate normative developmental data. Included in the battery were some techniques which specifically focused on aspects which have been reported by others. For example, L. Bender's notion of deviation



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in height and weight and motoric disturbances as well as P. Bergman and S. Escalona's finding of hypersensitivity to stimulation of schizophrenic children are investigated.

The second purpose was to determine whether there are two subtypes of childhood schizophrenia: those with organic brain damage and those without.

To fulfill their aims these experimenters utilized more than seventy measures. A number of those employed are in routine use by clinical psychologists. Others are those utilized in ordinary neurological and/or physical examinations. Some measures were especially devised for their purposes and an additional number are more in the nature of laboratory data. Included are ratings of the "adequacy" of the families.

The results are presented in forty-eight tables, including the factor analyses of the data bearing on schizophrenic subclusters (discussed in a chapter prepared by the late Irving Lorge). On the basis of the findings Goldfarb organizes the tests into three groups: (1) those that do not differentiate normals from schizophrenics nor between those with and without organic brain damage; (2) those that differentiate psychosis from non-psychosis but not between the subtypes; and (3) those that differentiate his subgroups of schizophrenia from one another and normals from psychotics.

Were it possible to take the findings at face value there would be much reason for rejoicing. Unfortunately there are a number of methodological tactics in the collection of the data and in the reporting which obscure the value that exists. In a number of instances not all children received scores on all of the tests in the battery, *viz.* the family moved, the child was uncooperative, the child was discharged, etc. To compensate for this, a score was assigned on an arbitrary basis (pp. 64-68). It would have been helpful if Dr. Goldfarb had specified in what instances and how often such prorating occurred. Two alternate solutions come to mind to undo the absence of complete data: (1) obtain more subjects for which the arbitrary assigning of a value is not required so that all subjects have scores on all measures; or

(2) base the comparisons on the data that are available. In the latter instance there might be different Ns for each measure as well as in each group.

Another disquieting feature is that the experimental group differs from the control group in terms of intelligence level as do the two schizophrenic subgroups from one another. One might legitimately wonder what other measures are affected by these disparities. The author does not discuss this to any extent.

Questions also might be raised as to the advisability of mentioning "high correlations" without quoting them; and one is disturbed by the statement that small sample theory was used when there was no accompanying statement of which tests of significance were applied to secure the "critical ratio" values (p. 69).

Despite technical deficiencies, one can not help but be pleased that this attempt was made. Perhaps through reworking of the data, further amplification by Dr. Goldfarb or by additional definitive research, clarification of issues will be achieved.

## The Mad World of Research Scientists

Bernice T. Eiduson. Foreword by Harrison Brown.

*Scientists: Their Psychological World.* New York: Basic Books, 1962. Pp. v + 299. \$6.50.

Reviewed by SHERWOOD H. PERES

*The author, Bernice Eiduson, received her PhD from the University of California in Los Angeles and now, in a different part of town, is an associate professor of psychology at the University of Southern California. She is also Director of Research at the Reiss-David Clinic for Child Guidance. The reviewer, Sherwood Peres, did his predoctoral work in industrial psychology with Robert Wherry at Ohio State University. He is currently Industrial Psy-*

*chologist for the Sandia Corporation in Albuquerque, and prior to assuming this position he worked with the U. S. Army Personnel Research Office. In both of these positions, especially the Sandia one, there has been contact with the many-faceted world, mad or not, of scientists.*

AS THE number of research and development laboratories steadily increases, there is also a similar increase in treatises telling us what scientists and researchers are really like. The present book is no exception. Its author writes, "There is a tendency to think of the scientist exclusively in stereotype and . . . an empirical study of living men currently in research science was long overdue." However, the reviewer, after reading *Scientists*, believes that an empirical study about researchers is still overdue. The book sadly misses its goal. The volume reports a clinical study, replete with T.A.T. and Rorschach protocols, of 40 male scientists working in physics, earth and soil sciences, chemistry, and in the biological and zoological sciences. All these persons are affiliated with a university or an academic installation on the West Coast.

The plan was to view the sample from a number of vantage points, using "empirical techniques," in order to develop a composite psyche for a representative research scientist. But because of the author's clinical orientation, the work bogs down in such psychological jargon as "a narcissistic character picture in a guilt-ridden man with a low anxiety tolerance."

We see the composite scientist portrayed as an individual who grew up in an atmosphere fraught with emotional overtones: his lack of close family relationships; his father infrequently at home; his mother viewed with hostility; his speech defects and physical handicaps; and religious and racial discrimination against him. The author contends that, because of these pressures, the scientist experienced long periods of forced isolation as a child. He then turned inward and experimented with his abilities and extended them. We see the scientist in his adult life as a 'loner' who is usually closeted





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—Rudolf Arnheim, *Sarah Lawrence College*  
1962; 828 pages; \$9.00 text

# Introduction to the Statistical Method

*Foundations and Use in the Behavioral Sciences*

by KENNETH R. HAMMOND, *University of Colorado*  
and JAMES E. HOUSEHOLDER, *Humboldt State College*

Enlivened by 31 numbered boxes that feed the student gems of historical interest or dramatic pronouncements about statistics, this text provides a pleasant introduction to the subject. For reasons easy to appreciate, I like the organization. It begins with its feet on the ground in the theory of measurement and goes all the way to the theory of decision.

—Stanley S. Stevens, *Harvard University*  
1962; 431 pages; \$7.00 text

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with his colleagues, emitting some strange scientific language. In his home life he is passive and, as expected, his wife runs the home quite completely, making the majority of the decisions dealing with the money, children, friends, and social obligations.

It would appear that Eiduson's "empirical study" lends credence to the most widely accepted stereotyped picture of the scientist held by the layman. Those who have studied statistics realize that when one is dealing with very small samples, despite the selection, he must be very careful not to generalize beyond the data. The author, after carefully informing the reader in the first chapter that the sample was not selected randomly nor was it to be considered average, generalizes her results to the entire scientific community. The terms, "the scientist" and "the researcher," are spread so generously throughout the chapters that they would give the uninformed reader a misleading conception of research scientists.

The author apparently writes to two quite diverse levels of readers, since, on the one hand, she goes to great lengths to oversimplify the rationale and use of the T.A.T. and Rorschach, while on the other, she uses 'clini-psychiatric' terms which only an advanced graduate student or a professional in the field would understand.

The style of the book is reminiscent of a dissertation for there appears to be an attempted justification for every hypothesis or finding. Each of the seven chapters of the book is followed by a *Notes* section having from three to nineteen detailed footnotes. This somewhat detracts from easy reading.

On the meager positive side, with a restructuring and reemphasis, the book could be an interesting study of a small sample of scientists who inhabit our universities, although generalization would be tenuous.

The study does give some insight into the psyche of the academic researcher, although, if the author's analysis of these researchers is accurate, these men would have a very difficult time adjusting to industrial research. Further, *Scientists* may give industry some clues about how to establish a research climate which is attractive to academic

researchers.

Finally, the author decries the all-out effort to change the public image of the scientist from the 'baggy pants eccentric' to the common man, a person who can and should be emulated by every American child. However, the reviewer predicts that those readers who actually work with research scientists will not accept Eiduson's picture of one.

## Man in New Environments

Karl E. Schaefer (Ed.)

*Environmental Effects on Consciousness.* New York: Macmillan, 1962.  
Pp. ix + 146. \$5.50.

Reviewed by S. B. SELLS

*The editor, Karl E. Schaefer, received his MD from Kiel University and is presently Head of the Physiology Branch, U. S. Navy Medical Research Laboratory, at New London, Connecticut. During World War II he worked on physiological studies in submarine medicine for the Germany Navy. The reviewer, Saul B. Sells, now Professor of Psychology and Director, Institute of Behavioral Research at Texas Christian University, was for ten years Head of the Department of Medical Psychology in the USAF School of Medicine, Randolph Field. He is the 1956 recipient of the Longacre Award, (granted by the Aerospace Medical Association) and in 1957 received a USAF Commendation for Meritorious Service. He has published and edited extensively in fields of aviation psychology and bioastronautics and also is the author of an introductory text, Essentials of Psychology (1962).*

WHEN man ventures outside his natural ecologic niche he encounters problems of environmental physiology that require bioengineering for life support. A concern for these problems is reflected in this unique collection of papers reporting a major section of the first International Sym-

posium on Submarine and Space Medicine, held at the U. S. Naval Submarine Base, New London, Conn., in 1958. The present volume is focused on the effects of environmental extremes in submarine and space environments on the central nervous system and behavior. The other part of the symposium is published under the title, *Man's Dependence on the Earthly Atmosphere*, MacMillan, 1962.

Most of the material included has been reported elsewhere, but not in the psychological literature. The papers are, for the most part, authoritative, well written, and admirably condensed. As a whole, this symposium presents a valuable compilation of psychobiological reviews not only of timely interest in the applied areas of space and under-sea exploration, but also of basic scientific significance.

The report is divided into two panels. The first, consisting of five neurophysiological research reviews under the title, "Effects of Various Environmental Factors on the Central Nervous System," discusses topics in aviation and submarine physiology, such as the effects of hyperoxia and hypoxia on visual system tissue (Werner Noell), oxygen requirements of the cochlear response (Kempton Wing), effects of carbon dioxide on the central nervous system (P. O. Therman), the mechanism of oxygen toxicity under various atmospheric pressures (S. N. Stein), and nitrogen narcosis (H. J. Taylor). These papers contain a wealth of empirical data, but at the same time point out a considerable lack of systematic conceptualization and investigation in environmental physiology.

The second panel consists of eight papers under the general title, "Effects of Acute and Chronic Environmental Changes on Consciousness." The problem of consciousness is broadly formulated by Hallowell Davis, program chairman, in terms of the *subjective* world of "ideas," which is contrasted with *objective* neuroanatomy and physiology. In an effort to clarify the nature of the subjective, Davis stated, "As far as I can analyze it, information is really the whole substance of the subjective world—information, ideas—whether this be the immediate sensory input, or the



memories of the past, or calculations, plans, or objectives of the future." Davis further attempted to deal with the question, "Conscious of what?", and specified recognition of self as opposed to an environment, and orientation in time and space as criteria of consciousness.

THIS dual emphasis on information processing integral to behavior and on self awareness and orientation provides an umbrella under which the contributions of the other seven panelists are related. Ashton Graybiel's important review of orientation in space, with reference to vestibular functions, discusses the role of the semicircular canals and the otolith apparatus in orientation in space and indicates areas of needed research. S. J. Gerathewohl summarized the effects of gravity-free conditions, including Russian data not generally available, and concluded that weightlessness may be more important as a psychological stress, reflecting differential human tolerances amenable to personnel selection, than as a source of physiological dysfunction. David Simons reported on the unique combination of personal and environmental factors responsible for experience of the "break-off phenomenon" on his high-altitude balloon ascent. Karl Schaefer reviewed work on the effects of CO<sub>2</sub> on consciousness in submarine medicine and L.G.C.E. Pugh reported on chronic exposure to low oxygen supply and adaptation to altitude by members of Himalaya expeditions. The ceiling for permanent living appears to be fixed at 18,000 feet. A brief summary of John Lilly's work on sensory deprivation was included. The final paper, by A. R. Behnke, reviewed the effects of nitrogen and oxygen on consciousness and presented some new data.

Both panels were followed by summaries of group discussion, which contain interesting exchanges, and miscellaneous tidbits of information. This symposium will most likely be useful to scientists and technologists in the life sciences support of aerospace and undersea programs with reference to the reviews of particular topics. It is not an integrated, systematic treatment of the topic implied in the title.

# Soviet Psychophysiological Methods

P. O. Makarov

*Metodiki Neirodinamicheskikh Issledovani i Praktikum po Fiziologii Analizatorov Cheloveka.* (Methods of Neurodynamic Research and Manual on the Physiology of the Human Analyzers.) Moscow: Vysshaia Shkola, 1959. Pp. 269.

Reviewed by IVAN D. LONDON

The author, Petr Osipovich Makarov, was in 1957 Director of the Laboratory of Analyzer Physiology in the Ukhtomskii Physiological Institute at Leningrad State University and in 1960 was associated with the Laboratory of Sensory Biophysics at the same University. Since 1930 he has been engaged in research on the biophysics and psychophysiology of the analyzers, particularly of the visual analyzer. He has based much of his work on the investigations of the biophysicist, P. P. Lazarev, and has introduced many new concepts, methods, and instrumentation in research in these fields. He is the author of *Neirodinamika Cheloveka* (The Neurodynamics of Man) (1956) and of *Neirodinamika Zritel'noi Sistemy Cheloveka* (Neurodynamics of the Visual System in Man) (1952). The reviewer, Ivan D. London, is Professor of Psychology at Brooklyn College of the City University of New York and Director of its Institute of Political Psychology. He received his PhD from Tulane University and for 25 years has pursued the study of the society, the science, and the people of Soviet Russia. He is particularly interested in Soviet psychology and related disciplines, and in the pursuit of that interest he abstracts Soviet psychological literature, conducts research on political psychology in communist societies, and has interviewed and procured data from hundreds of ex-Soviet citizens. Presently he is studying Chinese in preparation for research on and perhaps in the Far East.

IN TERMS of Western practice and definition Makarov's book is a compendium of methods and problems in

sensory psychology or psychophysiology. In any case, it is intended for use by the student of psychology as well as of physiology. Hence, its interest for us.

The volume represents a curious combination of elementary and advanced topics. In it are detailed for the first time under one cover a number of methods developed by Makarov and other Russians. New methods developed in the West are also considered. Guidance to the student is provided frequently in ample detail, but at other times in skimpy outline. Elucidation of background and topical materials is likewise uneven—in some cases the treatment is full, in others sketchy and inadequate. Elementary statistical methods are introduced in belated acknowledgement of the importance of the statistical treatment of experimental data. Unfortunately, the author's attempt to make up for the long neglect of statistics in the Soviet laboratory, while commendable, is not at all satisfactory and is marred by technical inaccuracies. In spite of the above-noted deficiencies, Makarov's book is very interesting and informative. The intrusion of political ideology and Pavlovian dogmatism is so minimal that, when they are noted, they create the impression of being dutiful inserts.

On paging through Makarov's book a number of questions come to mind: first, the book purports to be a manual developed over a period of 15 years and designed for use in the 4th and 5th years at the Leningrad State University "when students of physiology are already sufficiently prepared to carry out intelligently many complicated experiments, when they are already doing experimental work on their own for



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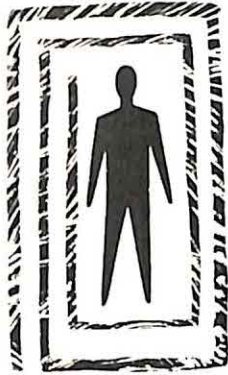
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their thesis." But judging from the elementary scope and nature of some of the exposition, they would appear little prepared for such work, at least in "sensory physiology." As a matter of fact, the author states that "one of the aims of the given manual was to acquaint the students . . . with the standard methods and procedures of research" (in this area)—matters that, along with a minimal statistical *sine qua non*, should have been, one would think, mastered previously. Here is a puzzle that possession of a plan of studies would help clear up.

Next, the neglect of statistics in the Soviet laboratory has been a notorious phenomenon for years and represents, in spite of counterstatements from foreign apologists, one of the weaker sides of the Soviet experimental picture—a judgment now openly shared in the Soviet Union, where measures to redress the situation have recently been adopted. It is usual to see in the widespread statistical backwardness, long evident in psychology and the related disciplines, a lasting consequence of the "Decree on Pedagogical Distortions" whereby the Communist Party in 1936—among other things—put an abrupt end to the testing movement and diminished drastically the role of statistics in education and psychology. But this of itself does not explain the carry-over of the neglect of statistics to the laboratory. Furthermore, it does not explain why Soviet mathematicians, who have been contributing significantly to the field, should have had so little influence on the applied working-levels. Nor does it explain the failure until recently to take serious note of whatever statistical texts were available—in the main, translations from foreign sources. Many, many questions await exploration here. The fact remains that there is once more an interest in statistics. But judging from Makarov's unhappy treatment of the subject and from what the reviewer has seen elsewhere, the effects of the long neglect will be with Soviet psychologists for some time to come.

A third point lies in the area of necessary equipment. In spite of frequent references by Makarov to alternate or simpler instrumentation, one gains the

impression that the equipment necessary for the proper conduct of the experiments discussed in the book is generally available. Yet, we know from the Soviet literature that this is not the case. As a matter of fact, Makarov himself raises the issue in a footnote in the concluding section of his book:

"Many people turn to us with the question: How to acquire the apparatus, described by us in the present manual? In line with a decision of the USSR Ministry of Higher Education and the consent of the Rectorate of the Leningrad State University, it is possible to conclude arrangements with the University to enable the conduct of suitable experiments involving the construction of the required apparatus."

How this works out in practice is a moot question.

The paucity of references to Pavlovian theory in Makarov's book, as compared to what an earlier text very probably would have contained, confirms the steady drift from an excessive Pavlovianism to a more balanced emphasis and perspective. Yet even here one feels that, despite outward appearances, matters have really gone much further. In fact, Makarov's attempts to be on appropriate occasion Pavlovian have an unconvincing air. For example, after affirming in the traditional manner the basic role of the conditioned-reflex method in research on the sensory organs, he cites verbal report of sensory quality as a fundamental instance of Pavlovian method! His general argument proceeds as follows:

"Since the verbal response is the result of conditioned-reflex connection, formed in the course of one's life, between the cells of the cortical center of one or another analyzer and the kinesthetic, auditory, and visual nerve cells which preserve the traces of words (spoken, heard, written, and seen), the method of verbal response concerning the presence of stimulation or its quality should be viewed as a special case of the method of conditioned reflexes when applied to the analyzers in man."

In other words, "the use of the second signal system . . . is necessary and indispensable, since only verbal or verbal-

motor responses give indisputable witness to the participation of the cortex in the reaction of an analyzer." The progressive weakening of Pavlovian orthodoxy in the Soviet Union is an interesting subject for exploration. There is reason to conjecture that in some instances it is a non-fortuitous function of distance from Moscow. Makarov's base of operations is the rival city, Leningrad.

Other questions come to mind, such as Makarov's criticism of Mueller's law of specific energies—a criticism which hews closely to the "Leninist line" set forth in the second edition of the *Large Soviet Encyclopedia*, but which in the context of Makarov's exposition has a perfunctory air. Enough, however, has been said. Here is a valuable book that, like so many other books from the Soviet Union, says more on occasion to the discerning reader than is spelled out on the printed page.

(This review was prepared in the frame of activities supported by the National Science Foundation grant G19469, awarded to Dr. Josef Brozek.)

## Cases for Neophytes

Arnold Buchheimer and Sara Carter Balogh

*The Counseling Relationship: A Casebook.* Chicago: Science Research Associates, 1961. Pp. vii + 234. \$4.75.

Reviewed by LAWRENCE M. BRAMMER

The first author, Arnold Buchheimer, a product of Ohio State University, is Associate Professor of Education at the City University of New York and also is Supervisor of the University's Guidance Laboratory of the Division of Teacher Education. His collaborator here, Sara Carter Balogh, is presently a counselor on student problems both at New York City Community College and at Brooklyn College. The reviewer, Lawrence Brammer, is a counseling psychologist in charge of the Sacramento State College Counseling Service



and also a professor in the counselor training program. His prior experience includes counseling at Stanford University, where he obtained both the MA and PhD degrees, and also in the Army where he worked to rehabilitate psychiatric patients.

WHAT really happens in a relationship with a client is one of the most pressing questions a neophyte counselor or therapist can ask himself. The present collection of cases, with parallel commentary, attempts to describe what happens at the verbal level. As the subtitle implies, this is a casebook designed primarily as a text for public school counselors-to-be, although it would be applicable to all psychological counseling contexts.

An important question pertaining to any casebook is what does this book do that numerous others do not do? First, the cases have a minimum of editing and most of them are taken from graduate practicums. This is a strong point in practicum teaching, because students see active cases rather than polished textbook examples of cases. The cases also represent a broad spectrum of client problems, ages (six to eighteen), and levels of skill in handling. There is always the danger that case presentations become models rather than illustrations; but the authors wisely caution against this tendency.

For the counselor in training, there is a helpful and up-to-date focus on the counselor himself as well as upon the client and his problems. There is an emphasis on the relationship between client and counselor rather than on technique *per se*; but where technique is discussed there is a neat progression from example to application to theoretical issue. The basic framework for the treatment of each of the twenty-seven cases is a) the statement of the problem, b) exploration, c) closing and d) planning. For each case there is a commentary, keyed by number to the responses of counselor and client.

While the authors disclaim the completeness of this book as a text in counseling, mentioning that it is to be used as a casebook in conjunction with a standard text, about one-fourth of the book is devoted to a simple introductory

treatment of counseling. This would pose an awkward problem for the instructor of the first course in counseling, for the theoretically oriented portion of the book, while insufficient for exclusive use, would still overlap substantially with a supplemental standard text on counseling. It is understandable, however, that the authors felt the need to provide a conceptual framework for their case analyses.

A useful feature of the book for provoking study and for initiating class discussion is the question section at the end of each case. The fact that instructors with differing theoretical orientations would have differences in opinion on the significance of the questions, or on conspicuous omissions, should not detract from this useful feature.

Since counseling and psychotherapy are such theoretically controversial fields, a legitimate question is "What is the authors' position?" There is no readily identifiable theoretical preference, unless it is the communications approach wherein the authors stress counselor awareness of the client's meanings, of his perceptions of himself and others. A further theoretical anchor is "self psychology" within the framework of phenomenology. Much of the text is devoted to inferring, clarifying, and making explicit the client's picture of himself and his method of construing his world. Some of the formulations come from the authors' own research, but they deftly avoid labeling their point of view and seem to be broadly eclectic in the best sense of this term. Such eclecticism is commendable in a text for this level of training where students tend to grasp all too eagerly for stereotyped "schools" of counseling.

The authors emphasize the quality of the relationship as the principal helping medium, as against the intellectual understanding of the client through the gathering of information. Information collecting, which is the nemesis of so many high school counselors, is seen by the authors as significant only if it relates to an understanding of the client's personal world.



# Bibliography of Nonparametric Statistics by I. Richard Savage

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# Scientific Method: Forethought or Afterthought

Russell L. Ackoff. With the collaboration of Shiv K. Gupta and J. Sayer Minas

*Scientific Method: Optimizing Applied Research Decisions.* New York: Wiley, 1962. Pp. vii + 464. \$10.25.

Reviewed by KENNETH W. YARNOLD

*The author, Russell L. Ackoff, is Professor and Director of the Operations Research Group at Case Institute of Technology where he did his undergraduate work in architecture but he received his doctorate in the philosophy of science from the University of Pennsylvania. He is the co-author (with Churchman and Arnoff) of An Introduction to Operations Research (1957) and author of Progress in Operations Research, Vol. I (1961). The reviewer, Kenneth Yarnold, now Vice President and Director of the Systems Division of Dunlap and Associates, was educated in zoology and genetics at Oxford University. He entered the field of operations research in 1937, before there was such a field, and performed operations research for the British Army in World War II until he came to Washington as the first British exchange officer at the Operations Research Office of Johns Hopkins in 1949. He joined Dunlap and Associates in 1950.*

THE AUTHORS of this book are distinguished in the practice and teaching of operations research, a field in which there has been rapid growth since World War II. In its simplest terms, this field consists of the application of every useful technique, culled from sciences and mathematics, to the problem of optimizing the way in which an organization is operated. It may be seen as the use of scientific method to illuminate the decision-maker's task: to

help him to see the probable consequences of his alternate decisions, and, by choosing the outcomes he prefers, to make the 'best' decisions. In predicting the probable outcomes, use is generally made—implicitly or explicitly—of a "model," usually mathematical, of the operations being studied, a model into which data may be inserted. Operations research may also be defined as a scientific way of attacking "practical" problems: which makes one ask "what is a scientific way?" a question which may offer another reason for this book.

The book is written largely around the decision process. It is possible to regard almost everything we do as a decision-making activity. The decisions range from the trivial ones (such as to have an olive or lemon peel in a martini) to the major decisions which, as Sir Charles Snow has reminded us (*Two Cultures and the Scientific Revolution*, Cambridge, 1959), may be vital to national survival and, at the same time, so ill-understood as to invite gross error. Research, like almost all activities, consists of making a sequence of decisions: seldom as trivial as the making of a martini, or as grave as developing the hydrogen bomb.

Evidently, one does not need a formal, complex mathematical model or a deep understanding of decision theory to decide the question of olive versus lemon. Clearly one *must* have this—and much more too—to make decisions affecting national survival. How much complexity is justified in any particular

decision-making activity? This depends not only upon the decision-maker's view of payoffs and costs associated with alternatives, but also on his degree of acquaintanceship with the scientific techniques concerned. So another reason for this book is to acquaint other scientists with decision theory.

IN *Scientific Method*, the authors give a reasonably full and clear account of the classic operations research approach to problem-solving. While little of the material is actually new, most of it will be new to most scientists. Most of it, too, will be comprehensible, to any careful reader, even if he is unsophisticated mathematically. It is hard to believe that most scientists—academic or applied—would not benefit by its study: even if they do not directly apply the methodology to their problems, it will generally illuminate their thinking about the nature of the research process, and improve their skill at it.

The authors have been plagued by the difficulty of steering a middle course between too much depth and too little. In general, the choices of depth which they have made are good ones, but they will not satisfy everybody. Experts cannot expect to learn very much about their own fields: but experts are customers for journals rather than for textbooks. Social scientists can expect to make good use of most of the materials given. Adequate references are provided to sources of fuller information.

The book is arranged in the same sequential manner as a piece of research appears to a student: it proceeds from the criterion problems, through the formulation of the research task, the derivation of models (which are, of course, no more than mathematical constructs, manipulation of which simulates nature in those respects which are important to the problem), the problems involved in using the models (such as measurements, sampling, etc.), the testing of the model, and the derivation of solutions. It also covers such topics as experimental optimization, and the implementation and organization of research, the last of which may be of special interest to social scientists.

Actually, of course, research does not normally go in the neat direct path



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## ☐ HUMAN ENGINEERING GUIDE TO EQUIPMENT DESIGN

Edited by CLIFFORD T. MORGAN, *University of California, Santa Barbara*; ALPHONSE CHAPANIS, *The Johns Hopkins University*; JESSE S. COOK III, formerly *Technical Material Information Service, McGraw-Hill*; MAX W. LUND, *Office of Naval Research*. 640 pages, \$9.95.

This authoritative reference book, conceived and supervised by a joint Army-Navy-Air Force Steering Committee, presents a comprehensive and up-to-date coverage of the field of human engineering. It covers man-machine system planning and evaluation, visual and auditory display of information, design of controls and workplaces, design for ease of maintenance, environmental effects of performance, and anthropometric data. The individual chapters are largely independent and may be read in almost any order. The emphasis is on basic facts and well established principles of immediate and direct application to design problems rather than on theoretical or academic considerations. Specific design recommendations are included.

## ☐ SYSTEMS AND THEORIES IN PSYCHOLOGY

By MELVIN H. MARX, *University of Missouri*; and WILLIAM A. HILLIX, *Navy Electronics Laboratory, San Diego*. McGraw-Hill Series in Psychology. Available in June, 1963.

The primary purpose of this book is to provide the advanced undergraduate and the beginning graduate student in psychology with a single, up-to-date source containing the basic information about systematic and theoretical problems in psychology. The approach is scientific rather than subjective or clinical. The authors provide not only the basic tenets of various classical and contemporary viewpoints in psychology but also a philosophical framework within which the tenets can be evaluated.

## ☐ PEOPLE AND PRODUCTIVITY

By ROBERT A. SUTERMEISTER, *University of Washington*. McGraw-Hill Series in Management. Available in June, 1963.

This unique combination text and readings book is the first to appear on the primary management problem of employee productivity and job performance. In an effort to clearly explain and analyze the most important factors (and the inter-relationships among these factors) influencing employee job performance and productivity, Sutermeister has integrated and synthesized the latest research findings in the behavioral sciences as they relate to them.

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suggested by the organization of the book. At the very least, it tends to be iterative: to loop backwards into itself, to redefine the problem at intervals during its solution, to re-examine assumptions, progressively refine models, and perhaps only at the end finally settle upon what was the precise question to which a partial answer has been achieved in the research.

WE KNOW only too well that people do not act logically in their daily lives: not even scientists. Do they act logically—for example, along the lines shown in *Scientific Method*—in their scientific work? A distinction may be drawn between algorithmic methods, which may be defined as both necessary and sufficient, and heuristic ones, which are not both necessary and sufficient, may not be logical, and are—at best—“plausible” in Polya’s sense of the word (*Patterns of Plausible Inference*, Princeton, 1954). Scientists, like ordinary folk, rely far more upon the “plausible” than many of them care to admit. Why is this, and is it good or bad? We know that even “simple” problems can be so enormously complex that even if algorithmic methods exist for their solution, they would take many years to compute even on an ultra high speed computer. For these, we have always been forced back on judgment, which we find hard to define, and harder still to evaluate. For many problems no algorithmic path even exists.

Concerning the role of scientific method in discovery, Cohen and Nagel (*Logic and Scientific Method*, Harcourt, Brace & Co., 1934) say “the ability to perceive . . . a problem, and especially one whose solution has a bearing on other problems, is not a common talent . . .” In other words, there is no royal road to creation.

Perhaps even more important is the thesis developed by Arthur Koestler in *The Sleepwalkers* (Macmillan, 1959), in which, by examination of such men as Copernicus, Kepler and Gallileo, he shows that our ability to recognize the meaning of our own intellectual leaps forward is strictly limited by our habits of mind and previous history. Perhaps

they look more revolutionary to us than they did to the leapers.

This is a book about scientific tools. We may read it with profit even if we share the view that scientific tools are used by scientists more in the ritual justification of their conclusions than in the reaching of them.

## Patterns in History and of Bias

W. T. Jones

*The Romantic Syndrome: Toward a New Method in Cultural Anthropology and History of Ideas.* The Hague: Martinus Nijhoff, 1961. Pp. vii + 256. 23.75 guilders.

Reviewed by NORMAN C. MEIER

*The author, W. T. Jones, is a philosopher who was educated in Swarthmore, Oxford and Princeton, who now teaches at Pomona College and who has gradually become more interested in the history of culture than in the history of philosophy. Norman C. Meier, the reviewer, while a graduate student was invited by Seashore to the University of Iowa as an Assistant and entered upon aesthetics research which was recognized by a long succession of grants keeping him at Iowa, except for periods as visiting professor at west coast universities and at the Sorbonne, working with distinction to merit the status of Professor of Psychology Emeritus. He was responsible for three Monographs in the Psychology of Art, authored the book Art in Human Affairs, (1942) directed seven research programs and currently is as busy as ever with two—in the field of special abilities and creative processes of artists.*

THIS primarily theoretical volume is a recent addition to the series sponsored by the International Scholars Forum.

The title, unfortunately, does not en-

lighten one concerning the nature of the volume. The author's objective is to propose a methodology that will afford better, perhaps precise, insight into historical trends of thinking, creative production, and such theoretical structures as metaphysics and theory in science. He proposes that the approach involve an examination of the biases existent in important personalities contributing to the culture of such given periods as the Medieval and the Romantic. Such disclosed patterns of bias predominating in any group will characterize the period. To these prevailing, dominant patterns he applies the term *Syndrome*; to the particular pattern of a given individual he uses the term *configuration*. The Syndrome represents the central tendency of a range of biases, or the predominant style preference of the period.

Seven “axes of bias” are proposed: (1) the Order/Disorder Axis (preference for system, clarity, structure—as opposed to complexity, fluidity, disorder); (2) Static/Dynamic; (3) Continuity/Discreteness; (4) Inner/Outer (self-involved versus others); (5) Sharp-focus/Soft-focus; (6) This World/Other World; and (7) Spontaneity/Process. These he regards as methodological tools for the analysis of style, as reflected in non-verbal (e.g., painting) as well as in verbal media (poetry, theories). Theoretical behavior, he asserts, may be considered as a continuum, interspersed with ‘anchor points’ (Hilgard’s ‘facts’) which are items of general acceptance. The reviewer believes “orientation” would be a more acceptable term than “bias.” In Jones’ approach, the period in psychology of Koffka, Köhler and the Swarthmore group generally, would have reflected a Continuity Bias, but the group probably considered themselves as simply oriented toward a more adequate perceptual analysis of meaningful behavior.

In practical application, a lesser precision would be expected in the arts and social science where ‘anchor points’ are fewer and there is greater preoccupation with language (Jones’ term: ‘talk’), which involves considerable subjective interpretation, yet the author feels that the question of the width of the parameters within which subjective factors



can intrude, would, by his approach, be made amenable to identification and limitation.

In considering the Arts he finds Biases 1 to 5 operating. In his discussion of History, Biases 1, 3, 6, and 7 appear. Few particular Biases are singled out in discussing the Physical Sciences, with their greater number of 'anchor points,' but in Social Science, Bias 3 (reference to Bruner and Postman, Durkheim, Benedict, Kardiner) and Bias 4 (time-motion studies) appear.

Chapter IV presents a working hypothesis which suggests patterns of biases expected in four Syndromes. The *Medieval*, for example, would be strongly marked by Static, Order, and Other-World biases; the *Renaissance* by Sharp-focus and This-World; the *Enlightenment* by Sharp-focus, Order, Discreteness, Static, Outer, and This-World; the *Romantic* by Disorder, Soft-focus, Dynamic, Inner, Other-World, and Continuity (strongly marked). The following chapter, which gives the volume its title, presents a detailed consideration of Poetry as exemplifying patterns of biases dominant in the Romantic Syndrome. Other chapters present a contrast between the Romantic and Enlightenment Syndromes in Metaphysics and in Political Theory.

IN THE reviewer's opinion the methodology has promise of bringing some measure of objectivity and elemental statistical treatment in the areas explored. Human limitations, however, would seem to pose some serious problems, one of which would be that of consensus among scholars who themselves are sometimes afflicted with bias. Another problem would be the competence of a given scholar to function effectively in areas outside his speciality, for cross comparisons. Such difficulties would not present themselves in some instances but would in others. For instance, in history of art, consensus could be expected in assigning a Soft-focus Bias to Monet, or a Dynamic Bias to Bellows. But what of Renoir's *Le Moulin de la Galette*? Would his grouping of individuals at tables or dancing, independently dispersed, exemplify a Discreteness Bias? Or would Renoir's artful placement, posturing, rhythms, easy

transition and relating of group to group and all to the whole with a perfect balance of tensions suggest unity—a Continuity Bias? An instance cited in the book of Discreteness Bias—Bellini's *Religious Allegory*—could well be considered Continuity, because the receding-return pattern bound together by plastic forms in the upper half, argue for Continuity, much as the luminous, all-enveloping treatment of cloud, rock, and other forms effectively provides the unity of El Greco's *Vice of Toledo*—rather than the river, as the author states.

It would thus seem that the task of research proposed by the methodology would be a formidable one. How does one master the problems of consensus of competent specialists in assessing the relative conformities and non-conformities? How does one control the examples in which subject-matter alone and not necessarily the style trend, dictates the Bias?

## One Tribe in Diaspora

Marshall Sklare (Ed.)

*The Jews: Social Patterns of an American Group.* Glencoe, Ill.: Free Press of Glencoe, 1960. Pp. v + 669. \$10.00.

Reviewed by BERNARD KUTNER

Dr. Sklare, the editor of this volume, is a sociologist who is a specialist in studies of racial, ethnic and religious groups, and who is currently Director of the Division of Scientific Research of the American Jewish Committee. His earlier books include *Conservative Judaism: An American Religious Movement (1955)* and *The Riverton Study (1957)*. The reviewer, Bernard Kutner, received his PhD from Harvard under Gordon Allport in 1951 and thereafter worked on the research staff of the Commission on Community Interrelations of the American Jewish Congress before

switching into social psychological research on problems of aging, interpersonal relations in medicine, and of rehabilitation. He is currently Associate Professor of Preventive and Environmental Medicine and of Rehabilitation Medicine at Albert Einstein College of Medicine, Yeshiva University. He is the principal author of 500 over Sixty: A Community Survey of Aging.

WOVEN into the complex structure of American life are a host of semi-homogeneous, partially integrated, partially assimilated and determinedly different cultural sub-groups. One of these, the American Jewish group, is the subject of this collection of writings and research reports. Scholars and students of American Jews, of intergroup relations, American minorities and of the cultural fabric of 20th Century America will delight in having a new group of readings brought together by a conscientious editor. These are the works primarily of sociologists and secondarily of psychologists and are, as the editor explains, readings in the sociology of American Jewry, not a sociology of American Jews. It is neither comprehensive nor encyclopedic in coverage (and is not so intended) but there is something here for all tastes and interests. The editor has painstakingly stitched together some previously published articles with extracts of masters and doctoral theses and dissertations, unpublished survey reports, excerpts of larger works, etc. The result, contrary to expectations from such a *pot pourri*, is generally satisfying (if one can forgive the omission of an index).

In a sense this collection reflects a major trend in modern American sociology: sociological theory and analysis is today firmly committed to empirical, usually quantitative verification substantially paralleling the development of psychological science in this respect.

The book is divided into six sections: an overly brief historical setting, a section on demography and social mobility, one on the Jewish community (in communities of varying size), another on religion, a section on identification and group belonging and a final part—a conglomeration of hard-to-classify papers on values and cultural idiosyncracies.



One is startled to find here no section devoted to reactions to American or foreign anti-semitism although individual papers include incidental references to this phenomenon. A section on the Jewish family would have served a useful purpose. We are pleased to find Strodtbeck's papers drawn from the SSRC Talent Study, and Martha Wolfenstein's paper on Jewish mothers. But there is little else on families.

On the positive side there is here presented well-founded evidence of some interesting aspects of Jewish life in America. Jewish families tend to be smaller in size than those of the general population. Although residential movement is common, among Jews, it is generally within or between large metropolitan centers rather than from urban to small-town or even suburban living. Adolescents tend to be conflicted over questions of Jewish identification. Secularization has invaded and altered religious observances. There are many such observations, the striking aspect of which are the parallels with other religious, social and ethnic groups.

AMONG the numerous items of interest are several papers dealing with various aspects of the origin and sources of Jewish liberalism in social and political attitudes and values. However, there are limitations to these excursions into intrinsically fascinating subjects. Some of the pieces are overly brief, merely alluding to the wealth of material from which they are drawn. Some, on the other hand, are satisfying in their depth and thoroughness.

The overall effect of the whole collection is on the positive side. It is not a definitive work and leaves the discerning reader with a sense of unfulfillment. Yet these thirty-three selections reveal the richness of contemporary research on Jewish life in America. It also points to significant gaps in this research, but this is not the fault of the editor. Perhaps another and future collection will open significant new areas: personality growth and change through old age; studies of the assimilative process; and studies of the Jewish lower-class (overshadowed by far by emphasis upon upward social mobility). Until such as

these appear in print this collection will stand as a strong resource among very few works on the Jews.

A minor although important aspect of this work is worth noting. After completing the various individual contributions, there is left a sense that the book fails to make clear in an integrative way that American Jews are—as are their ethnic cousins of other origins—not only a social group, but a multifaceted religious group, a well-stratified

economic group, a complex cultural group and to a considerable extent no group at all. The analytic process of examining “aspects” produces less than the desirable closure about the American Jews in the comprehensive context of the American social system. Perhaps this task was beyond the intention of the editor but some such perspective would provide the reader with the *Gestalt* which all the individual writings seek to fill in by their collective efforts.

## Assorted Imbalances on Retardation

Lawrence C. Kolb, Richard L. Masland and Robert E. Cooke (Eds.)

*Mental Retardation*. Baltimore: Williams & Wilkins, 1962. (Proceedings of the Association for Research in Nervous and Mental Disease) Pp. vii + 331. \$15.00.

Jerome H. Rothstein (Ed.)

*Mental Retardation: Readings and Resources*. New York: Holt, Rinehart & Winston, 1961. Pp. xii + 628. \$6.75.

C. J. C. Earl

*Subnormal Personalities: Their Clinical Investigation and Assessment*. Baltimore, Md.: Williams and Wilkins, 1961. Pp. v + 338. \$7.00.

Reviewed by RUE L. CROMWELL

The three editors of the first book listed here are all physicians and all have shown a persisting concern for mental subnormality. Dr. Richard Masland is director of the National Institute of Neurological Diseases and Blindness. Jerome Rothstein, editor of the second book, is a psychologist who is Professor of Education at San Francisco State College. The late C. J. C. Earl, the Englishman who authored the third book, was well known for developing the Moron Battery and for his studies using projective techniques in connection with mental deficiency. The reviewer, Rue Cromwell, received his PhD at Ohio State University in 1955 and moved immediately to George Peabody College for Teachers where he has been ever since, deeply involved in the active and

unique Peabody program of research and training in mental deficiency. The fact that he was the first chairman of APA's Ad Hoc Committee on Mental Retardation signifies his general status in this area of concern.

MENTAL retardation, whether the psychologist wishes it or not, has been and is continuing to be an area for which he is vitally responsible. The term itself is a controversial one. For the reductionistic thinker, who believes that events described in more molecular terms are the causes, the true and real bases, etc., of human performance, it is a most annoying construct. From a molar behavioral point of view, however, all those who are mentally re-



tarded may be described by a single common denominator. The Heber manual of the American Association on Mental Deficiency (AAMD) defines mental retardation as a condition of intellectual functioning below the average range, occurring during the developmental period, and associated with an impairment in adaptation. (Perhaps for the first time in the development of any clinical diagnostic system, the operational definition of terms used were given deliberate attention.)

The parents of children who are retarded (three per cent of population) seem to like the term. Congressmen institutionalize it through their legislation. The President has named his Panel on Mental Retardation. Certain private and public foundations have an eye to project proposals specifically under the heading of mental retardation. It looks as if the term—and the arbitrary circumscription of the area—will be with us for a while.

A few years ago the public interest and demand clearly outran the professional interest in mental retardation. Parent groups responded vigorously. Legislation brought new research funds. But definitive texts and reputable journals were not available. Professional personnel for certain specialized training and research functions could not be had for love nor money. (They still can't be had for love.) Some thought it was a bandwagon fad which would pass quickly. This has not happened. Thus, the professional person has often been caught with his responses down when asked to provide answers.

This is the background against which these books on mental retardation are reviewed.

Two of the three books have the title *Mental Retardation*. From such an undivided title, one expects a total compass. It is not quite like giving a textbook the title *Man*, but it is at least 3% in that direction. In neither text does one get this total treatment. Of these two, the one edited by Kolb, Masland, and Cooke presents the proceedings of a symposium on mental retardation by the Association for Research on Nervous and Mental Disease. The other, edited by Rothstein and appropriately subtitled, is a collection of readings and

resources. I wonder if the titling was chosen to increase circulation in this field where more comprehensive texts are presently being sought.

THE BOOK of symposium proceedings is an enlightened and stimulating set of papers on basic research in biological correlates of mental retardation. As if to salve conscience, papers on environmental deprivation, on cultural-familial influence and on the education of the mentally retarded are tacked on at the end. Kolb, in his preface, apologizes for the imbalance, saying that the areas of investigation moving forward with greatest promise were emphasized. To imply, as he does, that research in social processes is not moving forward dramatically is not correct. A more correct and very justifiable reason might be that the biological correlates of mental retardation were merely what the organizers of the symposium were most interested in.

The symposium very appropriately blends basic knowledge not specific to mental retardation with that which is directly specific to the area. With the dramatic developments in genetics, the papers in this area will be dated by the time the reader gets this review. Nevertheless, the initial work on chromosome counts and Mongolism by Lejeune, Turpin, and Polani will be important reading in the history of both genetics and Mongolism.

Second only in importance to the chromosome discoveries have been the discoveries of inborn errors of metabolism. The normal metabolism of the organism is dependent on a series of enzymes. Enzymes are catalytic agents of a complex structure. According to present theory each gene accounts for the presence of an enzyme; thus, the genes in effect program the metabolic functioning and unfolding development of the organism. When a defective gene occurs, the program is interrupted. The respective enzyme is not present in sufficient quantity in order to continue the chain of reactions. The metabolic substances build up. In case these substances are damaging to brain cells, mental retardation is the result. Although identified inborn errors of this kind account for only a microscopic

proportion of mental retardation, it may yet have an important pay-off in the more vaguely understood and diagnosed disorders. The symposium gave adequate attention to the identified disorders, such as phenylketonuria and galactosemia.

A theory of antibody-antigen interaction is reported in the symposium paper by Robinson, Sawyer, and Najjar. Such a theory, by people not in any way involved with mental retardation, may have a greater impact on the field than much of the work which is done within the field. This should be a sobering thought to the administrators of granting agencies who are dealing with funds earmarked for use in mental retardation.

The timely topic of radiation did not receive the usual amount of attention in the prenatal and neonatal sections of the symposium. Neither did the tumor (new growth) conditions.

In the symposium paper on the cultural-familial defective, Cooke, a pediatrician, reminds the audience that at least 75% of all retardates are not approached through any present medical concepts or understandings. In the social and psychological aspects of this vast group lie the problems which this symposium ignored.

THE BOOK of readings by Rothstein has its imbalance toward education. After some introductory readings on diagnosis, learning theory, and broad social problems, there are 34 selections on education of the mentally retarded. Their diversity would attract interest from the special educator and the school psychologist. However, they do not come successfully to grips with the problem of government vs. family's responsibility for the children so low in intelligence they can not profit from classroom instruction. Another basic question with no final answer here: should teachers of special (especially trainable) classes receive years of professional training, or can they be non-professional people screened for personality, understanding, and ability to carry out in adequate cook-book fashion the essentials of a curriculum developed and programmed by educational researchers?



An excerpt concerning problems of operational definition is taken by Rothstein from the Heber AAMD manual. The historical review by Dunn ignores medical and psychological in favor of the social and educational treatment. The readings on diagnosis, assessment, and classification would leave the psychologist and medic feeling neither furnished nor refurbished for his clinical chore. One would hope that the work of both the White House Conference and the President's Panel on Mental Retardation will soon render out of date the readings on the social problems of mental retardation. McPherson's careful review of research on learning is already out of date. More learning studies with the retarded have been done since her 1958 review than in all the years previously. A major contribution of the book is the general overview of educational and psychological research by Dunn and Capobianco.

Most books on mental retardation omit any discussion of parental counseling, a major function of psychologists and social workers in this area. Rothstein's book does not. Although genetics counseling is not included in the counseling section, there is a delightful article by Patterson giving a parent's advice to professional counselors.

THE third of the present trio of books, *Subnormal Personalities*, is the first major work ever published on personality in mental retardation. For this reason it should perhaps be forgiven for any weaknesses and shortcomings arising from the groundbreaking. C. J. C. Earl, the author, known in England for the development of the Moron Battery and the use of projective techniques in mental retardation, died shortly before the work was completed. H. C. Gunzburg, a close colleague of Earl and a noted British psychologist and journal editor, finished the book.

In his preface Earl effectively stated the purpose of his personality constructs: not to differentiate between the normal and subnormal but to differentiate and predict behavior within the subnormal group. His observational techniques are objective. However, his biases, values, anecdotes, and ways of

treating data reflect an experienced, bright, and warm clinician rather than a tough-minded field or laboratory researcher.

The rich clinical experience and the individualism in conceptualization compensates for the looseness and occasional lack of discipline in the scientific language. Few texts have used case history and anecdotes as effectively. Not merely supplementary, the case descriptions are often crucial to understanding the ideas being developed.

The array of topics range widely on the classical "central-peripheral" continuum. Mortoricity is treated both quantitatively (activity level) and qualitatively. An empathetic description of consciousness gives a vivid picture of how the retardate differentiates and selectively responds differently from the normal.

A most fascinating part of the book deals with "aspects of personality," a set of personality constructs ranging from weakness, simplicity, and viscosity to the more conventional psychopathy and psychosis. The reader is tempted to take a "nothing but" attitude and translate these into more conventional terms in learning theory, intelligence, psychopathology, and personality theory. However, the sensitive shades of meaning forbid this. A psychologist overlearned in the traditional American constructs would find a stimulating challenge to use these terms in evaluating personalities of the mentally retarded.

The insightful treatment of subnormal language indicates how language training affects the individual in matters beyond linguistic skills. The effect of such training on the individual's personality is described.

At the end of the book are chapters by Gunzburg. His systematic approach to mental testing, treatment, and training is presented. After this, Earl's Moron Battery procedures are appended.

The breadth of coverage in these three volumes present a background by which to view the value conflict among psychologists in their attitudes toward mental retardation. The field can be seen as a barren and hopeless one, professionally unstimulating, a refuge for the less-than-adequate psychologist. On

the other hand, it can be seen as an area where more social rehabilitation can be accomplished than in the mental disorders. It can be seen as an area where the intellectual demand to master the biochemical, physiological, social, and educational factors in relation to psychological ones are greater than in any other area of psychological work. From those with the latter value system comes the feeling that psychologists should be actively discouraged from entering the field of mental retardation unless they have the intelligence, training, and motivation to meet this great variety of professional demands.

## Complex Topic Complex Talk

Robert L. Sutherland, Wayne H. Holtzman, Earl A. Koile & Bert K. Smith (Eds.)

*Personality Factors on the College Campus: Review of a Symposium.* Austin, Texas: Hogg Foundation for Mental Health, 1962. Pp. xi + 242. \$1.00 (paper) \$2.25 (hardcase).

Reviewed by E. PAUL TORRANCE

All four editors of the present volume live and work in and around the University of Texas. Robert Sutherland is Professor of Sociology and Director of the Hogg Foundation for Mental Health. Wayne Holtzman is Professor of Psychology and Associate Director of the Hogg Foundation. Earl Koile is an associate professor in educational psychology who is sometimes volunteered by the Hogg Foundation for work on one or another of its assorted functions. Bert Kruger Smith directs the Foundation's Professional Information Division. The reviewer, E. Paul Torrance is Professor and Director of Educational Research at the University of Minnesota. He did his graduate work at the University of Michigan, worked for a spell as Director of the Counseling Bureau at Kansas State University and then as Director of the Survival Re-



search Field Unit of the Air Force Personnel Research and Training Center. His book *Guiding Creative Talent*, published in 1962 by Prentice-Hall, will soon come to the attention of CP's readers.

THIS book is the review of a symposium which brought together a galaxy of pioneer contributors to research on personality factors on the college campus, consisting of Nevitt Sanford, T. R. McConnell, C. Robert Pace, Ernest R. Hilgard, Theodore M. Newcomb, Dana L. Farnsworth, Fillmore H. Sanford, Robert F. Peck, and Martin Trow. Most of these contributors had had distinguished careers in psychology, sociology, education, psychiatry, or administration before their current pioneering work on the application of modern social and personality theory to the study of higher education.

Two novel features of this volume should be noted: the nature of the conference and the format of the "review of a symposium."

The conference itself is somewhat novel in that over 100 administrators and faculty members from 30 colleges were invited "to think about their own campuses as communities worthy of scientific study and their own students as personalities whose mental health is important." This process was implemented by formulating and submitting in advance of the symposium a series of searching questions, presentations by outstanding researchers in the field, and discussing the ideas and research findings presented.

The contributors dealt with such problems as: the implications of personality studies for curriculum and personnel planning, differences in student attitudes toward civil liberties, implications of differences in campus atmosphere for evaluation and planning of college programs, teaching machines and learning theory, student peer-group influence and intellectual outcomes of college experiences, and who really helps our students. There were also reports on studies underway in Texas colleges, student mental health in a college population, and student cultures and administrative action.

The format of the "review of a sym-

posium" is also somewhat novel. This review was accomplished by interspersing the text of the presentations in different kinds of type with composites of the statements made by the participants after each presentation and with reflections by Fillmore H. Sanford. This was done in an effort to recapture some of the excitement of the give-and-take of the symposium. Unfortunately, this reviewer failed to catch the excitement of this give-and-take in the way that it has been preserved, for example, in the published proceedings of the University of Utah Research Conference on the Identification of Creative Scientific Talent.

The format also confronts the reader with an extremely complex task of distinguishing between the words of the contributor, the distilled reactions of the participants, and the reflections of Fillmore Sanford. The sparkle of the reactions of the participants was apparently lost in most instances in the process of compressing them into composite statements. Thus, many of them appear in the form of rather obvious and banal observations. The reflections by Sanford, however, came through to this reviewer with much greater potency. He picked up the challenges in each presentation and made them more powerful. At times the reader has the feeling of being "wired into" his thinking processes as he listened to or read each paper.

The reader who is looking for documented research reports of the original studies of the authors will be greatly disappointed. Specific findings and descriptions of study procedures are reported only in rare instances. The research-oriented reader would do better to turn to the articles in *The American College*, edited by Nevitt Sanford (1962, CP, Nov., 1962, 7, 393-5). In fact, there is considerable overlap between these two volumes in terms of the programs of research represented. Many of the research reports on which the contributors based their papers also appeared in professional journals and in the proceedings of other conferences. The presentations are relatively non-technical and should be of interest to many workers who are concerned with higher education and youth.

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1962 423 pp. \$7.00

**VAN NOSTRAND**  
120 Alexander Street  
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# On an Old Frontier

Mary Alice White and Myron W. Harris

*The School Psychologist*. New York: Harper, 1961. Pp. x + 431. \$6.00.

Reviewed by JAMES F. MAGARY

*The authors, Mary Alice White and Myron W. Harris, are both long-time New Yorkers. Mary Alice White received her degree at Columbia University and is presently in private practice and a consultant in the schools of Pelham, New York. Myron W. Harris has a private practice as a clinical psychologist and also a faculty post at Teachers College, Columbia University. He spent seven years as a school psychologist in the Bronxville Public School System in Bronxville, New York. The reviewer, James F. Magary, is Assistant Professor of Education at the University of Southern California where he teaches a course entitled, "Problems of School Psychology." He obtained his PhD at Indiana University in 1960. He is co-editor with John R. Eichorn of The Exceptional Child (1960), and is presently editing a multiple authored text for Prentice-Hall entitled School Psychology: In Theory and Practice. He has served as a school psychologist both with the Devereux Foundation and in the Canal Zone Public Schools and is presently on the executive board of the California Association of School Psychologists and Psychometrists.*

ONE of the fastest growing specialties on the American psychological horizon is that of school psychology. In the last decade there has been a five fold increase in the number of psychologists employed by public schools. And since the school psychologist is more likely to have wider contacts with the general public than any of his brethren from other divisions, the public image "psychologist" may be heavily influenced by the perceptions of

the psychologist who serves the schools.

In the history of the profession, the volume by White and Harris is only the second or third attempt to define the role of the school psychologist; and such definition in itself is a Gargantuan task. The role of the school psychologist is usually defined by state educational statutes, implemented in a local school district by the dictates of the school administrator, and tailored to the specific situations by the inclination and training of the particular psychologist. Those who have been designated as "school psychologists" have often displayed considerable diversity in their training and in the services they perform. It is no wonder then that there is a rarity of carefully written training materials that focus on the psychologist who is actually functioning in the school setting. White and Harris have attempted to fill the need for an elementary training book and they have produced one that will be of service to all professional people, regardless of specific job titles, who work in pupil personnel services in the public schools.

The authors give a definition of school psychology, "as that branch of psychology which concerns itself with the personality of the pupil in interaction with the educational process" to distinguish it from educational psychology as the study of learning as it relates to the educative process. This dichotomy exists neither in theory nor practice.

The major weakness of this volume is that its authors never come to grips with the role of the school psychologist as an important sub-species in the expanding genre of pupil personnel specialists. Since 1896 and the days of

Witmer and Binet, one responsibility which has given the school psychologist his *raison d'être* was his ability to make skillful diagnostic studies of individual children for the purposes of placement in special classes. In fact the most frequently performed duty by the psychologist in the schools today (as indicated by the available quantitative studies) is the conducting of individual case studies to determine which children should receive special educational provisions as dictated by the specific state educational code. Thus one can view with some misgiving the fact that the present book devotes but a single chapter to the psychodiagnostic evaluation. The difficult matter of differential diagnosis and evaluation of mentally retarded children, an assignment that consumes great chunks of the school psychologist's time, is discussed in only two pages of this volume.

Few will deny that the right arm of most school psychologists is the Binet box, while the left arm is the WISC kit. The literature on these tests is exceedingly large and the school psychologist in training should be very familiar with their strengths and limitations. White and Harris cursorily dispatch these instruments with one page of comment, while they discuss personality appraisal and projective techniques at great length.

Statements such as the following deserve either more amplification or else deletion: "the Stanford Binet is generally accepted as the test of choice for the retarded, down to the mental age of two" (p. 59), "when time permits, most school psychologists favor the Rorschach test as the indispensable projective tool" (p. 228), (no research cited, in spite of the fact that most studies would be contrary to this finding) and later, "the school psychologist is in the awkward position of using instruments in which he may believe with all his heart, but to which his head cannot agree" (p. 230).

THE REVIEWER would like to commend the authors for their inclusion of classroom observation as one of the means toward more effective psychological evaluation of school children. It is important that the well-trained school



psychologist use himself as a tool without slavish recourse to his testing armamentarium in developing insights about children. The view offered by White and Harris that the "psychologist be as specific and practical as possible in his recommendations to teachers," deserves qualified acceptance.

White and Harris reflect the current ethos in the fields of clinical and school psychology in that they see the professionally trained specialist in psychology assuming the role of mental health consultant and developing the ability to help front-line persons rather than attempting to keep up with the increasing demands for direct service. In the case of the school psychologist the front-line person is the teacher and White and Harris have made many suggestions to help the tyro in this field find methods of working with teachers, administrators and students. The recognition that final responsibility for the welfare of the individual child rests with the classroom teacher is an important lesson for all ancillary educational personnel to learn.

An outstanding aspect of this text is the authors' synthesis of the relevant research findings regarding socio-economic variables and their impingement on school achievement and adjustment. The chapters, "Educational Adjustment of the Pupil Population," and "Mental Illness in Relation to the Pupil Population" are excellent. This material cannot be found elsewhere with economy of time and effort and these sections merit the book finding its place on the school psychologist's five foot shelf and for its use in all school psychologists' training programs.

The reference material at the end of every chapter in this volume is very limited; however, most important sources are included. The school psychologist will find the various sample forms included in the index a useful compilation. The book certainly does not serve as "a textbook, a handbook and a manual" as the authors' preface so grandly announces; however, it does fill a present survey text need for use in the training of our school psychologists.



## What'll You Have?

Morris E. Chafetz and Harold W. Demone, Jr. Foreword by Harry C. Solomon

*Alcoholism and Society.* New York: Oxford University Press, 1962. Pp. 319. \$6.95.

Harvey Nash

*Alcohol and Caffeine.* Springfield, Ill.: Charles C Thomas, 1962. Pp. xi + 169. \$7.25.

William C. Bier, S. J. (Ed.)

*Problems in Addiction: Alcohol and Drug Addiction.* (The Pastoral Psychology Series, Number 2) New York: Fordham University Press, 1962. Pp. vii + 247. \$5.00.

Reviewed by MAURICE KORMAN

*The first book is co-authored by a Harvard physician, Dr. Chafetz, and a Yale sociologist, Dr. Demone, both of whom have had extensive professional involvement with processes of alcohol. Harvey Nash, author of the second book, is a psychologist presently associated with the Illinois State Psychiatric Institute. William C. Bier, S. J., who edits the third book, is also a psychologist and is chairman of the department at Fordham University. The reviewer, Maurice Korman, began his life as a psychologist by absorbing all forms of corn-fed empiricism at the University of Minnesota. Since then he has engaged in some rat-running and has become a recent Diplomate in Clinical Psychology. As a member of the psychology department at the University of Texas Southwestern Medical School in Dallas he now is concerned, among other things, with research on judgmental processes and on cerebral damage. In the past he has studied alcohol abstractly in the laboratory and has worked with it concretely in the clinic. There is no report of any social or personal or introspective involvements.*

Chafetz and Demone's *Alcoholism and Society* is a valuable contribution. Although the authors' bias is clearly psychoanalytical, their oft-repeated belief in a multiple causation approach leads them to attempt a formulation of an omnibus theory of alcoholism which also pays homage to physiological, familial and cultural influences. There is undeniable comfort in a middle-of-the-road approach, yet premature attempts at synthesis may obscure the fact that there may after all be some quite distinct processes with phenotypically similar alcoholism syndromes as their end result.

This book's review of the literature on the whole is a good one. For instance, of such important documents as the World Health Organizations' 1955 symposium on "craving," enough is said to make the interested reader want to go to the original. On the other hand, the treatment of the learning theory approach to alcoholism is greatly oversimplified. The authors reason that if learning and reinforcement were the important variables, "then treatment would be simple"—which, of course, it isn't. As anyone knows who has tried to extinguish an appetitive response built on strong drive, many trials, and

AS A general, simply written introduction to the area of alcoholism,



powerful reinforcement, the alcohol "habit's" high resistance to extinction is no more than what one should expect.

The middle sections of the book, dealing with drinking in preliterate and literate societies as well as with contemporary alcoholism programs, are well-written and informative. The authors conclude their review of both older and contemporary societies by attempting to identify the drinking patterns that seem to lead away from pathological drinking: "group drinking (versus solitary drinking); drinking in a consistent and defined pattern; ritual drinking; traditional drinking at meals; introduction to alcohol at a young age within one or more of these patterns." The slow, hesitant growth of governmental alcoholism programs in the United States, which the authors review, seems to parallel the lay person's own approach-avoidance conflict about the problem of alcoholism. From Dr. Chafetz's travels behind the Iron Curtain comes an interesting account of how the Czechs, whom he feels are doing a god job, are attempting to treat alcoholics through a controlled comprehensive approach and close follow-up. The Russians, by contrast, appear to have a basically moralistic, punitive approach.

The authors present a perceptive analysis of Alcoholics Anonymous and of what they feel are the basic reasons for its success: the focus on symptom rather than underlying problems, the constructive use of compulsion, the return to a middle class way of life, the cult-like procedures that feed the members' emotional needs.

The final chapters, in which the authors use the public health framework of primary, secondary and tertiary prevention, effectively summarize the little that we know and point to the beginning of a sensible prevention program. Perhaps the major criticism that can be made of this section is that there is insufficient awareness of the basic failure of attempts to apply traditional psychotherapeutic techniques to the alcoholic; consequently, no serious attempt is made to reformulate the therapeutic problem. All in all, this is a book that will interest a wide range of read-

ers. Its virtues clearly outshine its defects.

NASH'S *Alcohol and Caffeine*, the second book of the present trio, is an account of a carefully designed experiment in which four groups of 14 college students each were given two dosages of alcohol, one dosage of caffeine, and a placebo, respectively. Following this, the Ss took some 50 "tests" from which over 100 measures were derived and analyzed. Such investigations are important if the commonly held stereotypes (e.g., alcohol is a depressant, period) are ever to give way to delineations of the more complex interactions of alcohol and other drugs with a host of other variables.

While the author is to be commended for his efforts to cut down error variance through his attention to body surface area, maintenance dosages and pre-drug performance levels, the small *N* per cell and the choice of tasks laid out for the Ss leave something to be desired.

For example, eleven sentence completion items ("When they turned him down for the job, Bill . . .") give Nash a "Constructive Reaction to Stress" score. The same items do yeoman service by allowing themselves also to be scored for "Mobilization of Aggressive Energies for Self-Forwarding Action (in Reaction to Stress)." In contrast to this quickie way of getting a fix on such important variables, think, for example, of Bitterman and Holtzman's painstaking, multi-procedural efforts expanded in an attempt to be reasonably sure that it *was* something like "anxiety" to which they were relating GSR conditioning. Cronbach's admonition to the experimentalist in his APA presidential address, to study and refine the construct validity of operational procedures before undertaking experiments seems not yet to have borne fruit. The reader will not be surprised that the penalty for such free floating constructs are negative results: the chapter on Affectivity and Response to Stress, otherwise an important issue, is concluded in nine pages. Although something of the same criticism applies also to the capacity measures, these are handled in

a more sophisticated fashion.

The results are generally unimpressive, and it would seem that the author's conclusions are more strongly phrased than the data warrant. For example, a comment such as "it is plain that *overall intellectual efficiency was enhanced by caffeine*" (author's italics) finds its principal support in the fact that *one* out of 40 independent measures and two out of seven composite measures differentiated the caffeine and placebo groups at the .05 level. The author is thus forced to rely on the fact that the sign of the difference between the placebo and caffeine groups was in the right direction in the majority of the measures—an insufficiently stringent requirement. In drawing his conclusions, the author is not unaware of this argument. He simply believes otherwise.

Despite its shortcomings, this book is worth reading if only for its carefully documented discussions of many problems in drug research and an illuminating chapter on double blind procedures. The author's discussion of the psychological mechanisms of drug action with particular reference to Barmack's hypothesis and his comments on the disinhibition hypothesis should provide the basis for future research. One would also like to see the trend of experimental studies on alcohol with normal subjects continued, especially with more attention being paid to the interactional effects provided by organismic and situational variables.

THE third book reviewed here, *Problems in Addition: Alcoholism and Narcotics*, reports the proceedings of the Institute of Pastoral Psychology held at Fordham University in 1959. It includes 22 papers by a variety of professional and lay persons, and it is, as such compendiums often are, rather uneven in quality. It contains some interesting tidbits, such as Judge Murtagh's discussions of the controversy between the Los Angeles and New York police departments (should drunks be left to lie in the gutter?), or Father Ford's reference to theological controversies on the consumption of alcohol (you can drink *ad hilaritatem*—but only if you translate this as cheerfulness, not hilar-



ity). Perhaps the most interesting parts of this book are Father Ford's rather sensible discussions of pastoral treatment, especially in his perceptive distinctions between the alcoholic's neurotic and real guilt, and his pointed suggestions for the pastoral approach to the alcoholic. This is a book which should be of value to the audience for whom it was written; psychologists may find it less engrossing.

## One Boy's Pain

Beulah Parker. Foreword by Theodore Lidg

*My Language is Me: Psychotherapy with a Disturbed Adolescent.* New York: Basic Books, 1962. Pp. v + 397. \$8.50.

Reviewed by MARY ENGEL.

The author, Beulah Parker, is a psychiatrist and is a clinical instructor in the Department of Psychiatry at the University of California Medical School. One of her leading interests has been that of adapting psychoanalytic principles to the training of non-psychiatric professional personnel. The reviewer, Mary Engel, received her PhD from George Peabody College in 1956 and spent two years at the Menninger Foundation as a post-doctoral fellow. She then went to Michael Reese Hospital where she was supervisor of USPHS post-doctoral training of clinical psychologists. Presently she is Assistant Professor in the Graduate School of Education, Harvard University, and Clinical Associate at the Judge Baker Guidance Center, Boston. Her interests lie in the area of diagnostic problems in dealing with emotionally disturbed children, especially at the adolescent level.

DAVID was an adolescent who went to see Dr. Parker once a week for six and a half years. The initial diagnosis was ominous. He was in a border-

line psychotic state, was given to delinquency and had been treated without success by a previous therapist. At sixteen, he was withdrawn, cautious, yet motivated for treatment.

For a year and a half, communication between Dr. Parker and her patient took place on a metaphoric level.

David: "I am thinking about phonograph records and wire recorders. It would be nice if the grooves could be arranged so that one could play words in many different combinations but always make sense . . . One often has to play music one does not want to hear in order to get to the parts that one does want to hear."

"Therapist: "Yes. Also, at times, a recording isn't clear, and a place may have to be played over and over in order to get it."

Intensely ambivalent over being understood, David emerged very slowly from his initial borderline psychotic state. He gradually learned to trust his therapist. After one hundred hours, he was more willing to have his "code language" translated:

David: "I'm still mad at my 'Family living' teacher for making me take the course . . ."

Therapist: "I think the teacher is a stand-in for me. You are still mad at me for telling you you should remain in therapy right now . . . you need to see yourself as forced by me, . . . we agreed mutually that you were not ready for the Army."

David: "I think I feel guilty for not wanting to go in."

After five years of treatment, David was able to communicate directly about his most basic conflict: fear of, yet wish for heterosexual closeness.

David: "I'm wondering why I can't fall in love." . . . "She is a perfect girl for me, but I can't seem to fall in love with her."

Therapist: "Perhaps you have been too much hurt in the past. 'A burnt child fears the fire.'"

David: "But I don't want to be afraid of fire! I need the warmth! . . ."

*My Language is Me* is based on quasi-verbatim exchanges between David and his therapist. Some of over two hundred sessions are fully presented, others are collapsed into a few paragraphs. Oc-

asionally, the author interrupts her account to discuss concepts of analytic theory, as these relate to the treatment process. Her theoretical commentaries, while sufficient to orient the non-clinical reader, are uneven in their quality. The discussion of transference is scant and the role assigned to countertransference is unclear. Dr. Parker's statement that she tried to "control" countertransference ignores recent developments. Generally, the less than conscious reactions of the therapist are seen as a necessary part of good treatment and diagnosis.

Also, contrary to recent trend is the author's minimizing of delinquent acting out. Differentiated discussion of the communication value of antisocial actions of the patient is absent, although he frequently communicates through overt behavior arising out of his painful conflicts. There is no reference to the work of Ekstein and his associates concerning borderline adolescents, (1, 2).

The chief value of *My Language is Me* is that it tells us of the *quality* of the treatment of borderline adolescents through the words of patient and therapist. Those unfamiliar with this clinical area will find a splendid introduction, others will delight in the skill and sensitivity of the therapist. This book is about two people and how they learned to understand each other, over a long period of time, and how this understanding resulted in a profound change in David.

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Won't this whole instinct matter bear revision?  
 Won't almost any theory bear revision?  
 To err is human; not to, animal.  
 Or so we pay the compliment to instinct,  
 Only too liberal of our compliment  
 That really takes away instead of gives.

- ROBERT FROST





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## Precision, Elision, and Revision

Joseph Berger, Bernard P. Cohen, J. Laurie Snell and Morris Zelditch, Jr.  
*Types of Formalization: In Small-Group Research.* Boston: Houghton  
Mifflin, 1962. Pp. v. + 159. \$4.50.

Reviewed by A. JOHN ARROWOOD

Of the four authors, Berger, Cohen and Zelditch are all Associate Professors in the Department of Sociology at Stanford University, having reached there by individual vocational routes after having, one and all, received degrees in sociology from the Department of Social Relations at Harvard (1958, 1957, and 1955, respectively). The other author, J. Laurie Snell is Professor of Mathematics and Vice Chairman of the Department of Mathematics and Astronomy at Dartmouth College. He received his PhD at the University of Illinois and taught at Princeton and Illinois before Dartmouth. The four authors got together in 1959 under the auspices of the Social Science Research Council to review the use of mathematical models in small group research. The present volume is the result. The reviewer, A. John Arrowood, is Assistant Professor of Psychology at the University of Toronto, going there fresh from graduate work at the University of Minnesota (a 1961 PhD) with Harold Kelley, Stanley Schachter and Ben Willerman in the Laboratory for Research in Social Relations. He acknowledges his academic history to be, for obvious reasons, brief, and reports his enthusiasm, perhaps because of inexperience, to be high. At the moment he is thinking and doing research in the sometime related areas of decision processes, cooperation, competition and cognitive dissonance.

havior is increasingly frequent is evidenced by the appearance in recent years of models of individual and group learning, of rational choice behavior, of small group interaction, of voting behavior, of communication, conformity, and occupational mobility. That it is, however, a complex activity is suggested by the apparent dissimilarity of the behaviors and behavioral products which frequently are labeled model building and models respectively.

The processes within, and the products of, small groups represent an area of research and theory which has attracted considerable model building attention. The relative recency, popularity, and heterogeneity of the modeling of social-psychological phenomena have given rise to questions concerning the nature and utility of this approach. It is to such questions that *Types of Formalization* is directed.

The authors—three Harvard-trained, Stanford-based sociologists (Berger, Cohen, and Zelditch) and one Dartmouth mathematician (Snell)—met in 1959 under the auspices of the Committee on Mathematics of the Social Science Research Council to review, and to consider ways and means to further, the application of mathematics to small group research. Although the authors have some commitment to this approach (having, themselves, constructed models applicable to small group research), they have produced a book which, as Sanford Dornbusch points out in the brief but insightful foreword, "is intended to reach beyond the model-building clique to curious colleagues and the frankly hostile." Berger, Cohen, Snell, and Zelditch have

THE CONSTRUCTION of formal models is an increasingly frequent, albeit complex, phenomenon in the behavior of behavioral scientists. That this be-



not made the mistake of writing exclusively for Zelditch, Snell, Cohen, and Berger. Nor have they limited themselves to addressing solely the small group social psychologist—of any of the several sociological or psychological persuasions. Rather, or in addition, the goals of the book are three. The authors hope to stimulate (1) an appreciation of the range of applicability of formalization to problem areas in the behavioral sciences; (2) a clearer recognition of the relation of model building to research; and (3) a clarification of the criteria appropriate to the evaluation of behavioral science models.

To this end, the authors have attempted to classify existing models according to the primary goal of their formalization. And, by formalization, they refer to “the general process of making explicit the logical structure of a set of assertions”—including translating a set of statements about empirical phenomena into a particular formal language, either quantitative (e.g., the calculus) or non-quantitative (e.g., symbolic logic).

THE AUTHORS distinguish between three kinds of primary goals—i.e., three types of formalization—and provide a concise discussion of one example of each. The first of these types they call the *explicational* model, which aims at the explication (rendering of a precise meaning) of one or more basic concepts. Although these basic terms need not occur within the context of a theory, it is a matter of some mystery as to what they are basic to if they do not. The example analyzed is Cartwright and Harary’s graph theoretic (re)formulation of Heider’s treatment of the balance concept. The authors note that Cartwright and Harary do not attempt to restate all of Heider’s theory in graph theory terms. The terms “entity,” “relation,” and “system” in the original are co-ordinated to the terms “point,” “line,” and “graph” respectively in the model; and “balance” is defined in the model in a manner consistent with Heider’s use of the term in the original. Thus, it is the concept of balance itself, rather than the entire theory, that is explicated.

Next the authors deal with the *representational* model, the aim of which is to represent a specific type of well-documented social phenomenon in as precise and as formally simple a fashion as possible. Representative of the representative approach is Cohen’s four-state Markov model of the Asch conformity experiments.

A third type of formalization considered has as its primary goal neither the explication of a basic concept nor solely the representation of a specific recurrent phenomenon, but rather the development of a “general explanatory theory which formally accounts for a variety of observed processes.” Both this third type of model and the representational model employ a relatively small number of underived quantities to describe an observed process. Unlike the representational case, however, the underived quantities here are the theoretical constructs of the general theory being formalized. Hence, this type of formalization is termed a *theoretical-construct* model. Illustrative of it is the Estes-Burke learning model applied to the group situation by Suppes and Atkinson.

THE AUTHORS conclude that the specific characteristics and functions of a model should be judged in terms of the goal of the formalization, that this goal may differ at different stages of the research process, and that formalization can be fruitful at more than one stage of that process.

In each case of formalization considered, the purpose was to produce a precise statement of something—either of the definition of a concept or of the description or proposed explanation of a phenomenon. Moreover, in each case considered, the formalizer has made certain simplifying assumptions or elisions—either of procedure (I shall not talk about this part of the original conceptualization or this aspect of the phenomenon), or of substance (this is what I shall say about those things that I am going to talk about). Furthermore, in each case, certain revisions have been suggested as a result of (or, at least, in the course of) formalization—either in the original conceptualization, or in the procedures or foci

of study with regard to the behavior of interest.

Is there a research scientist alive who does not attempt, at least, to do all of these? Is *Types of Formalization*, then, an explicational approach to the research concept, a representational approach to research as a recurrent social-psychological phenomenon, or a theoretical construct approach to a normative theory of the research process?

The answer is that it is all three. The authors attempt (informally) to explicate what is meant by model building; they analyze models representative of this approach; and they point out the advantages accruing to the enterprise. One might have wished for some more extended discussion of the precise relationship between formalization and model building (are they identical?) and between these two (or one) and theory construction. But perhaps the explication of these relationships can be approached currently only via representation. I have the feeling, however, based on admittedly casual empiricism, that many social psychologists who regard model building as an esoteric and auto-erotic exercise engaged in by colleagues (or interlopers) essentially uninterested in doing research of their own might not react so harshly to formalization, were they to be exposed to this treatment of it and of its connection, present and possible, to both theory and research.

In a volume having, in part, a classificatory aim, it is surprising not to find an index. The absence of an index, however, is not at all disturbing in view of the authors’ tightness of organization and their inclusion of a 48-page annotated bibliography of 42 items ranging from A (belson and Rosenberg) to Z (ajonc and Smoke)—each entry describing problem, method, and results. It is difficult, after reading this book and glancing through the bibliography, to continue to believe that a model in the behavioral sciences is the same thing as one description of a model husband—a poor imitation of the real thing.

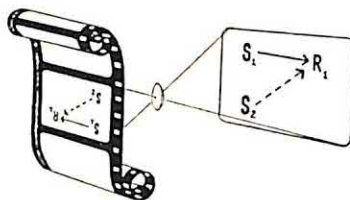


There is no more serious thing than liberty.  
—ERNEST HEMINGWAY



# INSTRUCTIONAL MEDIA

Edited by A. A. Lumsdaine



## Programing Portrayed in Breadth — not Depth —

John L. Hughes

*Programed Instruction in Schools and Industry.* Chicago: Science Research Associates, 1962. Pp. 299, \$6.00.

Reviewed by MERLIN C. WITTRICK

The author, an industrial psychologist who got his PhD from Columbia in 1953, has been engaged for most of the time since then in personnel and training research for the International Business Machines Corporation. Between times he lectures at C.C.N.Y. Several years ago (with an initial assist from David J. Klaus), he and Walter McNamara initiated the development of auto-instructional programs for several of the training courses at IBM. The results of these programs show dramatic gains in training efficiency and have been widely cited. Wittrock, the reviewer, is Assistant Professor of Educational Psychology in the School of Education at the Los Angeles campus of the University of California (see review). He has his BS and MEd from Missouri and took his PhD from Illinois in 1960. He has written and experimented with an instructional program which successfully taught kinetic molecular theory concepts to first-grade children. Avocationally an excellent baritone as well as clarinet player, he is most interested intellectually in the application of mediation theory both to classroom teaching and programed instruction (which he sees as the ideal milieu for investigating mediational processes). Currently Wittrock is directing the development of programed learning sequences aimed at teaching

kindergarteners to achieve transfer through "discovery learning."

FROM its phenomenal growth in the past few years many psychologists and non-psychologists alike seem certain that the infant, programed instruction, is destined for a long and productive maturity. With such enthusiasm, it is inevitable that many people in schools and industry seek elementary introductions to programed instruction. It is equally as inevitable that several of these productions appear on the book market. One of the best of these introductions is *Programed Instruction in Schools and Industry*. Hughes writes the book for educators, training specialists, and executives in industry who may want to write or use programs, and for other interested people who are unfamiliar with programed instruction. As the author of an introductory book, Hughes had to decide what a beginner must learn to obtain a basic familiarity with programing. He seems to have decided upon a little bit of almost everything.

The first four chapters present some basic ideas about programing. After a brief introduction into the stimulus-response-reinforcement cycle, the concept of programing is related to traditional teaching procedures and to teaching machines. The constructed response, linear program, as developed by

B. F. Skinner and his followers is compared and contrasted with the multiple-choice, branching program initiated by N. A. Crowder.

Some of the better-known teaching machines are pictured and discussed. The use of machines, programed texts, and scrambled books to present programs to students is objectively presented with the conclusion that although the machines offer control over a student's behavior that the texts and books do not, it is the quality of the programed materials rather than the way they are presented which is more important for determining learning outcomes. The objective discussion of such controversial issues as Skinner vs. Crowder programs and machines vs. texts, is refreshing. Unfortunately, there is not a detailed discussion of any of these topics. The fundamental issues from learning theory which underlie the use of constructed responses, immediate reinforcement, length of frames, etc., are purposefully minimized. Neither is there much of a summary or discussion of relevant research literature on programed instruction. In the preface to the book, Hughes mentions his reluctance to consider theory because the non-psychologist may incorrectly assume that programed instruction can have little practical value until theoretical controversies are resolved. He presents an analogy between theoretical principles in programed instruction and motors in automotive design. That is, some makers place the motor in the front of the automobile, while others prefer to place it in the rear, yet both types of automobiles are functional. As a result, the first part of the book becomes a fine description of programed instruction but with aphorisms replacing theory and explanation.

A brief review of several studies is presented in Chapter 5 to show how programed instruction can be applied to education, industry, military service, and at home. Most of the remainder of the text (Chapters 6-8, and 10) is devoted to the writing of a program, including the plan of the program, the design and construction of frames, the cost and the mechanics of preparing programs, and the executive's problems of organization of a local staff to write



programs. This "how-to-do-it" section of the book stresses the preparation of a constructed response-linear program.

In the fall semester of 1962, the reviewer taught a class of 175 undergraduate, educational psychology students who were preparing to be secondary school teachers. Each student was required to buy a copy of *Programed Instruction in Schools and Industry* and to prepare, with the help of this book, a program on a subject suitable to be taught in the secondary school classroom. The program was to be at least 50 frames long and was to be supplemented with a pretest and a posttest. The program and tests were administered by the educational psychology students to at least two secondary school students. From the many hours spent by the reviewer with these educational psychology students and with the Hughes book, it seems that the section on writing programs provides invaluable assistance to beginning students.

However, when put to the specific use mentioned above the book had deficiencies. There was insufficient attention given to preparing explicit, behavioral objectives. This problem of specifying criteria was compounded with an inadequate discussion of the preparation of pretests and posttests to use with the programs. Also, hardly fitting for a beginning programmer were the rather extensive discussions of cost and time estimates, secretarial problems, page layouts, suggested title pages, and attractiveness of covers. When the student began to delve into why some frames would communicate knowledge while others would not, there was little fundamental information he could gain by referring to the textbook. In short, the book became weak at the crucial points. Neither did it carry one example through the entire process of writing a program. That is, examples were presented in the context of particular points, such as fading or vanishing cues, and there was not sufficient interrelationship between examples. Quite often the students were interested in a detailed classification of different types of prompts, such as echoic, textual, formal, and thematic, but could find very little on these particular subjects.

Those students who preferred to write a branching program, rather than a linear program, were given very little help by the book. It seemed that by minimizing in the earlier part of the book the theoretical issues it was more difficult, not less difficult, to write a program, even a constructed response-linear program. The biggest problems of programing which the students faced appeared to be those of stating objectives, organizing the frames, and deciding upon the types of criteria to use to evaluate teaching, not upon the mechanics of programing. The Hughes book has no examples of tests or of criteria. Neither does it delve into the important question of how criteria, i.e., recall, recognition, transfer, and savings, are related to the types of programs which can be written. One student commented, "Maybe programing is effective because it makes the learning and testing situations very much alike—little transfer is needed. The student gets reinforced for behavior which is similar to behavior on the posttest." Although Hughes can scarcely be held accountable for not providing answers to such questions, it seems that when the book is put to a specific use the fundamental issues will arise, and will go unanswered.

After comments on published programs, their evaluation, and on the use of programs in education and industry, the text of the book ends. On page 113, the appendices begin and occupy well over half the total pages of the book. The appendices include a list of commercially available programs and samples of academic and industrial programs. There are also lists of available teaching machines, publishers of programs, and manufacturers of teaching machines. The 27 samples of academic programs include topics from *Areas of Rectangles* to *Your Car and Safe Driving*. The 22 samples of industrial programs include topics from *Balancing the Tellers Cage* to *Detecting Counterfeit Money*. Usually only a page or two are devoted to sampling any one program. The samples of programs are not related to the text. (Chapters 1-11). However, the extensive appendices perform a worthwhile function of placing together a variety of very useful infor-

mation. The major weakness of the appendices appears to be closely related to a weakness of the earlier sections. A wide variety of programs is provided but almost all of them are only briefly sampled without comment. The book is comprehensive but it does not present much in depth. Perhaps an introductory book can be expected to do no more. In any event, it is one of the few books where one can find some practical and constructive help on writing a program and on introducing and using programs in schools and in industry. It is well written and illustrated but also relatively expensive for the amount of information which it contains. Hughes more than adequately accomplishes his intended purpose of presenting in non-technical language a comprehensive introduction to programed instruction. It deserves to receive wide circulation among the audience (mentioned above) for whom it was written.

Although the book will not provide new information to those who are sophisticated in programing, it will give sophisticated programmers cause to ponder. Because Hughes is objective and comprehensive in his approach toward sampling the field of programed instruction, some of the strengths and weaknesses of the book reflect strengths and weaknesses of the current state of knowledge about programing. It is not entirely his fault that more can be said about cost, time estimates, page layouts, and the mechanics of programing than can be said about how programs teach or how different criteria of learning relate to the types of programs which can be developed.

The major problem of the programmer is not mechanics of program writing but is that of sequencing stimuli to attain explicit and measurable objectives. Programed instruction offers a careful, empirical analysis of the effects upon learners of sequential segments of connected discourse material. It would be easier than it is at present to write an introductory text on programed instruction if empirical research could develop a sophisticated theory which would relate the parameters of stimuli to the mediating behavior which they probably produce in the learners, i.e., a theory of how programs teach.



# ON THE OTHER HAND



## WHAT SZASZ SAYS

George Kelly's review of Szasz's book *The Myth of Mental Illness* (C.P., Oct. 1962, 7, 363) stated that Szasz failed to question the concept of "illness" and, furthermore, that he had not abandoned nineteenth century dualism. This reader came away with a different impression from the book. Nowhere in the book is any sort of dualism implied. The entire book is aimed at showing the shortcomings of the concept "mental illness" and of trying to replace it by a theory of personal conduct in terms of the theories of communication, games, interpersonal relationships, and learning theory, all of which are devoid of biological reductionism.

Kelly's review failed to do justice to Szasz's efforts to show the importance of a commitment to certain theories in both psychodiagnostics and psychotherapy. Kelly only alluded to two other concepts Szasz discussed; "privacy" and "labeling." Szasz believes certain psychiatric labels may in themselves alter a patient's entire life e.g., possibilities for a job and acceptance by society. He does not believe psychiatric categories are without value, but rather that these labels may often carry with them ill effects. He further alludes to the possibility of new diagnostic categories based on the adequacy of inter-personal relationships.

Szasz's discussions of privacy in psychotherapy and the abuses of psychiatry are neglected topics warranting the attention of all those engaged in psychotherapy and psychiatry.

GARY GOLDENBERG  
*Letterman General Hospital  
San Francisco, California*

## WHO'S CONFUSED?

I wish to make two comments concerning the review of my last book (CP Dec., 1962, 7, 445-446).

The first is a correction of fact. CP is behind the times in crediting me with six children—it has been seven for more

than two years. I make this correction in spite of the fact that it may—and has—invited some not altogether complimentary comparisons between the number of my children and the number of my books.

The second comment concerns the review itself. It may appear to be looking a gift horse in the mouth to be critical of a favorable review. But Arbuckle does something which is a common practice among reviewers, and thus offers an opportunity to bring it to the attention of future reviewers.

In three separate places Arbuckle claims that the book, or the author, is confused. In one he states that "the organization of his material appears to be somewhat confused." What he seems to mean is that he disagrees with my organization. Again, he states that "Chapter 12, 'Personal Counseling,' is another illustration of semantic confusion," when he seems to mean that he disagrees with my terminology. Arbuckle also states that the book is weakened "because of lack of attention to the theoretical and philosophical concepts underlying counseling and personnel services." Either he did not read the first two chapters, or this is again an illustration of a difference in use of the terms theory and philosophy. Super, in his review (*J. counsel. Psychol.*, 1962, 9, 397-398) notes that the strength or uniqueness of the book is its "cohesive or integrating theoretical position."

Perhaps it might be more accurate, in some instances at least, if the reviewer admitted that he was confused and suggested that the reader determine if the cause of the confusion lies in the book and its author. I do not accuse Arbuckle of confusion, however. He is in fact less guilty of the semantic confusion to which I object than many reviewers. What I suggest is that if a reviewer disagrees with an author or has a difference of opinion, he label it as such, and not say or imply that the author is confused or in error. There are differences in such

things as terminology and organization, and the opinions or preferences of the reviewer are not necessarily better or more correct than those of an author, although reviewers tend to assume or imply this.

C. H. PATTERSON  
*University of Illinois*

## KEEPING BOOK ON BOOKS

All over the country, in fact all over the world there must be committees to advise on the purchasing of books "in psychology" for college, university and public libraries. Many, many hours are spent on this task, which may be rather interesting when done for the first time, but which becomes less interesting as time goes on.

Is there any way in which this rather tedious job can be lightened? Book reviews are a help, of course, and most committees are likely to rely a good deal on Contemporary Psychology. The Mental Health Book Review Index can also be useful by providing a means of locating reviews in other journals.

What is lacking is a classified summary from time to time in which books are listed by areas or fields with a ranking or grading, such as "essential," "good secondary source," "routine textbook," "peripheral reading only," "appeal limited to specialists," etc. Somewhat as if the Mental Health Book Review Index had been cross bred with Menninger's 1950 Guide to psychiatric books!

Whether a certain amount of centralization of the library selection process is desirable or not is debatable. We certainly do not want a psychology book of the month (or week) plan, but some guidance for libraries beyond what is now available might be good.

STEVEN S. VANDENBERY  
*University of Louisville*

## ADDENDUM

Dr. Joyce M. Hoffman's list of approximately 330 paperbacks which are of use in psychology omitted three New World Books published by International Publishers, 381 Park Ave. S., New York 16, N.Y. Each of the books was written by Harry K. Wells. They are:

1. Ivan P. Pavlov. \$1.45. NW-12.
2. Sigmund Freud: A Pavlovian Critique. \$1.65. NW-13.
3. Failure of Psychoanalysis. \$1.65. (Just published.)

S. D. KAPLAN  
*Lincoln State Hospital  
Lincoln, Nebraska*



## **Contributions to Modern Psychology**

*Selected Readings in General Psychology, Second Edition*

*Edited by* DON E. DULANY, JR., University of Illinois; RUSSELL L. DEVALOIS, Indiana University; DAVID C. BEARDSLEE, Michigan State University, Oakland; MARIAN R. WINTERBOTTOM, Judge Baker Guidance Center, Boston, Mass. Enlarged by some 25 percent, the second edition of this text contains a new chapter on social psychology, and a total of 16 new articles. The set of basic experimental reports and case studies presented in this collection provide a more detailed view of research in psychology than can be gained from the general discussions in textbooks and lectures. May 1963 504 pp. paper. \$3.50

## **Current Perspectives in Social Psychology**

*Readings with Commentary*

*Edited by* E. P. HOLLANDER, and RAYMOND G. HUNT, State University of New York at Buffalo. The book provides a valuable collection of some 50 significant papers contributing to theory in social psychology. The selections, almost all of them published in the past decade, reflect the entire range of productive thinking in the field and form a sound basis for understanding the nature of the work being pursued today in social psychology. Substantial commentaries by the editors preface each of the eight sections. August 1963 608 pp. paper prob. \$1.50

## **The Experience of Anxiety A Casebook**

*By* MICHAEL J. GOLDSTEIN and JAMES O. PALMER, University of California, Los Angeles. This collection of 24 unanalyzed case materials is designed to give the student direct experience in interpreting raw clinical data. The cases have been carefully selected to represent a wide variety of syndromes and dynamic personality patterns and to include important problems in diagnosis. Questions accompanying each case help guide the student. April 1963 320 pp. paper. \$2.25

## **Selected Readings on the Learning Process**

*Edited by* THEODORE L. HARRIS, University of Wisconsin, and WILSON E. SCHWAHN. 1961 437 pp. illustrated, paper. \$3.50

## **Foundations of Psychopathology**

*By* JOHN C. NEMIAH, Harvard Medical School and Massachusetts General Hospital. 1961 352 pp. \$6.50

## **General Experimental Psychology**

*An Introduction to Principles and Techniques*

*By* LAWRENCE M. BAKER, Purdue University. 1960 448 pp. illustrated \$6.00

## **Method and Theory in Experimental Psychology**

*By* CHARLES E. OSGOOD, University of Illinois. 1953 808 pp. college edition. \$11.00

***Oxford University Press / New York 16, N. Y.***



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Contemporary Psychology: A Journal of Reviews is published monthly. The subscription per year is \$10.00, foreign \$10.50, single number \$1.00.

Published by the American Psychological Association at 2210 W. Dallas, Houston, Texas and 1333 Sixteenth St., N.W., Washington 6, D. C.

Second-class postage paid at Houston, Texas and at additional mailing offices. Accepted for mailing at special postage rate stated in section 132.122, Postal Manual.

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# Contemporary Psychology

A JOURNAL OF REVIEWS

VOLUME VIII

May, 1963

NUMBER 5

## Sweet Lemons

Douglas H. Lawrence and Leon Festinger

*Deterrents and Reinforcement: The Psychology of Insufficient Reward.*  
Stanford, Calif.: Stanford University Press, 1962. Pp. 180. \$4.75.

Reviewed by RUSSELL M. CHURCH

Both authors, Douglas H. Lawrence and Leon Festinger, are Professors of Psychology at Stanford. Lawrence, a Yale PhD, has worked primarily in the field of learning while Festinger, a product of the University of Iowa, has heretofore borne the label and function of an experimentally inclined social psychologist. Festinger has authored before. Lawrence hasn't. Outside of the present volume, their principal joint focus of attention has been the GO board. The reviewer, Russell M. Church, is Associate Professor of Psychology at Brown University. He took his degree from the Department of Social Relations at Harvard and his research there and since has been concerned with the application of learning theory to social behavior and has held a particular focus on problems of negative reinforcement.

LAWRENCE and Festinger present a bold thesis—that the theory of cognitive dissonance, originally developed to explain some behavior of human beings in social situations, may also apply to some problems of animal behavior. *Deterrents and Reinforcement* represents an important amplification of the address Festinger delivered to

the American Psychological Association as a recipient of the 1959 APA Distinguished Scientific Contribution Award.

Traditionally, reward has been considered a factor that increases response strength while resistance to extinction has been used as a measure of response strength. Nonetheless, there is evidence that partial reward results in greater resistance to extinction than does 100 per cent reward, that delayed reward results in greater resistance to extinction than does immediate reward, and that a more effortful response results in greater resistance to extinction than a less effortful response. The authors believe that these observations can be incorporated into the main body of learning theory only with great difficulty, i.e., that learning theory is at an impasse. They suggest that this is the time for a major revision, and they present the outline for this revision in terms of cognitive dissonance theory.

There is a variation of one of Aesop's Fables involving a fox that ate the sour grapes, but called them sweet. According to cognitive dissonance theory, if the immediate attraction (sour grapes) does not justify the action (of eating) an animal may find extra attractions for its action. Some-



LEON FESTINGER & DOUGLAS LAWRENCE  
Over the GO Board

what more formally, the situations considered in this book involve a dissonant relationship between the facts that (a) the response has been made, but (b) the reward was insufficient. This produces dissonance motivation that the animal will seek to reduce by finding added attractions in the situation. These added attractions may be measured either in a preference test or in a test of resistance to extinction.

IN THEIR development of the theory Lawrence and Festinger have seriously considered the relevant experiments of others, although they did not use evidence from free-responding situations. Most of the 60 references are recent—two-thirds of them since 1956. Most importantly, they have used the theory of cognitive dissonance as a framework in which to present 16 experiments of their own. All of the experiments are relevant to the theory, and most of them are consistent with it. The subjects in these experiments were rats, and the apparatus typically em-



ployed was a runway, often with turns and obstacles.

In the most extensive experiment, involving 146 rats, Lawrence and Festinger attempt to demonstrate that resistance to extinction under conditions of partial reinforcement is a function of the number of unreinforced trials, not a function of the percentage of reinforced trials. Groups of rats were trained in an S-shaped runway with varying percentage of reward (33%, 50%, and 67%) and with varying number of unrewarded trials (16, 27, and 72). The results confirmed the prediction that resistance to extinction was a function of the number of unreinforced trials, not a function of the percentage of reinforced trials. Unfortunately, these data are almost equally supportive of an alternative hypothesis that resistance to extinction, under conditions of partial reinforcement, is a function of the number of trials of training. A subsequent experiment under conditions of low effort (the hurdles of the runway were removed and the doors were counterweighted) found resistance to extinction to be independent of the number of unrewarded trials. This creates some difficulty for dissonance theory since the theory should account for the high resistance to extinction typically observed after partial reinforcement of a response involving a low degree of effort.

Presumably, it is of some consequence whether 3 nonreinforced trials occur in the context of 10 or 10,000 reinforced trials; whether they occur in the middle or the end of a series of trials; whether they are randomly distributed or bunched in the 10 trials, etc. Therefore, Lawrence and Festinger emphasize the unrewarded trial "only when this experience is related to expectation of reward." At the present time the theory is not sufficiently explicit to present a measure of whether or not the rat expected the reward on a given trial. When such a specification is made, it may be that the theory of cognitive dissonance has reintroduced familiar concepts, although it uses new words.

Several experiments demonstrate that resistance to extinction is increased by increasing effort during acquisition,

and that resistance to extinction after partial reinforcement is a function of the treatment in the goal box, not the treatment in the alley. Other experiments show that an animal prefers an area in which it has been delayed over one in which it has not. Cognitive dissonance theory seems to account for the set of facts reported in this book better than any single alternative hypothesis, but no single explanation may be adequate.

*Deterrents and Reinforcement* is well-organized, concise, and eminently readable. The use of the book form, however, precluded the detailed description of apparatus and statistical techniques employed which, in some cases, would have been valuable. On the other hand the presentation of this material under a single cover facilitates the assessment of the application of cognitive dissonance theory to problems

of animal learning.

Although it is unlikely that cognitive dissonance theory will have the impact on animal learning that it has had on social psychology, there should be a number of studies testing further implications of the theory. The authors note that there is currently lacking any substantial support for the theory from studies of punishment, yet these situations in which an animal continues to respond, although punished, are definitely within the domain of the theory. Whether or not the theory of cognitive dissonance has any lasting influence on the interpretation of animal learning, the book will have served an important purpose in presenting a set of new data on a series of related problems. Any theory of partial reinforcement, delay of reward, and effort, will have to attempt to incorporate these new facts.

## A Look into the Dark

Joost A. M. Meerloo

*Suicide and Mass Suicide.* New York: Grune & Stratton, 1962. Pp. iii + 153. \$3.75.

Reviewed by EDWIN S. SHNEIDMAN

*The author, Joost A. M. Meerloo, was born in 1903 at The Hague, earned his MD in 1927 at Leyden University and his PhD in 1932 at the University of Utrecht. Following the Nazi occupation he escaped to England in 1942, where he served as Chief of Psychological Warfare for the Netherlands Government and advisor to SHAEF and UNRRA. Dr. Meerloo arrived in the United States in 1946, becoming a diplomate of the American Board of Psychiatry and Neurology, Associate in Psychiatry at Columbia University, a Fellow of the Academy of Psychoanalysis, and a lecturer as visiting professor at several universities. The reviewer, Edwin S. Shneidman, is Co-Director of the Suicide Prevention Center, Co-Director of the VA Central*

*Unit for the Study of Unpredicted Deaths and Associate Clinical Professor of Psychiatry (in Psychology) at the University of Southern Californiana School of Medicine, all in Los Angeles. He edited Thematic Test Analysis (1951) and co-edited Clues to Suicide (1957) and The Cry for Help (1961). He is recent Past-President of the Society for Projective Techniques and is currently serving on the American Board of Examiners in Professional Psychology. In 1961-62 he was a Research Fellow in Harvard's Department of Social Relations.*

DEAR CP Reader:

I am sitting here, pen in hand, not unaware that I have a pistol in my



desk drawer, a bottle of Black Leaf-40 on the back porch shelf, at least 30 Doriden capsules in the medicine cabinet—my mind filled with suicidal thoughts. What occupies my mind are not, of course, thoughts of committing suicide, but rather reflections *about* suicide. There are so many unanswered key questions: What *are* suicidal phenomena? Why *do* people kill themselves? How *can* suicides be prevented? And so on. What has stimulated all this rumination is my having just finished reading Meerloo's *Suicide and Mass Suicide*. It didn't cheer me up. I can tell you. It's an interesting book though. The author, too, is apparently a very interesting person. In his book he tells about his surviving the concentration camps in spite of the Germans' attempts to kill him. He is very much alive today and very much lives in this book. This is a readable book filled with some gripping personal accounts, many psychoanalytically oriented assertions, and several thought-provoking reflections about one of the most—perhaps *the* most—frightening problems of our time. It is a book that each of us, were he to write it, would write in his own way.

"I like the essay form," says Meerloo in the very first sentence of his Foreword, "It liberates me from the limitations of scientific writings, I don't have to succumb to the scholar's compulsion to quote as many fellow professionals as possible . . . I can contradict myself and I can use emotional illogic just as frequently as scientific exactitude." He says it much more graciously and openly than most reviewers might dare.

Meerloo's main thesis in this book is "that there exists a very close relationship between personal suicidal feelings and the mass emotions in the world. Each individual, in the personal self-destructive feelings and habits of which he is usually unaware, contributes a grain of suicidal tendency to the collectivity. Moreover, because one man's feelings of despair and final renunciation of life is often rooted in some infantile experience shared by all men, there can be a tremendous contagion between individual feelings and collective emotions."

Within the book there are various kinds of statements and beliefs. I often puzzled as to where he had obtained some of his facts and statistics—at variance with some of our information—but inasmuch as this was not the main burden of the book, I do not feel it important to stick on these points. The range of the nineteen brief chapters contains an intriguing array of exciting topics—"Man's Raving Frenzy as a Disguised Form of Suicide," "The Suicidal Population Explosion," and "Suicide, Menticide, and Psychic Homicide,"—as the titles to chapters 12, 13, and 14, respectively, demonstrate.

Of particular interest to me was Meerloo's classifications of the various motivations for suicide. He indicates that he is "pleading here for an *infinite number* of motivations," but he actually presents two schemes. In relation to the first, he states that this scheme is based on a clinical analysis of more than 1,000 cases, and although this statement has a footnote subscript, I could find no further reference to these studies or these data. There are four categories in the first scheme: (a) Suicides that "have no conscious motivation. These are usually impulsive suicides in mentally deranged and alcoholic individuals;" (b) suicides "committed by mentally unstable people who have a great variety of motivations, justifications and rationalizations of their act;" (c) individuals who "commit suicide on impulse during strong emotion . . . and are not psychotic;" and (d) individuals who "commit the act after quiet deliberation . . ." His second classification of the various motivations of suicide include nine types, in which "the various categories overlap each other and our diagnostic labels are only shorthand for unique individual histories." The categories are: (a) suicide as the idea of magically being killed; (b) suicide as communication; (c) suicide as revenge; (d) suicide as magic murder and fantasy crime; (e) suicide as unconscious flight; (f) pathic (egoistic) suicide; (g) suicide as conscious flight (anomic suicide); (h) suicide as magic revival; and (i) altruistic suicide.

This second classification especially is of interest to all individuals who are concerned with suicidal behaviors. It may lead to confusions, however, precisely in the overlapping of the several *kinds* of classifications contained within

it. Among them are: (1) the role of the individual in his own demise. This is excellent in that no potentially worthwhile or useable classification of suicidal phenomena can ignore this dimension. (2) The role of magical thinking, which appears in three of Meerloo's types, but which—if one accepts the concept at all—may run like a gossamer skein through all of them. (3) The role of communication, which is probably present in all suicidal behaviors, certainly all those in which there is a "significant other." (4) The status of the individual vis-a-vis his society. In this connection it is refreshing to see reference to Durkheim's work in this volume. (5) The various roles of factors which are deemed to be conscious, unconscious, impulsive, deliberate, driven, etc. All these seem to be contained within Meerloo's suggested classification, but seem not to have been sorted and systematized as much as they might have been. But, withal, Meerloo makes a laudable effort to step beyond the usual threaten-attempt-commit trichotomy (which seems to be so much in vogue today) and to eschew purely diagnostic labellings.

The very title of this book is a fair play on words: *Suicide and Mass Suicide*.—Meerloo has a previous book called *Delusion and Mass Delusion*—as though the two were identical or, at least, similar. This thought raises some important issues not explicitly dealt with within the book itself. For example, if there is any meaning to the analogy or metaphor of suicide with mass suicide, can one also achieve any understanding of the threat of "international suicide" through the paradigm of tensions within a dyadic relationship? Specifically, would it be possible, from what we have learned about the reduction of tensions of two marital partners (who hate each other within the context of an inescapable marriage) to make suggestions that might be applied meaningfully to the stressful dyadic relationship of Aunt Vanya and Uncle Sam? Much has been learned by psychologists and psychiatrists from work in family therapy and from studies of stressful dyads on how to mollify interpersonal tension, suspiciousness, hostility, potentially destructive behavior, and these insights are being tapped by a number of psychologists interested in reducing international (potentially mass-suicidal) frictions. In



this connection, the thoughts and concerns and suggestions of Osgood, of Murray, of Ralph K. White, to mention a few, especially commend themselves.

It is evident throughout the book that the memories of the German crematoria are not—nor should they be—banked in Dr. Meerloo's breast. (The book is dedicated "To my brothers and sisters, who lost their lives on the altar of Hitler's destructive and suicidal delusions"). This book is, in some ways, one more opening of those satanic furnace doors, and in reading this book we are at times seared by the heat even if we are not continuously blinded by the light. Understandably a large part of the wisdom that Meerloo imparts seems to stem from his experiences with the dark side of life. Perhaps too much. And this very possibility raises one of the most interesting issues in this book. One of Meerloo's main themes, caught in the following passage, seems to be: "In a world where primitive drives are exalted into heroic ideals, man has to murder either himself or others. He kills something in himself by throwing himself into the turmoil of his instinctive drives. It may be called killing his ego or murdering his inner steering pilot." We are reminded of that remarkable scene in *Moby Dick*, in the chapter called "The Try Works" (Chapter 96), in which Ishmael, gazing into the awful "redness, the madness, the ghastliness" of the whale crematorium on board ship is induced to an "unnatural hallucination" that might have led to the destruction of his world, and unknowingly and confusedly becomes turned about, facing the stern, away from the guiding compass. "Uppermost was the impression, that whatever swift, rushing thing I stood on was not so much bound to any haven ahead as rushing from havens astern." The moral seems clear to Ishmael: It may well be (as Meerloo implies) that knowledge of darkness, along with knowledge of light, is an indispensable ingredient of the highest wisdom, but one must also

beware of gazing into the Hells of the world too long lest one becomes turned about and lose his sight of the compass and his grip on the tiller.

Perhaps Melville wrote in an overall happier world—threatened only by mammoth intra-psychic conflicts—that did not know the crematorium and the

bomb. Meerloo is not Melville (nobody is), but nonetheless, he reflects with a great deal of poignant confusion and poignant accuracy—with wisdom, deep intuition, and, at times, unabashed openness and self-confession—much of the suicidal agony of our age.

## Science Keeps on Becoming

Thomas S. Kuhn

*The Structure of Scientific Revolutions.* Chicago: University of Chicago Press, 1962. Pp. xv + 172. \$4.00. (Also: International Encyclopedia of Unified Science, Vol. II, no. 2, University of Chicago Press.)

Reviewed by EDWIN G. BORING

Thomas S. Kuhn, the author, in the words of the reviewer, is "a bright young man of forty, who took his bachelor's degree summa cum laude at Harvard in 1943, spent four years in graduate study at Harvard, years during which he shifted his interest from physics to the history of science, and then with an early PhD under his belt was chosen to join for three years the free intellectual community which is the Harvard Society of Fellows. He went to the University of California, Berkeley, in 1956, expanded presently under a Guggenheim Fellowship, and then met the social scientists at the Center for Advanced Study in the Behavioral Sciences. He is now Professor of the History of Sciences at Berkeley." The reviewer, Edwin G. Boring, in the words of the reviewer, "was born in the year Mach published the *Analyse der Empfindungen*, crept in under Titchener's wing at Cornell one month after William James died and so also a month after Wertheimer got off the train at Frankfurt to found Gestalt Psychology, was granted a PhD one day before he married but a whole year after Watson delivered behaviorism to a waiting world, went to Harvard just after the *Psychologische Forschung* got going, and thinks he founded CP in the centennial year of Freud's birth."

SCIENCE used to have resilient theories that persisted for long years defying contradictory facts. Does not every academic psychologist now over fifty years old remember teaching sophomores the Hering theory of color vision in which complementary red and green were bluish and not the primary hues, a theory which Christine Ladd-Franklin tried to fix up with another theory that would not explain other anomalies? It was of such theories that B. F. Skinner quipped that C.N.S. must mean the Conceptual Nervous System, fashioned in the imagination of psychologists rather than from the observations of neurologists. Yet these theories proved amazingly viable. Max Planck remarked that often a theory stands up against contradicting facts until its authoritative author dies. J. B. Conant has said that a theory withstands contradiction until a better theory arrives to replace it. That phenomenon is the science's horror vacui in respect of theories. J. J. Thompson commented away back in 1907 that a scientific theory is really a policy and not a statement of truth after all. Theories persist as long as they are usable.

In the last couple of decades the model has been brought out to replace the theory, which has fallen into dis-



repute because it has claimed to state truth without assimilating its contradictions. Truth is all-or-none. There are would-be truths but no half-truths. The model does not claim truth-value. It is an aspiration for a generalization. It may be employed for a limited universe. You see how well you can get your data to fit, perhaps adjusting the model to make the fit better. If the fit is good, you have a good summary of these data and then you may use the model to predict other data and test it empirically. If the prediction is borne out, the model gains in dignity and importance.

Now here comes T. S. Kuhn with his paradigms. A paradigm is a model for the way scientific thinking should go, the presumption implicit in investigation, taken for granted when it is being used, carried most of the time unconsciously in the stream of the *Zeitgeist*. A scientific revolution is a change of such a paradigm, and the more fundamental and inclusive the paradigm, the bigger the revolution. Actually paradigms are of all sizes, but the big ones are the best known and thus the most instructive.

It was a change of paradigm from Ptolemy to Copernicus. The earth is the immobile center of everything. The earth is relatively small in an enormous universe and is going like Jehu. There was a paradigmatic change from Aristotle to Galileo. The pendulum is a body falling under complicated restraints. It is a freely swinging weight with its timing independent of its amplitude. The change with Newton was from the interaction of small particles of matter to universal mutual attraction acting at a distance without contact. Attraction joins size, shape, position and motion as a primary characteristic of matter. Darwin officiated at the change from the goal-directed universe to a merely caused world. Organisms no longer changed in accordance with a Divine plan but merely as the inevitable outcome of conflict.

And then—leaving Kuhn behind since he does not deal with psychology in this book—we may note the change of paradigm from Cartesian dualism to modern behavioristics, a change not

yet fully accomplished, and the change from a Wundtian morphology of consciousness to the field dynamics of Gestalt psychology, a change which this reviewer thinks is fully accomplished—for everyone takes field theory as a matter of course today. These revolutions were small and altered no world view. Psychology has had as yet no big revolution, and perhaps that is why it seems to have had no Great Men. Freud may yet turn out to have shifted the largest paradigm, but your reviewer leaves its specification to posterity—precious, eagle-eyed, unmyopic posterity to whom so much of present confusion will become scrutable.

A REVOLUTIONARY change is both abrupt and slow, that is to say, it is of an all-or-none character but may require many years before it gains general acceptance. Kuhn uses Jastrow's duck-rabbit ambiguous figure from *Fliegende Blätter* as his illustration. You see the duck or the rabbit, never something in between, never a rabbuck. So with the paradigms. You fluctuate between them, now the old, now the new, and finally in the mind of the Great Man who becomes the Eponym of this change—Copernicus, Newton, Darwin—the new prevails. The slowness results from the fact that the change is continuous although all-or-none, for it has been conceived before the Great Man contrived to validate it, yet it began as an occasional minority view. It ends up eventually, of course, when the revolution is resolved as the majority view. Toward the end of a revolution the new paradigm has come to be the view of nearly all the experts—although Einstein could still wonder whether God himself was not able to transcend complementarity and know both the position and the velocity of the electron at once. Then the paradigm filters down to the public who almost universally had accepted the conservation of energy and repudiated perpetual motion before Einstein changed the world by writing  $E = mc^2$ . Kuhn is wise in choosing perception for his paradigm of paradigms: in perception the view of the world shifts suddenly and completely, for meanings

are discrete. "Why, how did that camel ever get over there in that field? Oh, no; it isn't a camel after all; it's a pile of stones."

How do revolutions get resolved? They get resolved by succeeding. Many little ones die in infancy. But Kuhn has this to say. Science does progress, more certainly and more obviously than do art, political theory and philosophy. That, indeed, is almost a definition: science is that discipline in which progress is the distinguishing feature. Progress means that all of the past remains embodied in the present. When a fundamental paradigm changes, then the scientists are not only busy fitting what were anomalies under the old paradigm into the new, but they are also to be found industriously rearranging all the old consistent facts within the new setting. The new paradigm does not disintegrate the old observations; actually it gives them new life in a new significance. It is this capacity of science for the new to absorb and revitalize the old that makes a paradigmatic revolution irreversible. A revolution may fail, but there is never an exact counter-revolution. A new revolution may eventually yield to a newer, but finally the past paradigm is found to be gone forever, eviscerated of its facts by the new paradigm that absorbs them.

Kuhn thinks that science has no goal, that its progress aims no whither. Certainly the acquisition of truth is not its goal and Kuhn uses that word *truth* in his essay only to note its inappropriateness. To this reviewer it seems that Kuhn missed his chance to point out the analogy between the Darwinian biological revolution in paradigm and the Kuhnian one in epistemology. "La science n'est pas; elle devient," said Flourens long ago. So science keeps on becoming, just as organisms do. Organic evolution is controlled by the ecology of living. Scientific evolution is controlled by the economy of thinking. If wisdom—not knowledge but wisdom—stretches further and further beyond the range of man's possible apperception, still the paradigms keep it under noetic control. They have to, for man creates the paradigms for this very purpose.



Except for paradigmatic restraint, man's irresponsible idiosyncrasies would scatter science to the four winds. Thus the paradigm has that dual role: it conserves the past and yet leaves escape possible for him who is wise and who dares.

It's a brilliant book, this. If this paradigm of paradigms could ever filter down to general public acceptance, how the misapprehension of scientific work would be diminished!

## Elusive Psychotherapy

Walter Bromberg

*The Nature of Psychotherapy: A Critique of the Psychotherapeutic Transaction.* New York: Grune & Stratton, 1962. Pp. vii + 108. \$4.50.

Reviewed by SOL L. GARFIELD

*The author, Walter Bromberg, is a psychiatrist and a prolific writer. Probably his best known books are Man above Humanity: History of Psychotherapy (1954) and, with Winkler, Mind Explorers (1939). The reviewer, Sol L. Garfield, is currently Professor of Medical Psychology at the University of Nebraska College of Medicine and also serves as Chief, Psychology Division, Nebraska Psychiatric Institute. He did most of his graduate work at Northwestern University where he received the PhD in 1942. He has functioned as clinical psychologist in several VA hospitals and clinics and has taught at the University of Connecticut and at Northwestern. He is a diplomate in clinical psychology, is currently secretary-treasurer of APA's Division of Clinical Psychology, and author of Introductory Clinical Psychology (1957, CP, Apr. 1958, 3, 101f.).*

THIS is a small book which attempts to analyze the basic meaning and nature of psychotherapy. Although this reviewer was mildly elated at being as-

signed such a potentially non-arduous task, this feeling was tempered by a later realization that the task was not quite as simple as it first appeared. The book turned out to be primarily a philosophical inquiry and one written in a style both heavy and abstruse. Furthermore, upon completing the book, I found it quite difficult to summarize.

Dr. Bromberg begins by stating that despite our clinical knowledge of the operations in psychotherapy, "the exact nature of psychotherapy has eluded psychiatrists. We do not know what psychotherapy is, but we do know what it does" (p. 1). The aim of the book is thus to appraise the essence of psychotherapy and also to attempt to analyze "the fundamental reason why psychotherapy of any type is successful in human beings" (p. 2). Although many psychologists might question whether we actually know what psychotherapy does, they would agree that the objectives set by the author are of basic importance for psychotherapy.

Essentially, what the author then attempts is a logical analysis of the psychotherapeutic interaction. He makes reference to the fact that most therapists are so influenced by the theories and constructs employed in their work that they cannot really see what is basic in psychotherapy. In this regard he points out that attempts to evaluate the explanations made by one school of psychotherapy in terms of another might produce explanations of the two schools in comparable terms but he questions whether this type of research would throw light on the basic question of "how and why psychotherapy functions successfully" (p. 3). His view is that the various approaches to psychotherapy do not really explain it as a "generic subject." As a result he is skeptical of what is "known" by current schools of psychotherapy and believes that one must go beyond the theoretical premises which are utilized in current psychotherapy. These only color and preclude attempts at getting at the essential factors in psychotherapy. "In order to view the relation existing between the members of the therapeutic dyad in its formal nakedness it would seem evident that explanations must be cleared away" (p. 14).

After these statements by the author one is led to expect some novel and basic appraisal of psychotherapy. However, the ensuing discussion is actually quite general and disappointing. In addition to discussing two postulates for the therapist, one arising from biological science and the other from psychological science, the author makes reference also to the "artfulness" and "conative drive" inherent in the therapist. The latter refer to non-technical aspects of therapy and what I interpret as the personal style of the therapist. The discussion of the underlying premise of the patient can also be summarized briefly. The patient's role appears to be that of accepting the interpretations offered by the therapist. The patient employs an "as if" premise—he behaves as if what the therapist tells him is valid. For this reason belief on the part of the patient is seen as important in the therapeutic interaction. Furthermore, according to Bromberg, the patient is the only common variable in psychotherapy.

In summary, this reviewer believes that some of the problems raised by Bromberg are of importance in psychotherapy. What one observes and what one looks for in psychotherapeutic practice or research is obviously influenced by his own theoretical views. However, I doubt that the analysis offered by the author will be of great help in increasing our awareness and understanding of problems in psychotherapy or in the planning of research in psychotherapy. In fact, much current research in psychotherapy is not included in the list of references.



*As long as sense was supposed to be the chief factor in knowledge, psychologists took a prime interest in the organs that were the windows of the mind, and in the details of their functioning; other things were accorded a sketchier and sometimes vaguer treatment. If scientists demanded, and philosophers dutifully admitted, that all true belief must be based on sense-evidence, then the activity of the mind had to be conceived purely as a matter of recording and combining; then intelligence had to be a product of impression, memory, and association.*

—SUSANNE K. LANGER



# Five Elementary Texts and One Question of Identity

Frank A. Geldard

*Fundamentals of Psychology.* New York: Wiley, 1962. Pp. 437. \$7.50.

Ernest R. Hilgard

*Introduction to Psychology.* (3rd ed.). New York: Harcourt, Brace & World, 1962. Pp. 678. \$7.75.

Norman L. Munn

*Introduction to Psychology.* (4th ed. Abridged edition of *Psychology: The Fundamentals of Human Adjustment*). Boston: Houghton Mifflin, 1962. Pp. xiv + 588. \$6.95.

Aaron Quinn Sartain, Alvin John North, Jack Roy Strange and Harold Martin Chapman

*Psychology: Understanding Human Behavior.* (2nd ed.). New York: McGraw-Hill, 1962. Pp. xi + 432. \$6.95.

S. B. Sells

*Essentials of Psychology.* New York: Ronald Press, 1962. Pp. xvi + 512. \$6.50.

Reviewed by JOHN K. BARE and PETER M. GUTHRIE

both of whom teach introductory psychology at Carleton College, both of whom received their PhD's at Brown University and both of whom taught at the College of William and Mary before moving to Carleton. John Bare was Carl Pfaffman's first PhD and is proud of it. Currently his research focuses on the functioning of the limbic-midbrain system and on the study of biological clocks in the eating behavior of rats. This Guthrie, P. M., is connected in interest and through a line of academic forebears with another Guthrie—E. R., that is. Our present Guthrie worked with Stanley who worked with Sheffield who worked with

Guthrie. And he now studies human behavior in the closest possible analog to the puzzle box.

EVEN if it is not true, it is sobering to consider the possibility that Psychology itself, alas, is suffering from a neurosis. As Hobbs has described it (*American Psychologist*, 1962), "The problem of the contemporary neurotic is not a lack of insight but a lack of identity, of purpose, of meaning in life. Nothing can make a person more anxious, or more guilty, than an unrelentingly clear appreciation of the absurd and desperate condition of man today."

A reading of five new texts for the first course, hard upon the heels of a review of eight others in this journal a year ago, suggested that the editor might simply reprint those earlier uncomplimentary remarks. But the authors of these and those elementary texts are highly capable people who have carefully considered the problems inherent in this particular kind of literary and pedagogical venture, and in spite of the obvious difficulties, have striven mightily to be the fourth BL&W or the new Munn. Even Munn tried it. Perhaps the elementary textbook is not neurotic. Perhaps Psychology is. What supporting evidence is avail-



able? The patient has said in the Michigan-NSF Conference on Undergraduate Curricula: "Early in the conference, each member spent some time drawing up a list of principles he thought important enough to be included in the undergraduate curriculum. Although we were interested in and respected each other's lists, it was immediately apparent that even though each of us could compile a set of principles that met the criteria, we could spend our whole time attacking and defending our choices, arguing about the implications of certain terms, the inclusiveness of the principles, and sometimes (alas!) even their truth. We soon decided that we were not constituted to draw up an approved list of principles . . ." Speaking of another Hobbes, Koch has observed (*American Psychologist*, 1961) that contemporary psychology "has sold to man an image of life as being nastier and more brutish, if longer, than any Hobbes could have entertained—an image which could leave the humanist only the role of the idle *voyeur* peering tenderly into a sewer." He had said the same thing in his Epilogue to *Study of a Science*.

What can be done? One therapist has demanded that we throw out the methodology with the subject matter (cf. Rollo May, *Existential Psychology*), another advises that we consider our *self* and relate it to our present personality (cf. D. O. Hebb, *American Psychologist*, 1960). The first recommendation is foolhardy, if not inhuman, and the second course of action is only dimly perceived. None of the present writers either attempts the one nor achieves the other.

These texts have many similarities, both in the selection and the treatment of the material. To this extent psychologists agree. But the very selection dictates discontinuities, and theory is sacrificed, with one exception, either to the cataloguing of facts or to the demands of pedagogy. But in spite of the similarities, the textbooks differ, and the discriminations are marginal. There we go again, perseverating. Here are the differences.

**G**ELDARD. Here is the fourth BL&W, according to Boring's Foreword, and

certainly not denied by Geldard in his Preface. But there is much of Geldard in Geldard. That is, he thought hard and wrote hard with his eye on the lodestar, the third BL&W. The selection of the material and even some of the illustrations are dimly familiar. Fundamentals implies Foundations, and you sink them deep; you build them strong and you build them forever, knowing that the upstairs remodeling, when it comes, merely reflects the fashion of the day.

Three themes—Motivation (including Emotion), Learning, and Sensing and Perceiving—are presented in the first 148 pages and developed in the same sequence in the next 141, but the Sonata form is prior to Beethoven, who introduced the integration of the themes and displayed their interrelationships. The last third of the book follows the intermission, and we hear some favorite short selections.

It is a bit heavier on sensation and perception, naturally, than some will want, and a bit leaner on the physiology of the receptor processes, naturally, than others will wish. The problem of the definition of learning is solved by adopting a Skinnerian increase-in-probability view, but one is a bit startled to discover that Hull is not included in the bibliography, and is mentioned only in connection with organic needs in juxtaposition to Freud, and as having named the *goal gradient* principle. Social is experimental after the several editions of *Readings in Social Psychology* by T. M. Newcomb and friends. The chapter on the Nature of Personality considers types and turns to measurement, and Freud gets his major treatment in terms of the dynamisms in the chapter entitled "The Interplay of Motives." Self, "for purposes of science . . . will not do."

**H**ILGARD. The sheer quantity of material in this volume is staggering, and thus few instructors will be disappointed because their favorite topics are not treated somewhere. Moreover, this book is meant to be contemporary, for one-third of the some 1000 references have appeared since 1957. Recall that one-third of the references in the

second edition had appeared between 1950 and 1953. As a consequence, there is some doubt that a single teacher could use the book without serious difficulty. He would need considerable re-tooling if he is to avoid a realistic anxiety that the capable and interested student could lay bare his ignorance. Or he might choose to omit several chapters. But combined with this coverage of recent developments is the author's perceptive sensitivity to the problems we face.

In this edition, the critical discussions continue, but references are no longer footnoted, and experiments are not presented in bold face. As is true of all the newer editions in this group, the book is more attractive than its predecessor.

The section on Growth and Development is expanded with greater emphasis on social growth, a refreshing contrast from the usual treatment of genetics and the norms of verbal and motor behavior. Theories of Personality is a chapter that discusses theories, from type to self. Theories of learning are described, of course, to include an introduction to Estes' probability model. Orienting reaction, release therapy, pre-cognition, intrinsic cortex, standard error of the correlation, scalogram—indeed, it's all here.

**M**UNN. To continue the musical metaphor, Munn is for Bach fans. The pace is constantly driving and sure, and the style is lucid. New research findings are skillfully integrated without comment and without excitement.

Carmichael, in his foreword, compares this shortened version to James's *Briefer Course*. The similarity lies mainly in the shortening; James's delightful light touch is missing altogether. But Munn's skill as a teacher and writer is plainly apparent, for programming is one of his habits and the student's understanding is as certain as the author can make it. Expect that the student will need, in Hilgard's terms, intrinsic or extrinsic motivation.

The shortened section on learning remains inclusive and well written, except for the topics of generalization and



discrimination; the short shrift for these matters is strange, particularly in view of their prominence in current research and their explanatory power in theories of learning and of personality. Theory is largely still missing throughout, here and in most elementary texts. This phenomenon is a bit difficult to understand in view of the fact that the texts purport to survey modern psychology.

A chapter on communication and language, together with one on remembering and forgetting, will provide a good introduction to the rapidly growing field of psycholinguistics. The recent contributions of Underwood, G. A. Miller, Osgood, and others in this area are described, and use is made of a number of Skinner's concepts.

The glossary is complete and will no doubt be a comfort to those students who use it. It suffers from many of the difficulties which Verplanck encountered—many of the definitions are simply "intraverbals," and will do little to increase understanding even though they may provide for many a multiple-choice item.

**S**ARTAIN, STRANGE, NORTH, and CHAPMAN. The authors see their task as the presentation of material that is "meaningful in terms of the beginning student's experience." If you seek meaning in terms of tentative hypotheses or miniature theories, it is not here. Conclusions are stated without qualifications, and the reader feels none of the frustration nor any of the intellectual ecstasy that occurs in solving the problems of behavior. The book becomes non-controversial, simply because it treats the students and psychology in such a relaxed and gentle manner.

A second edition was required, the Preface tells us, because of recent developments in learning, genetics, and color vision. In response to the need, the learning chapter has become two (which adds nine pages), DNA and RNA are briefly considered, and Land's findings are described.

The line illustrations have been redrawn and are handsomer than in the first edition. Research is described in the box separate, questions and an an-

notated bibliography appear at the end of each chapter, and there is a glossary.

This book, more than any other, drove us to re-read *THE LITTLE BOOK*, (W. Strunk, Jr. and E. B. White, *The Elements of Style*)—"style in the sense of what is distinguished and what is distinguishing. Here we leave solid ground. Who can confidently say what ignites a certain combination of words, causing them to explode in the mind?" We report no explosions.

**S**ELLS. One way to avoid the conflict produced by the influx of new researches, new viewpoints, and new methods is to repress them. Although Sells' *Essentials* aims to "present content essential to the formation of a solid foundation for understanding and studying the science of Psychology," the content, except for the final chapters on group processes, is behind the times. There is little discussion, for example, of such topics as imprinting, species-specific behavior, the reticular system, self-stimulation, or the current problems in human verbal learning.

There are difficulties of exposition too. While chapters on motivation and personality, abilities and intelligence, and the measurement of personality rely heavily on the work of Cattell, Guilford, and Murray, the terms correlation, reliability, and validity are not indexed and indeed are nowhere explained. They are used, however, and the better students may acquire meanings for them inductively.

The emphasis, wherever possible, is on applied problems, and the instructor who is concerned that his students learn the fundamentals first may be dismayed at the selection of materials for the first section.

The final chapters on group processes are the best part of the text, but they are concerned with highly specialized problems such as the decentralization of power within organizations. With the exception of that final section, the book is probably pitched too low for most elementary psychology courses, freshman courses included.

\* The premise of this review set the tone. In spite of the fact that as psychologists we should know better, like most teachers and parents we punish failures and fail to reinforce successes. Each of these texts deserves and will find support, and we confidently predict the necessity for a similar review in the next year.

## Collected Eclecticism

James A. Dyal

*Readings in Psychology: Understanding Human Behavior.* New York: McGraw-Hill, 1962. Pp. 444. \$3.95.

Richard A. King

*Readings; for an Introduction to Psychology.* New York: McGraw-Hill, 1961. Pp. 388. \$3.95.

Reviewed by NEIL A. CARRIER

James A. Dyal, editor of the first book, took his 1957 PhD degree from the University of Illinois, where he started out in clinical psychology but moved over into the experimental area to deal with animal learning. Now he is Associate Professor of Psychology at Texas Christian University. Richard A. King, editor of the second volume, took his PhD degree in 1959 from Duke University. His basic research interest while at Duke focused on animal learning, but now, as Assistant Professor at the University of North Carolina, he is occupied with the physiological basis of memory. The reviewer, Neil Carrier, received his PhD in 1956 in Social Psychology from the University of Michigan. While at Michigan he was both a teaching fellow and predoctoral instructor and in these capacities worked closely with Wilbert McKeachie. He is now Assistant Professor of Psychology at Southern Illinois University where he is in charge of the introductory course and is supervisor of the undergraduate curriculum in psy-



chology. He maintains a research interest in college teaching and is now most concerned with the emotional concomitants of learning. At the moment he is preparing an evaluation manual which will accompany McKeachie's forthcoming introductory text.

A CONCOMITANT of soaring enrollments is the current spate of 'readings' books. Such collections are usually justified as supplementing inadequate library facilities by bringing the student a well-chosen sample of original literature in an economical package.

The two books reviewed here were designed to help fill the void in the introductory general psychology course. Either should give the interested student much information and insight about psychology, both reflect generally judicious selection, but neither is economical.

Dyal has collected 56 selections in 13 chapters titled like those of the typical introductory textbook. After the introductory chapter on "Psychology: Past, Present, and Future," each of the remaining chapters is sub-divided into three sections: "biological factors," "psychological factors," and "sociocultural factors." Each section is usually represented by one or two selections. The organization and nature of the selections are said to present an "organismic approach" which is "more understandable to the beginning student." The editor's three objectives were to help the student learn about experimental and clinical methods, about the knowledge they have produced, and to modify his attitudes about man in general and himself in particular. About one-quarter of the selections consists of reports of single experiments, while another quarter gives summaries of long term research programs of outstanding experimental psychologists (e.g., Miller on conflict and drugs; Harlow on love). These two quarters principally serve the first two objectives. The third objective is met with the remaining one-half of the selections—essays and theoretical papers (e.g., Horney on culture and neurosis; Brown on sex-role development in a changing culture). The editor's criteria

in meeting these objectives were to select articles representing major content areas of psychology, which are not of a purely applied nature, and which facilitate an understanding of human behavior.

King's book was designed to accompany Morgan's *Introduction to Psychology* (2nd ed.) but should supplement other texts almost as well. He collected 60 selections under the 20 chapter headings of the text. Each chapter is divided into anywhere from one to four of the text chapter sub-headings. Each of these topics is usually represented by one or two selections of which about four-fifths are reports of specific research studies. The editor's declared five criteria are reasonable: selections should be largely empirical rather than solely speculative or theoretical (to counteract the misconception of psychology as a 'talking' discipline), a number of historically important 'classics' should be included, articles should use little psychological jargon, should be intrinsically interesting to the student, and should 'match' the Morgan text without being disqualified for use with other texts.

THE EDITORS differed in their treatment of editorial comments. Dyal restricted his to about two pages of introduction to each chapter. These comments highlight important problems in the chapter, accent certain results and implications in the selections, and clarify relationships between chapters. King's comments are more deliberately didactic. Each article is introduced with one or two paragraphs of comment and usually one or more "points to guide your study" (e.g. definition of a term used in the article, provocative questions to keep in mind while reading, methodological niceties to appreciate). After the reviewer got over his initial impression of 'pre-digestion' and cognitively switched his role from psychologist to 19-year old, he found he preferred this approach. Especially if an article reports an experiment, a brief preview of the problem and general approach of the study should help set the reader for maximum understanding of the article's

content. (If this be mollycoddling, please don't throw brickbats; throw evidence that it helps none and/or hurts some.)

The books also differ in their treatment of the end-references which originally accompanied each article: Dyal omitted them; King included them. The reviewer feels that the devotion by the latter of the equivalent of 22 pages to the total of 796 references is neither educationally nor economically justified.

Both editors abridged about one-half of their selections. One might wonder how meaningful to students some of the excerpts in the history section of Dyal's first chapter would be, and might question King on whether the Ames article on the trapezoidal window would be meaningful to students without (or even with) a demonstration, and on whether the 17 pages of Harlow's report on affection in infant monkeys should have been left uncut. But a sampling indicates both editors performed their selection and abridgement tasks quite well.

Most of the selections are of relatively recent vintage: about three-fourths of Dyal's were originally published since 1950 and about two-thirds of King's. The median year is 1955 for both. Compared with three other recent books of readings, Dyal shares two selections with one of them and one with another, while King shares three with one of them. In keeping with this general lack of overlap, King and Dyal have only one article in common.

In sum, here are two reasonably well-edited collections for the introductory course. In their selections and statements both editors show a commendable tolerance for different points of view. While the diversity in content and objectives from one introductory course to another is so great as to limit the applicability of a recommendation, it can be said that here are two acceptable candidates for courses of the non-monomaniacal variety.



*Each dog barks in his own yard!*

—RUDYARD KIPLING







#### LABELS THAT MISLEAD

SOME months ago, *CP* received from the Philosophical Library a book which, perhaps because of its labeling, has caused unusual but not necessarily profitless trouble. On thumbing the book, *CP* decided that it fell on the borderline of appropriateness for the journal. Clearly it could not be cast into the pile of rejects along with *Successful Selling through Hypnosis*, *Zen, Yen and You*, *The Virtue of Sexual Promiscuity* and *Self Surgery for Self Discovery*. Nor could it with confidence be stacked with the somewhat less interesting, more proper and perhaps equally inappropriate volumes *A History of Ethics*, *Political Essayists*, *Medical Economics in the Nineteenth Century* and *Moses and Monads*. The book dealt with existential philosophy. It was borderline. So, as with books of more obvious appropriateness, it was sent to a consultant who could advise about it. It was and he did.

As far as anyone consciously knows, neither *CP* nor its consultant was influenced by the book's label or jacket. But it remains a reasonable retrospective hypothesis that a look at the cover helped form an impression of what was inside, and that the book was in effect bought because of its cover. At any rate, the consultant, too, thought the book was borderline but recommended that it be given at least a brief review, particularly if such a one as Michael Scriven, Professor of the Logic of Science at Indiana University, could be persuaded to do the review. Professor Scriven could be persuaded. While none should discount the brainwashing potency of *CP*'s techniques for the engineering of consent, it is nonetheless likely that the official

title of the book also helped persuade Dr. Scriven to take on the reviewing assignment, and to do so in the face of the fact that he, like all of *CP*'s reviewers, was much too busy to accept the job. He also, then, must have judged the book by its semantical cover. But *he* took it home for a careful reading. Soon there arrived a letter from him saying, first and with thoughtful documentation, that he judged the book inappropriate for review in our journal and, further, that he was disturbed by what seemed a considerable discrepancy between material on the book's integument and material appearing within. Apparently, its beauty for him had turned out to be only skin deep. *CP* considered the question of appropriateness and, in the light of its own earlier doubts and those of the consultant, accepted the recommendation that there be no review. That was that. (So far, this was all a relatively normal procedure and one that is probably quite proper. The final decision might still be wrong but, if so, no one knows a more workable way to be so carefully and thoughtfully wrong.) The alleged mislabeling of a book, however, cannot so easily be disposed of. It is a matter of at least a literary import, and almost incontestably of more than that. *CP* asked Professor Scriven to write printably and more fully on his experiences with and impressions of the book. He did. Here is what he wrote:

One of the minor irritations of the browser in bookstores is the extent to which the fashion for misleading titles has spread. As a result, a book with the title *Human Nature in War* may turn out to be a study of the diaries of a sublieutenant in the Civil War, simply a minute piece of historical scholarship with no such gen-

eral impact as its title promises. Recently we have come across an example of this tendency invading at least the philosophical if not the psychological field. It is a book titled *Existential Thinkers and Thought* with the names Kierkegaard, Heidegger, Jaspers, Sartre, and Marcel following this title on the front of the dust jacket and "by Frederick Patka" underneath. This might be construed as either an anthology of the writings of these people edited by Mr. Patka or, perhaps more likely, as a study of these people by Frederick Patka. It turns out to be neither of these. The book consists partly of an essay by Frederick Patka on certain general problems of philosophy, connected with existentialism, plus five essays by five quite different authors, one on each of the named existentialists. Either the other authors should get their names on the cover along with Mr. Patka's, or none of them should, in which case certainly the names of the specific existentialists should not occur there since this suggests very strongly that they are represented by their own work within the covers. This kind of problem is like the problem of giving acknowledgements for assistance on published research work; a little thought about the morality of misrepresentation in the field of publications is required when other people's reputations are involved.

WHAT does *CP* do about such a development? Start banning or burning books? Or punishing publishers? Certainly not. But it does seem appropriate and maybe even worthwhile for *CP* to do what it has just done. It should talk and encourage talk about both the semantics and the proprieties of titles. *CP* thinks it has held steadfastly to the position that books are written, published and reviewed by people and that all this is done for other people. The complex communicative processes involved allow extensive but not limitless freedom, great but not wayward idiosyncrasy. There are restraints. Writing itself requires great control, containment, harnessing. If it is to be more than narrowly narcissistic self-express-



sion. Publishing and reviewing involve other restraints and containments.

And external realities demand of freedom mostly that it make peace with prevailing standards of intelligibility, of politeness, occasionally of elegance. Sometimes questions of propriety also arise. The latter are more likely to occur in titles than in

paragraphs, more likely in advertising than in reviewing. When they arise in connection with books in which CP's readers have an interest, the departures from propriety should themselves be clarified, as they occasionally have been (*CP*, June 1962, 6, 246), and as now.

—F. H. S.

## Experiencing and Talking

Eugene T. Gendlin

*Experiencing and the Creation of Meaning: A Philosophical and Psychological Approach to the Subjective.* New York: Free Press of Glencoe, 1962. Pp. v + 302. \$6.00.

Reviewed by JULIUS LAFFAL

The author, Eugene Gendlin, received his academic training at the University of Chicago under Carl Rogers, who has had a profound influence on his thinking. He received his PhD in 1958 but long before that began training under Rogers as a psychotherapist and research worker at Chicago's Counseling Center. Since 1958 he has been Research Co-ordinator of the Psychotherapy Research Group, Wisconsin Psychiatric Institute, University of Wisconsin. The reviewer, Julius Laffal, received his PhD in clinical psychology from the University of Iowa in 1951 and since then, except for a year in research in the Department of Psychiatry at Yale, has been with the Veterans Administration. Presently he is Director of Research, Psychology Service, at the VA Hospital at West Haven, Connecticut and also as Assistant Clinical Professor of Psychiatry at Yale. His research interests, as his publications indicate, include the psychology of language with a special concern for linguistic transactions in psychotherapy, and for the language distortions of schizophrenic patients.

THIS BOOK, by a student of Carl Rogers, is an effort to provide a philosophical foundation for a client-centered approach to psychological

problems and psychological research. At the same time it seeks to make a bridge between logical positivism which stresses operational definition and objectivity, and existential approaches which stress the importance of the subjective experience. Although Gendlin maintains that he differs with Rogers on the nature of *experience*, his book may be seen as an outcome of Roger's expressed view that "most urgently needed of all is a method whereby we might give operational definition to the construct *experience* in our theory." As Gendlin sees his task, it is to "make direct reference to experiencing possible for psychology, by means of an addition, rather than any alteration, of positivistic, behavioral, operational methodology." (p. 229).

The problem dealt with in this book will be familiar to those who have struggled with the nature of meaning, symbolization and language: that is, how to work validly with and speak directly of what is infinite in complexity, completely subjective, and ineffable—the feelingful experiencing of the individual. Much of Gendlin's book is an assertion and re-assertion of the primacy and superordinate importance of the *felt experience* as against conceptualizations of this experience in our efforts to know man and to know the world. In this respect he is solidly in

the existentialist camp. The source of all knowledge is the felt, immediate experience. More to the point for the psychotherapist, this felt experience is also the central feature of the psychotherapeutic process. Gendlin promises, and in the end says he has achieved, new methods capable of referring directly to the felt experience and different from the usual operational methods which are incapable of dealing directly with this experience.

GENDLIN starts with a dualistic position regarding meaning: one type of meaning is contained in relations of symbols to each other and to experiences, and one type is contained in the subjective or felt experiencing itself. The felt experiencing is the primary meaning. One is reminded of the well known triangle of meaning put forward by Ogden and Richards in *The Meaning of Meaning*, in which word, act of reference, and object make the three points; and also of Morris's and Carnap's division of the science of signs into syntactics (relations of symbols to each other), semantics (relations of symbols to objects) and pragmatics (relations of symbols to speakers). What is easily overlooked in these schemes is that relations between symbols and things (or events or experiences) cannot be independent of speakers, since the locus of all meaning relations lies within speakers. This error is fostered by the fact that language has a super-individual, communal quality which lends it an autonomous air. Gendlin has thus somewhat simplified matters by treating symbols as if they were or could be independent of the experiencing of the individual. Nevertheless, in a manner of speaking there are on the one side our felt experiences, and on another our symbolizations or conceptualizations of these experiences. Whereas felt experience is infinitely complicated, conceptualization is selective, partial and arbitrary. The crucial thing is the felt experience—not the conceptualization—and this is what the psychologist wants to study and work with. How may he deal directly with it rather than with conceptualizations?

One way of dealing with the sub-





EUGENE T. GENDLIN

jective experiencing or the felt meaning is by direct reference. Thus, in psychotherapy, the client says, "this feeling is sure strong, but I don't know what it is yet." This is a pointing to, rather than a conceptualization of, an ongoing feeling process. Gendlin offers an example of how this method of direct reference to experienced feeling is used in research. Counselors, alert to the distinction between immediate feelingful experience and conceptualization, rate the productions of clients in this respect. The patient who exclaims "I hate you!" experiences his feelings more directly than the patient who says, "I have this feeling of hate and it's for you." Another approach to experienced feelings is to have the client do a Q-sort in which he must respond to statements describing experiences which are not conceptualized, such as, "I felt something intensely but I didn't know what it was." Counselor ratings or client self-ratings referring to feelingful experiences may be correlated with responses on other tasks, such as TAT or Rorschach.

Of such ratings, Gendlin says that they are the beginnings of research directly measuring experiencing as distinct from conceptualization. In effect he proposes that we use the limited means available (essentially, pointing or referring to rather than talking about) as a first step toward bringing felt ex-

perience into research. Once the felt experience has been identified by this method of direct reference, it may then be related experimentally or correlationally to other operationally defined variables. Instead of limiting what we deal with by conceptualizing and naming, we will attempt to work with the undefined experience by simply pointing to it when it is there. For, "Experiencing" refers to something directly observable by the individual and observable by others indirectly in his expression of such direct observation. It is something present, although it is chiefly *felt* rather than *known*" (p. 242).

THE PROPOSAL to introduce an undefinable something (felt experience) as a central variable in psychological research and understanding in lieu of operationally defined variables will certainly crash against a thick wall of scepticism. Nor is Gendlin's palliative convincing: "However, terms defined by direct reference to experiencing need not obviate other terms defined by external observations. Of course, even if the same word be used, terms defined in these different ways must, and easily can, be carefully distinguished" (p. 268). This has overtones of a semantic bedlam in which meaning 1 and meaning 2 will constantly be getting mixed up. The undefinable felt experience may also serve something like an island of safety for those researchers who require a sacrosanct unknowable, although Gendlin has certainly been careful to offer his view not as a substitute for a positivistic approach but as a necessary complement.

Nevertheless, when Gendlin's illustrations of research strategy are examined, it appears that there is nothing very controversial in his methodology. Thus, although the felt experience is the thing being rated by counselors, and at which the client self-rating is getting, standard questions of reliability and validity may be raised and answered about the procedures, and the procedures themselves are operationally defined approaches to the felt experience. This is how one would go about study-

ing "anxiety" or "hostility" or a host of other variables which we know in an intuitive way.

The organization of Gendlin's book is somewhat unusual. The introduction and the final chapter are directed toward the psychologist. The intervening six chapters are directed toward the philosophic reader. As a whole the book seems unduly abstract and lacking in good illustrative material, especially for a work on the *felt experience*. It does, however, make an important point and this reader looks forward to the unfolding of its ideas in the research of Gendlin and his associates.

## Look Who's Talking

Eric Berne

*Transactional Analysis in Psychotherapy: A Systematic Individual and Social Psychiatry.* New York: Grove Press, 1961. Pp. 270. \$3.95.

Reviewed by GEORGE A. KELLY

The author, Eric Berne, is a physician who was formerly a consultant in psychiatry to the Surgeon General U. S. Army and who is now both a Lecturer at the University of California Medical School and Chairman of the San Francisco Social Psychiatry Seminars. The reviewer is George A. Kelly who has been almost everywhere, has done almost everything and is well known to almost everybody. He first appeared in CP as an author, when his two volume book *The Psychology of Personal Constructs* was reviewed. He has appeared often since as a reviewer. If CP's editor has anything to do with it, he will appear again.

IF THE reviewer had not made a prior commitment to CP this book would have been laid aside after the preface and introduction had been scanned. That would have been a pity. As it turned out, every word was read.

In spite of the author's off-beat way



of expressing himself he has come up with a delightful book about live persons caught up in the act of being themselves. Most psychiatrists, perhaps more keenly aware than he of what their profession expects of them, would have ground out a discourse about case histories, psychodynamics, or—nowadays—even such things as intervening variables. Yet with a bright-eyed Gallic sensitivity to what is child-like and what is tragic in our daily conversational exchanges, this man has contrived to produce a lucid paperback manual for group psychotherapists. Readers who insist that a book which is simple must also be trivial can save \$3.95 by not buying this one.

The volume's title notwithstanding, *transactional analysis* is only one phase of the therapeutic program the author describes, and, at that, perhaps not the most important one. He starts off with *structural analysis*. There is a sense in which each of us speaks with the voices of three selves. There is my *Adult* self—capitalized because it is used as a proper noun—I, my *Adult*. Then, sometimes, it is I, my *Child* self, or what I retain of him, who speaks. Sometimes it is neither, for I am not altogether a unique being either as I was as a child or as I pretend to be as an adult, but I am also my *Parent*.

If I say to myself, as I sometimes do, "I cannot understand why I do such irrational things," I am probably identifying, at the moment, with myself as the *Adult* I, yet I, in the sense that I am still a certain child—just as I have always been—volunteered the deeds I momentarily regard as puzzling. If I say to myself, "Come now! You oughtn't to do such things," it may be I, my *Parent*, who is sounding off. And if I say to myself, "I am fully aware of the futility of such acts," it may be I, the *Adult*, standing up to my two other selves—the *Child* who reached for what he wanted, and the *Parent* who disapproves of such goings-on.

NORMALLY, one keeps the initiative in the hands of his *Adult*, although both *Parent* and *Child* have something to contribute to the full life and should not be *decommissioned*. The first step in Berne's psychotherapy program is to

help the person analyze his structure so that he is in no doubt about which of his selves says what. This *structural analysis* has, in fact, three *ad interim* functions; to *decontaminate* the *Adult* of *Child* and *Parent* mannerisms, to define boundaries between the three, and to stabilize the situation so that the *Adult* can maintain a minimum of social control.

Now comes the next step—*transactional analysis*—but before we get involved with that perhaps we should say that what Berne describes as *Child*, *Adult*, and *Parent* are not equivalents of *Id*, *Ego*, and *Super-Ego*, but are all three *ego states*. Besides, they are not intended as psychodynamic abstractions, for the term, *Child* refers not to what may be childish in one's personality but to what he actually was as a child and still is. His *Parent*, similarly, refers to what his parent actually was and what there is of that particular parent that sticks with him. The terms refer to particulars, not to generalities labeled as traits.

Things begin to get interesting in Berne's groups when the participants start to realize whose *Child* is talking to whose *Parent*, and when they can tell the moment a person shifts the initiative to one of his other selves. During this phase the therapist does not necessarily confine his participation to that of his *Adult*. In advanced transactions he may invoke his own *Child* and invite a *Child-to-Child* interplay between himself and another member of the group.

It is at this stage that participants begin to understand which selves are involved in their long-term relations with others. An interpersonal relationship based entirely on transactions between one person's *Child* and another's *Adult* is not likely to last indefinitely, unless, I suppose, the *Child* is a patient and the *Adult*, if adult he is, is a well-paid therapist. Transactions can get crossed, too. For example, one person's *Adult* may address another's *Adult* but get a response addressed from the other's *Parent* to his *Child*. This does not make for a comfortable relationship at any age.

But if *transactional analysis* is interesting, then the fun really starts

when *games analysis* gets underway. Games such as *Wooden Leg*, *Run in My Stocking*, *Let's You and Him Fight*, *PTA*, *Ain't It Awful*, *Yes . . . But*, and *Rapo* ("Whaddya mean, I seduced you!") are widely played and group psychotherapy nails them for what they are.

Finally, there is *script analysis* and *relationship analysis*. Scripts are made up of sequences of scenes and the person with the script acts as his own casting director. The show goes on the road with repeated performances, although box office receipts are not always encouraging. Husbands who don't play their parts according to the script are likely to be dismissed from the cast. So are wives. Group psychotherapy, if successful, helps one abandon a bad script and improve his repertory.

This book is important not only because it has something to say, but also because group psychotherapy is probably the only form of intensive treatment that is economically practical for society's large-scale mental health efforts. Systematic approaches to group psychotherapy have been slow in developing. But Berne has devised a way of programming group efforts that makes a lot of sense.

A concluding note to the prospective reader: It would be a good idea to have an unabridged dictionary handy when reading this book. In spite of his ability to state the simple case, the author goes on a word binge every now and then. And when he does he comes up with some whoppers.



*It must be admitted that the aspects of behaviour which experimental psychologists have so far been able to subject to rigorous methods of study have frequently appeared trivial, especially when contrasted with the spectacular findings which psychoanalysts have claimed with methods which are less scientifically sound. The conclusion is tempting that what is true in psychology is not interesting, and what is interesting is not true!*

—A. H. ILIFFE





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# To Be or Not to Be Patient

Bernard M. Kramer. Foreword by Milton Greenblatt

*Day Hospital: A Study of Partial Hospitalization in Psychiatry.* New York: Grune & Stratton, 1962. Pp. vi + 103.

Rose Laub Coser

*Life in the Ward.* East Lansing, Mich.: Michigan State University Press, 1962. Pp. vii + 182. \$7.50.

Reviewed by JULIA S. BROWN

Dr. Kramer, author of the first volume, received his 1950 PhD degree from Harvard University and for several years worked as a social psychologist for the New York State Mental Health Commission, the Massachusetts Mental Health Center, and the Boston Evening Clinic. In 1960 he went to Tufts University School of Medicine where presently he holds the position of Assistant Professor of Preventive Medicine (Social Psychology). Dr. Coser, who wrote the second book, was educated as a sociologist at Columbia University, receiving her PhD there in 1957. Since 1959 she has been Associate Sociologist at the McLean Hospital in Massachusetts, and since 1961, Research Associate at the Harvard Medical School. The reviewer, Julia S. Brown, received her PhD in sociology from Yale University. She has held appointments at the University of Florida as Research Associate in the College of Medicine and Assistant Professor in the Department of Sociology. Currently, she is Assistant Professor (Medical Sociology) in the Department of Psychiatry of the University of Oregon Medical School and is engaged in research in social interaction in the psychiatric ward.

behavior profoundly affected by all manner of environmental factors. This viewpoint has provided much of the impetus behind the current increase in studies of the sociopsychological milieu of the sick individual. Some investigators, postulating the centrality of the therapist-patient relationship, have analyzed its nature and its effect on the course of the patient's illness and recovery. Others have tended to minimize the importance of this specific dyadic relationship and have explored other aspects of the social setting, particularly the organization and structure of the hospital community. Dr. Kramer and Dr. Coser may both be counted among this latter number.

Thus, Dr. Kramer in *Day Hospital* describes the structural form of an experimental facility introduced by the Massachusetts Mental Health Center to demonstrate the feasibility of a new conception concerning the treatment of the mentally ill. This concept has it that an emotionally disturbed individual may be adequately treated, even while continuing his residency at home, if he commutes to the hospital for limited daily periods to receive various forms of therapy.

Kramer's major aim in providing an overview of this pioneering service is to make possible some evaluation of its therapeutic effectiveness. His report is based on observations, on interviews with staff and patients, and on statis-

tical analyses of patient records. From these data, he provides descriptions of the physical plant of the hospital, its activities program, basic philosophical orientation, organizational structure, personnel and patient population. The latter group is characterized by means of distributions according to age, sex, marital status, education, occupation, diagnosis, length of stay, number of previous admissions, disposition at discharge, and form of treatment.

These distributions reveal that patients representing many diagnostic categories and many degrees of illness were indeed treated in the day hospital over a three-year period. They also reveal that long term cases, persons with psychopathic disturbances, and persons with strong suicidal tendencies were infrequently admitted. Furthermore, fewer men than women, fewer married than single individuals, fewer aged than young persons, and fewer lower- and upper- than middle-class members applied for admission.

Noting these differences, Kramer arrives at the somewhat unwarranted conclusion that the day hospital is well equipped to serve those categories of mentally ill persons that it in fact handled, but less well equipped to deal with the various types that were rarely treated. Indirectly, he implies that cases infrequently seen in the day hospital, whether because of administrative selectivity or failure to apply, can be treated more adequately by the conventional inpatient unit. Since no comparative statistics are presented, there is room to question the basic assumption that the frequency with which given patient categories occur in the case load of a service is a function of that service's ability to deal with them therapeutically.

There are some data to support the claim that the cases actually seen were adequately treated. It is pointed out that 62% of the patients were hospitalized 4 months or less and that 77% of the patients were discharged to the community. It is also stated, though here again comparative statistics are not given, that in these respects the day hospital does not differ significantly from the inpatient psychiatric ward.

In summary, the main conclusion of this study is that day care is not merely

THESE two books reflect a recent tendency to regard illness not simply as a biological or biochemical process, but as a complex form of human



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By MELVIN H. MARX, University of Missouri; and WILLIAM A. HILLIX, Navy Electronics Laboratory, San Diego. McGraw-Hill Series in Psychology. Available in June, 1963.

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## ☐ THE MACHINERY OF THE BRAIN

By DEAN E. WOOLDRIDGE, California Institute of Technology. Available in July, 1963.

This book is aimed at the large body of physical scientists and engineers interested in current research on the nervous system but lacking an adequate background in biology to read the technical works in the field. It is a nonmathematical and nontechnical account of the exciting and interesting work being done in the field of brain research. Where appropriate, analogies are drawn between the biological subject matter and related computer principles.

## ☐ A PROGRAMMED INTRODUCTION TO STATISTICAL CONCEPTS IN PSYCHOLOGY AND THE SOCIAL SCIENCES

By CELESTE McCULLOUGH and LOCHE VAN ATTA, both of Oberlin College. Available in September, 1963.

Initially developed as part of a Ford Foundation research project in teaching machines and programmed instruction at Oberlin College, this program has been thoroughly tested and developed through use with hundreds of students of psychology, sociology, education, political science, and economics at 12 colleges throughout the country as part of the McGraw-Hill field testing operation. The program requires an average of 20 hours and is intended for use as a supplement to instruction in those social science courses where it is important for students to gain an understanding of the elements of statistics. The emphasis is on the development of an understanding of statistical concepts with a minimum of statistical computation.

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an adjunct to a basic inpatient service, but is an adequate substitute for full time hospitalization for a wide range of patients. Kramer closes with the sanguine view that the day hospital "may well become the major primary psychiatric facility of the future." It is unfortunate that this conclusion is based, in Kramer's own words, on "rough clinical judgment" rather than on "careful biostatistical investigations utilizing the cohort method with adequate comparison groups and long term follow-up."

Without a controlled study, the important question of the relative effectiveness of day care and full-time care remains unanswered. Scientifically, therefore, the monograph is disappointing. On the other hand, Kramer's study may prove useful as a guide to administrators and community mental health workers searching for economical and effective ways of providing therapeutic care for large numbers of emotionally disturbed persons.

WHEREAS Dr. Kramer attempts to appraise the overall therapeutic value of a new kind of hospital, Dr. Rose Coser in *Life in the Ward* seeks to provide insight into the social and psychological effects of hospitalization on patients in a conventional ward. Although she is the sociologist, and Kramer the social psychologist, Coser's book has many more psychological implications than does *Day Hospital*, which fits more closely the stereotype of a sociological survey. In several additional respects, the two books differ markedly. Thus, Coser's primary concern is with general processes and patterns of behavior rather than with the unique characteristics of a specific hospital. Her orientation is theoretical and scientific, instead of historical and clinical as is Kramer's. Finally, her research was carried out on the medical and surgical wards of Mount Hermon, a general hospital, rather than in a psychiatric facility.

The data for her study consist of observations of patient and staff behavior, plus the responses obtained in interviews administered to 19 doctors and nurses and 51 male and female patients at the time of discharge. Using

these data, an attempt is made to delineate the process by which the newly admitted patient first adjusts to the hospital setting and later readjusts his behavior for life outside the hospital ward. In this analysis, Coser has adopted a functionalist approach. She examines both the formal and informal structure of the hospital and notes both the common and differentiating behavior patterns and values of people in the various staff categories. She places particular emphasis on the factors inducing or mitigating stress among interns, nurses, residents, and senior staff members, and indicates the effect on patient care and on the patient's role of the interaction among these groups.

From the research it becomes apparent that the patient's role is defined in quite different ways by the several staff groups. As a consequence, patients fulfill their roles in various ways. Two of these, termed "primary" and "instrumental" by Coser, are selected as major types of adjustment to the hospital environment. These two types are differentiated according to the patient's responses to certain questions, e.g., whether a "good" patient should be submissive or autonomous; whether the hospital and the doctor should offer the patient solace or competent technical care; whether the patient misses specific bodily comforts more than he does friends and activities; whether he anticipates discharge in order to secure creature comforts or to engage in social relationships and activities. In each instance, patients with a "primary" orientation tend to choose the former of the two alternatives, whereas patients with an "instrumental" orientation tend to choose the latter. Coser judges the primary orientation to be a better adjustment to actual hospital life, but the instrumental orientation more adaptive for the resumption of the non-sick role.

To account for these two types of adjustment, four variables are considered: age, number of previous hospitalizations, type of therapy (whether surgical or medical), and structural differences between the wards. Older individuals, patients with a history of previous admissions, and persons undergoing medical therapy tend to exhibit a primary orientation more fre-

admissions, and patients undergoing surgery. Patients assigned to the surgical ward tend to be more "instrumentally" oriented than patients in the medical ward. This tendency Coser explains on the grounds that the surgical nurse is more independent than the medical nurse, and that independent nurses make for independent patients. The difference in nurse role is in turn attributed to differences in the social structure of the two wards.

In view of the small number of patients interviewed, further research is needed to validate these findings. Were such research performed, it might be fruitful to direct attention to the general problem of ways in which differences in adjustment to the patient role frequently than younger persons, first affect the healing process. Thus, do patients with a "primary" orientation tend to recuperate more or less speedily, and suffer more or fewer relapses and complications than do patients with an instrumental orientation? Again, how does the patient's role vary as a function of whether the setting is a home, a private hospital room, nursing home, or day hospital of the sort described by Kramer? How do these variations relate in turn to the rate and steadiness of recovery from illness?

A few critical comments might be added. First, the author tends to group all nurses together, ignoring the divisive and integrating factors at work within the nursing hierarchy itself. Second, her analysis of the conflicting nurse-intern relationship is essentially a recapitulation of views previously expressed by such writers as Caudill and Stanton and Schwartz. Third, her treatment of "structural aspects" proves somewhat disappointing. For example, there is no obvious relevance of Henry's "pine-tree, oak-tree" typology to an explanation of the greater independence of the surgical as against the medical nurse. Alternative interpretations of the surgical nurse's initiative might be sought in the nurse-physician ratio, or in the fact that on the surgical ward the chief resident alone enjoys authority, while nurses and minor medical staff are relegated to the same plateau of prestige.

Finally, *Life in the Ward* lacks integration. Perhaps this comes about be-



cause Coser has already published several articles from the same source material. This volume, then, becomes more a collection of these essays than a single, tightly conceived piece of work.

These criticisms are minor. The book is very readable and stimulating. It should prove highly useful to health workers concerned with providing a supportive milieu without also producing "hospitalitis." It also belongs in the library of the behavioral scientist interested in bureaucratic organizations in general, or in the medical setting in particular. An excellent list of references pertinent to medical sociology is an added bonus.

## On Children, With Love

Glenn R. Hawkes and Damaris Pease

*Behavior and Development from 5 to 12.* New York: Harper, 1962. Pp. vii + 375.

Reviewed by PAULINE S. SEARS

Both authors received PhD's from Cornell University and both now work in the Department of Child Development at Iowa State University at Ames, where Hawkes is Head of the Department and Pease is Associate Professor. Pauline Sears, the reviewer, received an undergraduate degree from Stanford, and now has two PhD's from Yale, one a symbol of her academic achievement, the other a husband named Robert. She now is Associate Professor in the School of Education at Stanford and is deeply involved in research in child development, with particular present concern for cognitive development as it is affected by adult-child interaction.

THIS small and warm hearted book on development in the middle childhood years is guaranteed to leave one with a sympathetic appreciation of chil-

dren during these ages. The authors' purpose is to present the growth and development of children, during their school years, in their natural habitat. The readers will probably be parents and teachers in training.

The two authors, who are well known researchers and child developers from Iowa State College, have drawn from many types of sources, of wide variety, to illustrate the points made. The reference lists are modern and interesting; but they also include some material from the 19th century and refer to sources from anthropology, clinical case materials, and literature as well as the more usual research studies in child development. In fact, the name index is impressive in its breadth. Most of the outstanding names in child development literature are there, and a good many others which are not so standard. Among the latter are Jennifer Owsley (age 11), Booth Tarkington, and H. L. Mencken. One can read peaceably through this book, finding coherence in the point of view expressed, and an absence of alarming controversy. There is an implicit point of view that the child flowers best in a warm and permissive atmosphere; it is love that makes the world go round; and that "the adult who attempts to help a child enjoy freedom and accept responsibility is truly guiding" (page 286).

The book gives the impression of simplicity. The sentences are short, and large concepts may be handled in a short space. There is, however, the disadvantage that it hardly gives sufficient time or provocation for the reader to appreciate what the consequences of these concepts are. Facts are given with little evaluation and only rare description of the methodology by which the facts were obtained. Freud is taken care of in a couple of pages, and need theories disposed of in a paragraph. Brim's theory of social roles is briefly reviewed, and a series of summary statements regarding the growth of personality are arrived at. While the authors state that these are possibly oversimplified statements, the reviewer would question whether they are not at such an abstract level, and so little tied into the theories of personality development

which have been so briefly cited previously, that they will have little or no meaning to the reader except as a good source for multiple choice examination questions. For example: "Development is markedly affected by people." "Development is markedly affected by situations" (page 112).

TEACHERS and parents do not generally become active producers of research in the field of child development. Nevertheless, most college students are competent to appreciate some of the problems involved in the research leading to adequate understanding about child development. It seems unfair to deprive them of knowledge about the controversies which have existed in the field, and some of the historical developments which have had an impact on more recent thinking. In this book, the thinking is pretty much done for the student; little active cerebration is required of him. There is no appeal to the prospective teacher as one who can form hypotheses about behavior, or who can carry out innovative types of approaches to children. There seems little room for inquiry or discovery.

One wonders if teachers trained to the kind of approach represented by this book can show the flexibility in adaptation of teaching methods that the times demand. More likely, it seems, is that they will adopt passively the values expressed here as to qualities of desirable parents, and desirable school procedures, without any very effective analysis as to why they are adopting these values. Too many teachers find it beyond their powers to explain clearly to parents or pressure groups on the schools as to why they are doing the things they are. Criticism of their methods leads them to defensive reactions, angry or self-abnegating, rather than to open-minded evaluation of evidence. If they are accused of being soft-headed, they are only able to quote generalities from research studies, with little understanding of how the generalities were arrived at or where the evidence is sound and pervasive as compared to where it is skimpy.

Psychologists may wish to use this volume as a warm, friendly, non-threatening reading for parents and prospec-



tive teachers. These readers will get from the book a broad, and on the whole, sound knowledge of the body of literature applying to child development in the middle childhood years. They probably will not get much stimulus to question their own assumptions or to extend the boundaries of our knowledge in child development.

## Platitudes for Peace?

Quincy Wright, William M. Evan  
and Morton Deutsch (Eds.)

*Preventing World War III: Some Proposals.* New York: Simon & Schuster, 1962. Pp. 460. \$6.95.

Reviewed by LEONARD W. DOOB

The team of three editors includes a political scientist, Quincy Wright, a sociologist, William Evan, and a social psychologist, Morton Deutsch. Wright, now in the University of Virginia's Department of Foreign Affairs, has been studying war and peace before and since his 1942 *A Study of War*. Evan and Deutsch are both working presently at the Bell Telephone Laboratories and Deutsch, at least, works overtime, for he is one of CP's consultants. The reviewer, Leonard Doob, is the well known social psychologist who after receiving a Harvard PhD soon decamped to Yale, where he has been, mostly, since 1934. His books include *Public Opinion and Propaganda* (1948), *Social Psychology: An Analysis of Human Behavior* (1952), *Becoming More Civilized* (1960) and, most recently, *Communication in Africa* (CP, Oct. 1962, 7, 377f).

THIS BOOK is on the side of the angels, and sincerity oozes out of virtually every paragraph written by the 25 contributors and the three editors. A timid reviewer like me, consequently, hesitates to speak up and utter a harsh

word against it. Better, it would seem, to repress the conviction that any reasonably well informed social scientist has virtually nothing to learn from its pages which has not been more felicitously communicated in the press, from the pulpit, and even over television. Be objective, I say; don't tell them that Bertrand Russell is repeating what he has already said many times; that David Riesman is not as cute and as entertaining as he tries to be; or that Talcott Parsons uses big, big learned expressions such as "bipolarization of the world community," "*Cuius regio, eius religio*," and "the pluralistic structure of interests."

What, then, can I write? I dare not be matter-of-fact, for Boring's dictum has been blessed by the present Editor and passed to reviewers in CP: "Do not abstract the book." I have, nevertheless, just wasted two hours trying to produce an abstract in convenient, tabular form, not only because I enjoy deliberately disobeying these particular or any two authorities in a single swoop, but also because I consider it my Public Duty to give wider currency to the modes of avoiding catastrophe disclosed by the scholarly contributors. My procedure was simple: I have listed at the head of the columns the academic fields out of which the contributors come, and in the rows the principal ways of avoiding war which they advocate. That ingenious table, alas, must be abandoned, if only to spare this journal the expense of setting it in type: it has 15 columns to represent the fields and over 50 rows to accommodate the Big Ideas, most of which are too specific to be thrown into abstract, meaningful categories.

The labor on the table, though, has been fruitful. First, one proposal, that of unilateral action, emerges seven times. Osgood cogently but a trifle self-righteously argues for graduated reduction in tension; Fromm very emotionally and Fisher (Law) fleetingly for unconditional disarmament; Jerome D. Frank movingly for nonviolence; Daiches (Literature) unconvincingly for the renunciation of nuclear weapons; and Riesman satirically and Etzioni (Sociology) analogically for unilaterality in general. It is significant that alert, sensitive men from different disciplines in-

dependently focus upon a similar, unpopular solution.

Otherwise the table suggests little or no connection between an idea and a field of knowledge. Who, for example, would propose that the arms race might be stopped by having ordinary people report to an international inspectorate "any evidence of evasion of disarmament" that has been agreed to by their own government? A neurophysiologist, a lawyer, a political scientist, an economist, a philosopher, a psychologist? None of these, rather a Professor of Industrial Engineering (Seymour Melman). Or who would argue that "the basis of peaceful coexistence" is "improving the world," particularly through an efficient international weather service? A Soviet Professor of Applied Physics (G. I. Pokrovsky). The extolling of so many diverse plans strengthens the conviction that salvation is not directly obtainable through the natural and social sciences.

MORTON DEUTSCH, the one editor who is a psychologist, struggles publicly in a concluding chapter to summarize "a psychological basis for peace." He tells us that war is not inevitable, that misperceptions can lead to war, that these misperceptions have an explanation, and that conditions for mutual trust among nations must be promoted. But the generalizations which he squeezes out of psychology and this volume sound banal; for example, "our research indicates that mutual trust is most likely to occur when people are positively oriented to each other's welfare—when each has a stake in the other's doing well rather than poorly." But is that statement banal, or am I trying to demonstrate my own superiority with the epithet? The important point is not Deutsch vs. me, but a banal fact: it would be truly wondrous if any of the ideas in this volume, banal or not, were utilized to reduce the threat of nuclear war.

As psychologists or social scientists, we may not be able to give statesmen or ourselves an exact prescription to eliminate evil. Still any suggestion that looks as though it might work must be greeted with acclaim. For this careless



and irrational reason, therefore, the impressionistically derived, jargonistically pontificated Bright Thoughts and principles of this book dare not be glibly dismissed. I end a review at a point where I did not begin it: the symposium, the product not of science but of scientists in moving anguish, merits a deep, second glance.

## Russians Radiate

A. V. Lebedinskii and Z. N. Nakhilnitskaya

*Vliyanie Ioniziruyushchikh Islutsheonii na Nervnyuyu Sistemu (The Effects of Ionizing Radiations on the Nervous System)*. Moscow: Atomisdat, 1960. Pp. i + 187.

Reviewed by ERNEST FURCHTGOTT

At the time this book was written the senior author, A. V. Lebedinskii, was a corresponding member of the Academy of Medical Sciences of the U.S.S.R. At the Second United Nations International Conference on the Peaceful Uses of Atomic Energy in Geneva in 1958 it was he who presented the general summary survey on the biological effects of radiation. CP has no information on his collaborator here, Z. N. Nakhilnitskaya. Ernest Furchtgott, the reviewer, received his PhD in psychology at UCLA in 1950 and both before and since then has been teaching experimental and physiological psychology at the University of Tennessee. And, since 1948, longer than any other American psychologist, he has been studying the behavioral effects of ionizing radiations. His first review of the research in the area was published in *The Psychological Bulletin* in 1956, and a second review is now in press in the same journal.

THE RUSSIANS may claim, and with reason, to be the pioneers in studies of the effects of ionizing radiations on the nervous system and behavior. Already around the turn of the century

Tarchanov, E. C. London, Zhukovskii, and Goldberg, among others, reported on the neural effects of X rays and radium. This interest is probably attributable to the already dominant position of the neurologically oriented physiologists—Sechenov, Wedensky, Pavlov, and Bechterev. The neurological orientation in physiology is still present and the Soviets continue to be in the forefront, at least quantitatively, in studies of the effects of radiation on the nervous system and behavior. During the last two decades the Soviet researchers have probably published as many or more studies in this area than is represented by the combined output of all other workers. The present monograph, whose senior author was chief Soviet biological spokesman at the Geneva Conferences on Peaceful Uses of Atomic Energy, is a comprehensive review of Russian research. It also attempts to cover foreign studies, but here its coverage is far from complete. Many of the references to the latter are based on secondary sources.

The topics discussed include morphological and functional changes in the special senses ("analyzers," in Soviet terminology), and in the peripheral, central and autonomic nervous systems. The authors emphasize that the most significant findings in this area are based on the results obtained by conditioning and electrophysiological techniques.

According to most Soviet researchers the nervous system is extremely sensitive to ionizing radiations. The effects are supposedly direct, not vascular, manifesting themselves in changes in arousal and internal inhibition. The initial response involves the reticular system and the diencephalic and mesencephalic autonomic centers. Later cortical and other central structures are also affected. The magnitude and direction of the changes, excitation or inhibition, depends on the dose and the time after radiation. Many of the reported phenomena are phasic, i.e., initial excitation is followed by a state of depressed activity. The effects are treated in accordance with the general principles of Pavlovian conditioning theory. The authors do acknowledge that the mechanism of the changes has not been established as yet.

The experimental findings are presented with no acknowledgments of their shortcomings. No attempts are made to reconcile conflicting results, especially with reference to the large differences in minimal effective dose which have been reported by different investigators.

Many non-Soviet studies which did not produce data fitting the predominant hypothesis of high neural radiosensitivity are either not mentioned or the data are misinterpreted. For example, Arnold (1952) in testing retention of a maze reported that "the radiation technique and dosages of this experiment do not produce significant differences in maze learning and retention." Lebedinskii and Nakhilnitskaya cite this study as follows (p. 102): "For instance, it is a fact that after head irradiation of rats with doses of 300 and 700 (Arnold actually used 800) r there results an increase in the number of erroneous trials in running a maze and a change in the speed of running (W. J. Arnold, 1952)". Such omissions and errors, however, do not detract from the usefulness of the book as a record of the Soviet accomplishments in the area.

It might be appropriate to mention here the recent publication of a brief English account and critique of this kind of Soviet research. (Grashchenkov, 1962; Stahl, 1962).

(Prepared in the context of activities supported by National Science Foundation Grant G 19469.)

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# Light from Analytic Depths

Irving Bieber, Harvey J. Dain, Paul R. Dince, Marvin G. Drellich, Henry G. Grand, Ralph H. Gundlach, Malvina W. Kremer, Alfred H. Rifkin, Cornelia B. Wilbur and Toby B. Bieber

*Homosexuality: A Psychoanalytic Study.* New York: Basic Books, 1962. Pp. vii + 358. \$8.50.

Reviewed by ANTHONY DAVIDS

*All ten authors are located in New York and each is associated with either the New York Medical College, the Postgraduate Center for Psychotherapy or the New York University School of Medicine. All are physicians except Gundlach who is a clinical psychologist, and Toby Bieber, a social psychologist. The reviewer Anthony Davids, received his PhD in clinical psychology at Harvard University, then stayed on there for a while to work as research associate with H. A. Murray. In 1955 he moved to Brown University, where he now is Associate Professor of Psychology. Also he is Chief Psychologist at the Bradley Hospital, a residential treatment center for emotionally disturbed children. His chief present research interests lie in the areas of normal and abnormal personality in children and adults, and relations between personality and cognitive processes.*

THE AUTHORS of this book constitute an interdisciplinary team consisting of eight medical psychoanalysts, one clinical psychologist, and one social psychologist. These investigators devoted nine years to the study of one hundred-six homosexual males and one hundred comparison heterosexual males. They completed this extensive investigation with the remarkably small research budget of \$5,000.

These investigators gathered their data from objective questionnaires and submitted the findings to conventional

statistical analyses. More specifically, seventy-seven psychoanalysts filled out detailed questionnaires designed to reveal significant information about the life histories of their homosexual patients. The authors evaluated such phenomena as mother-child, father-child, and sibling relationships during childhood and adolescence; prehomosexual development, early sexual experiences, and formation of sexual attitudes; sexual fantasies and practices with females and with homosexual partners in childhood and adult life. Statistical analyses of the therapists' responses to the questionnaire items revealed numerous significant differences between the patients in the homosexual and heterosexual groups. The quantitative findings are enriched and expanded upon through use of case studies of individual homosexuals.

Despite the fact that there were ten co-authors, the presentation is well integrated and by no means a collection of independent chapters. The style is consistent, the book is clearly written and it makes for pleasant reading.

The first chapter contains an excellent, concise, presentation and discussion of relevant theoretical concepts and provides excellent sources for hypotheses to be tested in future investigations. As this chapter reveals, there are numerous and often conflicting theoretical notions about homosexuality. But there have been few empirical investigations designed to test and to validate the theoretical concepts.

In this regard, a few years ago, the present reviewer and his colleagues were surprised to find so few empirical studies of homosexuality when we searched the literature in connection with our study of homosexual male undergraduates (Davids, A., Joelson, M., & McArthur, C., *J. abnorm. Soc. Psychol.*, 1956, 53, 161-172). In fact, our search did not uncover a single study that had used both the Rorschach and the TAT in comparing groups of overt homosexuals with groups of emotionally disturbed and normal males. Hopefully, this book by Bieber et al. should provide the inspiration for, and lead to be followed in, the more concerted research effort that seems essential if this problem is to be understood and coped with by therapists and by society.

IN ADDITION to its potential contributions to scientific advances in this field, the present book is encouraging, from the clinical viewpoint, in its suggestion that homosexuality is reversible. Previous writers have led one to believe that treatment of homosexuals was destined to failure, that they could be diagnosed and studied, but that their behavior could not be modified. The present psychoanalysts, however, achieved notable success in treating a large portion of their sample. If, as the present book suggests, this deviant sexual behavior is not inherent, constitutionally or physically based, but is derived from learning and interpersonal experiences, then it seems that methods of prevention and methods of treatment may eventually be utilized to good advantage.

Although the present work represents a notable research accomplishment, some factors that limit the generality of the findings and interpretations should be pointed out. One important consideration is the fact that the subjects in this study were disturbed by, and were being treated psychiatrically for, their behavioral abnormality. In other respects, also, they represented a highly selected sample. The majority were well educated (37% had postgraduate education) and were financially comparatively well off (40% earning over \$10,000). Moreover, there was a relatively large proportion of Jewish sub-



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jects in both the homosexual group (41%) and the comparison group (67%). An ideal research design would include a normal comparison group as well as a disturbed comparison group.

Most important, however, seems to be the need for studies of psychodynamics in homosexuals who are not undergoing psychiatric treatment. Undoubtedly, the number of homosexuals receiving psychotherapy represents only a minute proportion of the adults who engage in this deviant behavior. It would, therefore, seem very worthwhile to conduct psychological studies of homosexuals who are successful in their work and who function competently in other areas of everyday social living.

In evaluating the significance of this book, it should not be overlooked that not too long ago homosexuality was viewed as a form of incurable "disease" that should be hidden from awareness. Just as emotional disturbance and mental illness have become more socially acceptable human difficulties and have become the focus of more research activity, as homosexuality comes to be viewed as less abhorrent and is shown to be amenable to therapeutic change, this problem area may also receive increasing research attention with consequent steps in the direction toward more effective social control.

## East-European Laboratory Manual

K. A. Ramul

*Demonstratsionnye Opyty po Psikhologii (Demonstration Experiments in Psychology).* Tartu: State University, 1958. Pp. 97.

Reviewed by EDWARD BAKIS

The author is professor of psychology at the University of Tartu, in Tartu, Estonia, U.S.S.R. Born in 1879 and

trained at the University of St. Petersburg (Leningrad), he founded in 1921 the Psychological Laboratory in the Estonian State University in Tartu and was able to restore it following the Second World War. Dr. Ramul has travelled widely and is linguistically at home in Russian, German, English and French, in addition to his native Estonian and the classical Latin and Greek. Edward Bakis, the reviewer spent twenty years at the Estonian State University, first as a student assistant, then as senior instructor in its Psychological Institute, and taught at the Baltic University in Exile. He is now head of the department of psychology at Sterling College, Sterling, Kansas.

This book is the result of over three decades of concern about simple ways and means of demonstrating to students basic psychological phenomena. A German version was published in 1961 under the title *Psychologische Demonstrationsversuche* (J. A. Barth, Leipzig, 87 pp.). Its predecessor entitled *Psychologische Schulversuche*, was issued by the same publishing house 25 years earlier. The stored copies were destroyed in the course of the Second World War.

The experiments conducted during lectures differ from genuine scientific experiments in that they are not intended to discover anything new. Their purpose is to show students what is already known. To achieve this, many laboratory experiments have to be radically redesigned. The requirements a demonstration-experiment must satisfy are, according to the author, as follows: the experiment should not present technical difficulties and should not take much time; the conditions and results should be very clear; the students should be able to participate either as subjects or as observers; the experiment should not require much training in introspection; and the observation should be suitably relevant to the topic presented in the lecture.

The beginning teachers are reminded that a clear and thorough explanation of each experiment is imperative. They also have to remember that the introduction of demonstrations necessitates a

very careful planning of the lecture: the content has to be presented in a shorter time. The reviewer would like to add that the author's own lectures and demonstration experiments were planned with great care and held the interest of his audiences.

As to the selection of experiments, besides conceptual and technical simplicity, their choice was directed by the undesignated goal of presenting the backbone of general experimental psychology. The author covered unconditioned and conditioned reflexes, sensation (visual, auditory; intensity of sensation), perception (space, time, motion), imagery and association of ideas, memory, acquisition of skill, attention, phantasy, thinking, emotions, and reaction times.

Of course, Western psychologists would limit the content of an introductory course in a different way and would include such topics as measurement of aptitudes and of intelligence, which are not an accepted part of Soviet psychology. Likewise absent, except for an arithmetical mean or two, are basic statistical concepts.

There are some interesting simple devices invented by the author, such as the gravitational chronoscope and there are demonstrations with practically no apparatus at all, such as measuring association time by means of a wrist-watch, with precision up to 1/5 or even 1/10 of a second.

The value of simple experiments described by Professor Ramul will be appreciated by many a Soviet teacher who has available neither expensive automated devices nor an experienced assistant who can handle the technical aspects of a demonstration. If the text were available in English, it is likely that some of the author's suggestions would be welcome by instructors of psychology who teach in small liberal-arts colleges in this wide country. Finally, teachers going to underdeveloped countries may find many uses for much of this material which is almost "culture-free."

(This review was prepared in the frame of activities supported by the National Science Foundation grant G19469, awarded to Dr. Josef Brozek).





# More Than a Taste of Touch in Russia

B. G. Anan'ev, L. M. Vekker, B. F. Lomov and A. V. Yarmolenko

*Osyazanie v Protsessakh Poznaniya i Truda (Touch in the Processes of Cognition and Work)*. Moscow: Publishing House of the Academy of Pedagogical Sciences RSFSR, 1959. Pp. 263.

Reviewed by HERBERT L. PICK, JR.

All four of the authors are to be found at the Leningrad State University. Anan'ev is Professor of Psychology and also Director of the Institute of Education, Academy of Education. He is one of the leading Soviet theoreticians in perception, and is the author of *Psikhologiya Chusvstvennogo Poznaniya* (Psychology of Sensory Cognition) (1960) and *Teoriya Oschuschenii* (Theory of Sensation) (1961). Vekker and Lomov are working in the recently established Laboratory of Industrial Psychology at the University. Dr. Yarmolenko is a Lecturer in Psychology there. The reviewer, Herbert Pick, received his PhD at Cornell University under the direction of R. D. Walk and immediately thereafter left for the Soviet Union to spend the academic year of 1959-60 studying at Moscow State University under the direction of Professor A. N. Leont'ev. Since then he stayed for a while at the University of Wisconsin, teaching visual perception and other subjects, but in September, 1962, he joined the staff of the Institute of Child Development at the University of Minnesota where he is now Assistant Professor of Child Psychology. His present interests include perception, developmental psychology and learning.

THE VOLUME, a culmination of more than a decade of work carried out in Anan'ev's laboratory, provides infor-

mation in the grossly neglected area of tactual perception and reflects several typical trends in Soviet psychology. The title includes two topics, cognition and work, which have been of great concern to Soviet psychologists. The cognitive problem is actually an epistemological one: how do we acquire knowledge of the real world and just what is the nature of this knowledge? The work problem implies the application of psychology. Knowledge for knowledge's sake is rejected by the Soviets on both theoretical and practical grounds.

The epistemological questions are handled in accordance with the current Soviet philosophical views. Their basis is Lenin's "copy" theory: we get into our mind an image, a real copy, of the real world. Our knowledge more or less exactly "reflects," like a mirror, the world as it exists. This is not a coded representation, and our knowledge of the real world is better than an operational definition. We obtain this image in no small extent by movements of our receptor organs—in this case hands—which reproduce with their movements the contours of objects which they explore.

The image of an object depends on the interaction of the receptor organ with the object. The mechanism by which the physiological effects become a psychological image is not clarified. The mind-body problem doesn't exist for Soviet experimental psychologists, not because they ignore the mind, but

because they consider the mind the highest organization of matter. The properties of the image are characterized. These include such qualities as unity and projection, the latter implying that the image is projected outside the organism. It is suggested that the property of projection depends upon the fact that a movement of the receptor is exactly compensated for by a reciprocal movement of the stimulus across the receptor surface.

Since the image arises out of such interaction between receptor and object a discussion of the nature of various types of interactions between material bodies is necessary. The classification is somewhat reminiscent of the classical primary and secondary qualities. Such qualities as form, size and duration can be directly impressed or copied by a receptor object or organ. Other qualities such as temperature, electrical state, etc. depend on the relation of the object in question to other objects on which it might act. A third group of qualities such as hardness, elasticity, stickiness depend on the internal structure of the object. Unfortunately there is no general discussion of how these properties are transduced by the tactual receptor.

A chapter is devoted to applications of the more basic ideas and methods to manual skill. Various experimental studies are reported in which touch is examined both as a regulator depending on feedback and as an executor process. Exemplifying the former studies are investigations of writing, typing, cutting, or carving in which tactual feedback is masked to varying degrees. The precision of operation in general decreases with degree of masking. Exemplifying the latter studies are motion analyses of the two hands performing work operations. In some operations one hand holds an object and the other moves. Other operations involve simultaneous movements of both hands. This synchronization is said to be regulated largely by tactual-kinesthetic feedback. (It is pointed out that results of time-motion analysis in capitalistic countries are used to exploit the workers, whereas in USSR they should be used to increase productivity for the good of society.)



The largest section of the book is concerned with various forms of touch. Chapter 3 concerns passive touch and is in the classical tradition. Chapters 4 and 5 concern active touch and involve tactual-kinesthetic sensitivity. Here the general concern of Soviet psychologists for response processes is manifest. Systematic analyses of hand movements involved in tactual exploration are described. A typical experiment involves giving a blindfolded subject an object to be explored tactually. As he explores it, moving pictures are taken. These are analyzed by various methods with particular attention being paid to the interrelations of movements of the fingers and the two hands. The subject is asked to draw a picture of the object at various stages in his exploration. These are analyzed for adequacy.

The main concern of the motion analysis is to define stages in the exploring process. So in bimanual exploration there is an orientation phase, a first groping phase, and a second groping phase. The orienting phase consists of movements of both hands in the air or on a table along the sagittal axis of the body until the object is reached. The hands slide over the surface of the object to its upper edge, the movement being regulated by the middle fingers. At the far point of the object both hands pause for 0.2 to 1.5 seconds. Thus the location of the object relative to the body is fixed. The next movements are directed towards analysis of the form of the object. This first groping stage involves circumventing the object with the two hands and obtaining a general idea of its form. Reproduction at this stage reflects the general form but details of the object and relations between them are not accurate. These improve with subsequent exploration the function of which is synthesis.

The above description, fairly characteristic of the text, seems too stylized; one wonders if this is not an ideal picture of the process rather than a typical picture. The comprehensiveness of the studies reported is manifest in the fact that investigations have been made of mono- and bi-manual touch, touch with exclusion of one or more fingers, and reproductions have been obtained after periods of delay up to a week.

Other interesting sections of the book include a chapter on touch in the blind, the deaf, a section on touch by means of artificial limbs, and a consideration

of the development of general perception in which touch is given a dominant role.

Weaknesses appearing in the book again are characteristic of Soviet psychology in general—relatively poor experimental design, lack of statistical treatment of data, and too much political and/or philosophical rationalization of, and perhaps influence on, their work. (The Soviet psychologists consider this latter a virtue.)

The interesting methodological approaches in the results obtained and their interpretations make the book a valuable contribution to the literature.

*(This review was prepared in the frame of activities supported by the National Science Foundation grant G14969, awarded to Dr. Josef Brozdek.)*

## Guidance for All

Robert Hendry Mathewson

*Guidance Policy and Practice. Third edition.* New York: Harper & Row, 1962. Pp. vii + 397. \$6.00.

Reviewed by GAIL FREDERICK FARWELL

*The author, Robert H. Mathewson, Professor of Education at the City University of New York, is well known for his many publications, over the past thirty years, in the area of guidance and counseling. The reviewer, Gail Frederic Farwell, has also had a long, but not as long, career in guidance and counseling, a career that has included counseling in the school setting as well as professing in the university setting. Since 1959 he has been at the University of Wisconsin where he is Professor of Education. His most recent book, An Introduction to Teaching, will be published by Macmillan in 1963. He is currently serving as an officer of the American Personnel and Guidance Association.*

As in the two previous volumes of *Guidance Policy and Practice*, Professor Mathewson dares to be different.

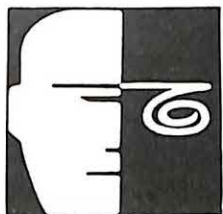
There are too few who have developed sound policy toward guidance work in educational institutions. In this volume, Mathewson has lucidly presented a viewpoint toward developmental guidance policy that offers much hope. The guidance program and personnel are not catch-alls for administrative hand-downs.

Guidance may very well be the continuous thread that guarantees *each individual* consideration throughout his school years. The focus of guidance lies in the individual-in-situation. Counselees do not live or modify their behavior in a vacuum. The model for Mathewson's approach to guidance is stated as follows, "The self-situational field of the individual may be visualized as an infinitely complex internal-external pattern of dynamic events, all interconnected and interacting in constant flux along a time dimension." From this framework, a courageous policy toward guidance is presented punctuated with a practical implementation. The author has not backed down as problems are confronted. For example, administrators, teachers and parents will need to review their own concepts toward education if this guidance policy is to become reality. A visionary program is proposed, modified by suggestions for intermediate steps that would provide transition from what is to what might be.

Professor Mathewson's broad range of experience in schools and colleges and his years of involvement in Counselor Education in the city colleges of New York are reflected in his development of guidance policy. The guidance policy proposed gives due consideration to the social influences of the home and community, the psychological forces within the individual, and the unique role of the school as it influences individual growth and decision making.

Mathewson aims in his approach to minimize the predictive function of the school counselor. Instead, the emphasis is given to his developmental, informative, and interpretive functions. There is a tracing of the historical antecedents of guidance, but to this reviewer the exciting aspect of Mathewson's discourse lies in his perceptive and courageous policy for the future. Guidance policy is based on the growth and positive





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features of human life rather than on the view that the guidance process is correctional. A developmental guidance policy causes a different view on the part of professional and lay people. No longer can guidance be concerned solely with getting young people into jobs or college. Self growth is crucially important in developmental guidance. This program is a school wide proposition and is not confined to secondary schools

as is frequently the case in current practice. The need for professional preparation in guidance is fully recognized.

This is not a "how to do it" book. Rather it offers many avenues for provocative thinking. The conservative and liberal alike in education will find much stimulation from *Guidance Policy and Practice*. All persons reading this volume should experience a cerebral itch.

by the 1950 address delivered by J. P. Guilford as retiring president of the American Psychological Association. To those who did not get the point, Guilford's detailed listing of a great variety of possible tests to explore the dimensions of intellect seemed tedious, and they failed to perceive the implicit criticism in his research strategy of the over-estimation of verbal comprehension and of convergent thinking which the educational system itself, with the help of the achievement test-makers, had fostered. Many still do not get the point of Guilford's monumental effort nor see its relationship to Wiggstein's turning away from his early exclusive concern with denotative meaning and strict logical inference to the study of natural language games, including non-linguistic behavior. The point is simply this, that the potentialities of the cortex and the expressions of intellect in behavior are much greater and more various than psychology and education had supposed.

## On Making the Familiar Strange

William J. J. Gordon

*Synecotics: The Development of Creative Capacity*. New York: Harper, 1961. Pp. ix + 178. \$4.50.

Reviewed by FRANK BARRON

The author, William J. J. Gordon, is President of the Invention Research Group, Cambridge, Massachusetts and is a Lecturer at Harvard University. He has contributed to such periodicals as the Harvard Business Review, the Philosophical Review and The New Yorker. The reviewer, Frank Barron, is a product of La Salle College and of the Universities of Minnesota and California (Berkeley, PhD 1950) and is now a research psychologist at the latter's Institute of Personality Assessment and Research where, among other things, he is creating many declarative sentences about creativity. Recent publications include articles on creativity for the Encyclopedia Britannica and for the Encyclopedia of Mental Health. Now in the works and soon to be published by Wiley is the book *Scientific Creativity* which Barron has co-edited with Calvin W. Taylor. Also coming, with the help of Van Nostrand, is Barron's own book *Creativity and Psychological Health*. In addition to all this, he is presently engaged in a study of the inheritability of originality and its relationships to schizotypic thinking and to ego-strength.

MEN of sensibility who were far enough away not to be terrified or destroyed by it were awestruck as they received the news of the explosion of the atomic bomb over Hiroshima. Intellect had intervened in natural process to a degree unimagined only a short time ago. The power of mind to create and to destroy was of a sudden shockingly apparent. That horrendous instant of mass destruction had both debased and magnified the human nature which most "civilized" men had thought they shared.

The reaction on a mass scale to this realization of destructive capacity and of creative potential has run the gamut from despair to immense hopefulness. The "creativity movement" which took hold in psychology at mid-century, and which was for a time dismissed by some as a fad which would soon pass, is now seen properly as part of a vast movement of mind, engaging the interest and becoming a central part of the goals of ordinary men and women everywhere.

In professional psychology itself, a disciplined research attack on the problem of psychic creation was signalled

BOTH Guilford and Wiggstein are models of abstraction, but their message is an earthy one, and William J. J. Gordon in this book shows both that he has got the point and that he can make it to others in a way that gets people closer to animals and to the earth as well as to poetic metaphor and the stars, all in the interest of invention and with a dollars and cents payoff never far out of mind. Gordon seems to have developed his technique of "synectics," which is a practical method of facilitating invention and enhancing creative thinking in groups, quite independently of professional psychology, although it may be of more than minor importance that Jerome Bruner worked as a consultant with the "synecticists" for a year and is credited by Gordon with helping to translate the group's observations of stages of inventive thinking into "operational mechanisms."

Gordon's approach throughout is based upon a thorough-going but highly sophisticated mechanistic viewpoint, marked by an unsentimental pragmatism which is nevertheless infused with an almost poetic feeling for biological analogy in the invention of new ma-



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chines. The word "synectics" means the joining together of different and apparently irrelevant elements into a meaningful whole; as applied by Gordon, synectics theory aims at a functionally efficient integration of five or six individuals into a group which will state and solve problems in an original fashion. The emphasis is upon the importance of the emotional and the irrational, the use of the commonplace as a point of departure for "making the familiar strange and the strange familiar," the achievement of complexity and elegance through perceptual openness, the deliberate cultivation of natural symbolism and personal fantasy, and the use of empathic identification with the inhuman.

In all these emphases, Gordon is very much in tune with the new times in psychology and in the world at large, and his linking of the practical with the poetic and the scientific is refreshing.

## Optimistic Horses and Lovable Animals

R. H. Smythe

*Animal Psychology: A Book of Comparative Psychology which Discusses the Behavior of Animals and Man.* Springfield, Ill.: Charles C. Thomas, 1962. Pp. v + 259. \$7.50.

Reviewed by KELLER BRELAND

The author is a British veterinarian who has written prolifically about a wide variety of animals. The reviewer, Keller Breland, is a psychologist who reports himself to have "contracted a bad case of dustbowl behaviorism (*Operantitis moribunda*)" while "behind the Ivy curtain," and that he has been led, over the past fifteen years and in connection with his *Animal Behavior Enterprises* to condition over 7000 animals of more than 40 different species. Clearly the founder of applied animal

psychology, he may soon be sending messages back through the Ivy curtain, for he now is involved in reworking a basic conceptual framework for animal learning.

THE TROUBLE with this book is that it is mislabeled. The subtitle claims it to be *A Book of Comparative Psychology*, but the claim is by no means justified by current American standards. Smythe is a British veterinarian who believes in animal ghosts (p. 151) and human water witching (p. 148). He also takes a dim view of Lloyd Morgan's canon (p. 184).

This book seems more in keeping with the tradition of the British nature lover than with tough-minded science, and would have been more at home under some such title as "Our Animal Friends and Their Wondrous Ways," or "Observations and Anecdotes of an Animal Lover." If marketed in this category, a reviewer could doubtless find many pleasant things to say about it, and it would escape the harsh scientific criticism which we must now apply to it.

The dust jacket blurb asserts that it is a "scientific study that reads like a novel." However, it certainly does not qualify as scientific psychology. Actually it is a digressive discussion of disorganized tidbits of animal knowledge gleaned, without credit, from the literature, interspersed with hearsay anecdotes, casual observations, and preposterous categorical judgments which invite the reader to believe a number of disquieting propositions.

Laboratory experimenters will be disappointed to realize (p. 31) that "Animals learn far more of our moods and intentions from observation of our expressions than we are able to learn from a study of the animal. They far exceed man in sensual perceptivity and seem to know exactly what is going on in the human mind." If, however, the experimenter is a real 'work-horse,' he may not be overly saddened because (p. 33) "Horses are usually optimistic in outlook. The horse which hangs its head habitually and drags its toes belongs to the ranks of the pessimists."

Neurologists, feminists, and students

of individual differences alike will be excited to learn (p. 48) that "In women the brain weighs slightly less than it does in man, but this does not imply that the female is lacking in intelligence since her grey matter is definitely superior in quality," and that "Among the lower orders of animals the females are less handicapped than women are by their emotions and they are almost invariably more intelligent than the males."

Reinforcement theorists can enhance their confusion by accepting the statements (p. 77) that "A great many young animals of various kinds when reared in domesticity learn to perform tricks, or special types of behavior, in return for rewards of food. Dogs are particularly quick at associating the trick or deed successfully performed with the reward, but horses hardly ever do so. They will accept the reward eagerly enough but it does not encourage them to repeat the successful performance. A dog will usually 'do it again,' so long as the supply of tidbits holds out, but in all cases he will insist on receiving his reward for his last trick before he responds to the encore."

Also Pavlovian experimenters will be dismayed to learn (p. 79) that "(dogs) respond to kindness and tidbits, but will go on strike if punished, and refuse to take further part in the proceedings."

Paleontologists will have to dig a little deeper, too, for we read (p. 108) "The short-headed and short-muzzled dogs which appeared, probably *millions*" (underlining by reviewer) "of years ago, as a 'sport' from the prevailing long-headed dogs, had underdeveloped jaws, often also undershot, and in addition they had short legs. They would have been quite unable to kill their game even if they had been able to catch it . . . and they probably hung around human settlements . . ."

PLAINLY a book of this ilk published under the title *Animal Psychology* does a grave disservice to the scientific field of comparative psychology, and one can only wonder what sort of editorial standards permit such mislabeling and the publication under the umbrella of science of such patently unscientific ma-





## *Educational Psychology in the U.S.S.R.*

**Edited and with an Introduction by Brian and Joan Simon**

Prepared with the active cooperation of the Institute of Psychology of the Academy of Educational Sciences in Moscow, this volume provides a unique survey of recent Soviet work on the psychology of learning. All of the papers are by leading Soviet psychologists. The material has been selected with an eye to the interests of teachers, administrators, and psychologists of the English-speaking world. \$6.75

## *Studies in Mathematical Psychology*

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Fifteen papers present the most recent developments in the analysis of mathematical models and their application to psychological data. Emphasis has been placed upon theoretical and theoretically oriented experimental studies. The extensive selection of material in this book makes it essential for research oriented psychologists, mathematicians, statisticians, and behavioral scientists interested in the application of mathematics to psychological phenomena. September. About \$12.50

## *Social Pressures in Informal Groups*

**Leon Festinger, Stanley Schachter, and Kurt Back**

This book, first published in 1950, has become generally recognized as one of the important pioneering works in the field of social psychology. "For theory of group structure and functioning, a number of very suggestive points emerge. . . . The study shows an impressive use of a battery of techniques in a carefully planned and systematic way."—*Human Relations* \$5.00

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terial. The reviewer was further puzzled as to what type of audience the book might justifiably be recommended. Certainly not to the youthful novice, because of the egregious lack of rigor and the quantity of downright misinformation. Clearly not to the sophisticated animal psychologist, who already knows the literature first hand.

Two possibilities suggest themselves for consideration: First, Smythe's *Animal Psychology* (along with recent lily-gilding treatises on the transcending intellect of the porpoise) could be profitably assigned to undergraduates in animal psychology courses as an elementary exercise in critical evaluation and an object lesson in how *not* to observe and report animal behavior. Secondly, psychologists who are discouraged by the apparent lack of progress in the field during the last half century may have their morale lifted by comparing this Romanesque book with any other American animal psychology text.

## Access to Abnormality

Theodore R. Sarbin (Ed.)

*Studies in Behavior Pathology: The Experimental Approach to the Psychology of the Abnormal.* New York: Holt, Rinehart & Winston, 1961. Pp. v + 341. \$4.00.

Reviewed by JOHN KANGAS

Theodore Sarbin, editor of this book, is a clinical psychologist who received his PhD from Ohio State University in 1941 and, since 1949, has been at the University of California at Berkeley where he is now a professor. Among his more recent publications is (with Taft and Bailey) *Clinical Inference and Cognitive Theory* (1960, CP, Nov. 1961, 6, 389f). The reviewer, John Kangas, took his PhD at the University of Minnesota and stayed on in Minneapolis for a couple of years as Instructor in the Division of Clinical Psychology at the Medical School. In 1960 he

went to the University of Oregon Medical School, where he now is Assistant Professor. Since he moved to Oregon, he reports, he has been committing most of his time to a "megabuck multidisciplinary anterospective study of brain damage in children and related clinical and teaching assignments." He remains a clinical psychologist and, as such resonates happily to the role of the general clinician in a medical school setting. He also enjoys evidence that applied psychology truly amounts to something.

THIS PAPERBACK collection of 39 recently-published articles on behavior pathology is being offered to the psychologist-pedagogue who is unhappy with the lack of space devoted to systematic empirical investigations in abnormal psychology texts, and who further believes that reading original journal articles will infuse his students with an appreciation of the vigor and profit of applying the tools of science to aberrant human behavior. As a text supplement, it is intended as an answer to the logistic problems involved in giving a class exposure to the same set of journal articles.

The choice of articles seems reasonably catholic within the limits the editor has set for himself. American journals, both psychological and psychiatric, are well sampled; foreign journals are not. The hypotheses tested originate in diverse sources—psychoanalytic dream theory, theories of learning, the icy purity of seeing what-goes-with-what. Articles devoted to evaluation of treatment techniques have largely been excluded in favor of material dealing more directly with the character of behavior pathology. To the editor's credit, the selected articles generally study abnormal subjects in exploring abnormality; but unfortunately, like the texts it is to supplement, this collection gives short shrift to the pathology of the young, the old, and the mentally retarded. However, something for everyone will surely be found in the breadth of variables examined; they range from serum pepsinogen levels to the influence of female relatives on successful hospital discharge. The student likewise will be

exposed to a broad spectrum of methods of data analysis.

Any one may of course quibble about the choice of articles; they undoubtedly are not your favorite 39, nor mine. But you must pose to yourself the problem of access to your selection.

A reader may also wish to quarrel with the almost complete exclusion of nonempirical material aimed at cutting through, or at least focusing upon, some of the logical and semantic confusions which plague psychopathology. But an equally strong argument can be made for Sarbin's implicit thesis that the demonstration of a new avenue of approach, rather than a shredding of the old, has considerable pedagogical vitality. A case in point is the Wittenborn article, which subjects the diagnostic entity of involuntional melancholia to factor-analytic scrutiny.

A MORE serious pedagogical problem lies in the nature of the research article itself. Writers of journal articles for the scientific literature do not address themselves to an undergraduate audience. Under pressure from space-conscious journal editors, the context of their research efforts is often translated into little more than a set of journal references. Pre-shrunk reports on methodology become understandable only to the initiate, and this doesn't necessarily mean all psychologists. These characteristics may be hallmarks of maturation in our science, but they don't facilitate comprehension by the undergraduate. The editor, cognizant of these difficulties, has attempted symptomatic relief by including only articles which pass his clinical tests of comprehensibility, by making deletions where possible, by adding brief introductory paragraphs to groups of articles, and by providing a glossary. That these efforts represent only a partial cure is obvious to the editor, since he comments in his students' preface that the student should skip the details and try to arrive at a general understanding of what was done.

Although it is unfair to single out Sarbin's useful collection as an example, there is a touch of irony in producing and reviewing such a teaching aid, championing as it does experimental



rigor in psychopathology, when its own case for pedagogical effectiveness rests upon armchair wisdom and clinical-teaching experience. The same stout scientific tools urged upon clinical psychologists and psychiatrists in dealing with behavior pathology could equally well be utilized in production and evaluation of textbooks and their supplements. What changes in understanding and attitudes can be documented to have occurred as a consequence of reading this book? What kinds of students were so affected? What in fact is the level of comprehensibility of these articles for undergraduates? Is it unreasonable to wish textbooks were accompanied by a teacher's manual setting forth such relevant research data on that text, and to wish that the reviewer might then write his critique with his *APA Handbook of Technical Recommendations for Psychology Texts* as his guide, much as he might now do with tests he evaluates? The arguments for the one are surely as cogent as for the other.

## Clinicians in Russia

Bluma V. Zeigarnik

*Narusheniya Myshleniya u Psikhicheski Bol'nykh (Disturbances of Thought in Mentally Ill Individuals)*. Moscow: The State Research Institute of Psychiatry, 1958. Pp. 593.

Reviewed by JOSEF SVANCARA

The author, Bluma V. Zeigarnik, is a Soviet psychologist, known in this country for the "Zeigarnik effect" which she discovered and wrote about in Germany when she was a student of Kurt Lewin. The present book was translated into German by Kurt Gottschaldt and J. Helm. The reviewer, Josef Svancara, is a clinical psychologist in Brno, Czechoslovakia. He was born in 1924 and received his PhD degree at the University in Brno. He has participated in a

study on neurotic illness in children, has published papers on emotional conflicts and on the work of psychologists in neurology and psychiatry, but his chief interest is neuropsychology. He emphasizes the use of experimental methods in clinical psychology.

PSYCHOLOGICAL laboratories in hospitals for nervous and mental disorders have been in existence in Russia since 1880. In particular, Korsakov and Bekhterev supported the development of experimental psychology in psychiatric clinics. Soviet psychologists continue to be concerned with mental diseases.

The material which forms the basis of the present book was obtained from 425 patients with a variety of neuropsychiatric diagnoses. The psychological laboratory findings were to serve both practical clinical and scientific purposes.

In the chapter dealing with psychological diagnostic methods the author rejects the Western test methods, referring to the well-known 1936 resolution on "perverse practices of the pedologues." She states the objection that mental tests do not adequately portray the qualitative side of mental disorders nor do they take cognizance of the dependence of mental processes on conditions of life. She also rejects the projective methods since they endeavor to reach into subconsciousness and are based on the Freudian or Neofreudian theories.

She bases her own approach on three fundamental principles: (1) examination of the disordered mental functions; (2) qualitative analysis of the patient's responses, and (3) objectivity of the criteria.

Psychological laboratory findings can serve as aid for diagnosis. The choice of experimental psychological methods depends not only on the patient's general mental status, the degree of his accessibility, but also on the level of his general education. These considerations are relevant both for the quantitative and, in particular, for the qualitative analysis.

The author's own procedures are based on four varieties of the word association experiment. By these techniques she investigates the degree of

generalization and purposefulness of associations, interpreted according to I. P. Pavlov's doctrine as "temporary connections." She examines the patient's ability to recall words, allowing him either to make some drawings (A. R. Luria's pictographic method) or to select a picture from the available supply (A. I. Leont'ev's version of the study of memory). Additional methods include object sorting (according to L. S. Vygotskii), picture arrangement, interpretation of proverbs, and elimination of one of the four objects not logically connected with the others.

The author distinguishes three aspects or classes of thought processes: generalization and abstraction, logical thinking, and purposeful and critical thinking. Disorders of these functions are analyzed in reference to selected groups of patients. For example, the impairment of the processes of generalization were studied in 25 patients with cerebral palsy, 35 patients with epilepsy, and 15 patients with severe cerebral lesions. The lowered level of generalization is explained in terms of decreased availability of "temporary connections," reflecting a disturbed balance of the processes of excitation and inhibition due, in turn, to weakened inner inhibition.

The author reflects upon the relationship between deterioration and development. She regards as incorrect the opinion that the patient's mental processes regress to an earlier stage of development. This appears to be especially true in reference to thinking.

METHODOLOGY is the key problem in Soviet clinical psychology. On ideological grounds "test" methods cannot be used. New ways must be found. American psychologists are acquainted with this point of view from A. R. Luria's recent paper (*Amer. J. Orthopsychiat.* 1961, 31, 1-16). But do Dr. Zeigarnik's experimental psychological methods essentially differ from psychodiagnostic tests? Some of them are, in fact, parts of existing tests.

A compromise solution may involve taking over methods of proven validity and working out a new interpretation which does not contradict the ideologi-



cal base. In such a framework it may be possible to use such tools and approaches as Wechsler's Intelligence Scale or Rorschach's perceptanalysis. Twenty-five years ago the Rumanian author Marinesco made an attempt at a Pavlovian interpretation of Rorschach. The possibilities of reevaluating the test in accordance with Pavlovian teaching were considered also by the Bulgarian author N. S. Shipkovenskii (*Zhur. Nevropatol. i Psikhiatrii*, 1958, 58, 1326-1331).

Zeigarnik's work is typical of the best in Soviet clinical psychology. It is characterized by a thoughtful approach to the problem and the concern for prac-

tical usefulness of the investigations; focal significances attached to the study of single cases in real-life situations; and there is a minimum use of statistics. Professionally, the present work is important in that it documents the feasibility and potential fruitfulness of the psychologist's cooperation with psychiatrists and neurologists. It should be recalled that even good recent psychiatric textbooks (cf. one by G. J. Sukhareva, Moscow, 1959) consider psychology to be essentially a speculative discipline.

*(Prepared in the framework of activities supported by the National Science Foundation grant G19469, awarded to Dr. Josef Brozcek.)*

## Secondary Prevention in Schools

Wesley Allinsmith and George W. Goethals. Including a field study by W. Cody Wilson and George W. Goethals

*The Role of Schools in Mental Health.* (Joint Commission on Mental Illness and Health, Monograph Series No. 7) New York: Basic Books, 1962. Pp. v + 337. \$7.50.

*Reviewed by* PAUL E. EISERER

Both authors, Wesley Allinsmith and George Goethals, were associated both with the Joint Commission on Mental Illness and Health and with Harvard University at the time the present book was written. At the latter institution, Wesley Allinsmith was connected with the Laboratory of Human Development in the Graduate School of Education, Goethals with the Department of Social Relations. Allinsmith since has moved to the University of Cincinnati where he is now Professor and Head of the Department of Psychology. Goethals has remained at Harvard where he continues as a Lecturer in Social Relations. The reviewer, Paul Eiserer, took his PhD in Chicago and spent a few years at the University of Oregon before moving back across the country to Columbia University's Teachers College,

where he is now a Professor in the Department of Psychological Foundations and Services. Primarily a clinical psychologist, he is nevertheless vitally interested in school psychology and for more than a decade has taught mental hygiene to teachers.

IT was to be expected that the Joint Commission on Mental Illness and Health would devote a major effort to a consideration of the role of the schools in mental health. The weight of evidence in the behavioral sciences has long pointed to the significance of the school years in correcting personality imbalances resulting from pre-school miseducation; also there is reason to regard the school as the social institution most amenable to public

manipulation in fostering healthy personality development. In Volume 3 of the series coming from the Joint Commission Studies, Albee makes a trenchant analysis of the crisis in education (chapter 9) and reports a "depressing educational picture." In Volume 5 of the series Robinson et al reaffirm our faith—"Next to the family, the school is probably the most important unit of society as far as the protection of mental health is concerned" (p. 154). Robinson's data, based on a sample survey of 15 counties and concerning the availability and use of special service personnel, offer no comfort to those who take this view seriously.

These earlier discussions have whetted our appetite for a tour de force with respect to the schools and mental illness and health. The present volume is divided into two parts: a commentary on issues and practices and a field study of "sources of potential tension in the public educational system." Since the summary volume, *Action for Mental Health*, appeared a year in advance of the above book and contains an excellent summary of Part I, only Part II is new.) The authors do not appear empty-handed before their hoped-for public, which they see as "professionals in the fields of education and of mental health, and citizens without special training who are interested in our topic." They bring results from an examination of 5,000 books and articles and from 2,000 completed abstracts. The breadth of coverage is suggested by a reference list of about 225 titles. They disclaim encyclopedic aims but accept the task of interpreting the contemporary picture of thought and aspiration about mental health and the schools, from nursery school through the university. The major focus, however, is on the elementary and secondary public schools.

Because mental health has become a secular symbol for salvation, no individual or social condition can escape its embrace. In an incisive summary of five current theories of the curriculum, the authors note that the proponents tend to justify their positions as the best way to achieve optimum human development; the definition of positive mental health is so encompass-



ing that there is room under the umbrella for everyone. For their part the authors are eclectic and pluralistic; they take what is good in each of the contending philosophies, assert that they are logically but not easily reconcilable.

THE AUTHORS accept the view that the schools have a strategic role in primary and secondary prevention of mental illness. They recognize that many children require therapeutic assistance from mental health professionals outside the schools. The major concentration of their effort is in secondary prevention and in the crucial role to be played by teachers. They contend that much more can be done than is being done, and without radical change in curriculum or school organization, perhaps without a major diversion of the teacher's energy from instructional obligations.

The strategy calls for better in-service and pre-service education of teachers since there never will be enough special services personnel to do the job. Strong support is urged, however, for substantial increases in special services. Seven possible levels of intervention in pupil illness are examined: detection, diagnosis and prognosis, first aid, referral, treatment, rehabilitation, and follow-up. This is surely a comprehensive program and one not likely to be found in its fullest potential in any school system today. The relative roles of teachers and specialists are discussed, arguing that flexibility and functional competence are more appropriate criteria than a priori role designation. Psychotherapy by teachers is proscribed but the reader is cautioned that psychotherapy is not to be construed as synonymous with treatment in which teachers may have a considerable part to play.

PART I, to which about two thirds of the book is devoted, essentially summarizes existing thought and aspiration about schools and mental health. It does not challenge contemporary doctrines nor suggest radical innovations. It assumes that if present ideas were more effectively implemented

great strides would be made in advancing the mental health of children and youth.

Part II, a field study, was designed to test the hypothesis "that a situation in which there is a discrepancy or lack of congruence between an individual's values and the prevailing practices has negative affect for that individual" (p. 182). Data are collected from teachers, high school seniors, and college seniors via questionnaires and procedure so arranged as to permit comparisons between personal values and perception of existing practices. Discrepancies necessarily have negative affect in the schools, stresses having mental health implications.

The hypothesis is sustained in general at a significant statistical level. But why should one assume that discrepancies necessarily have negative affect? Might not discrepancies between values and perceptions of practice motivate toward constructive change? Since the sample of teachers is drawn from excellent rather than poor school systems, might not the same teachers have unrecognized resources for constructive handling of conflicting and discrepant perceptions?

What an educational system we would have if the values of one million teachers were perfectly congruent with perceptions of existing practices!

The authors limited their inquiry entirely to comparisons of verbal responses to questionnaires without poking around the school systems to find out how the teachers were really behaving. As a result many of the authors' speculations about the mental health implications of the data are highly gratuitous.

The inclusive title of the book will disappoint some readers, for it promises a wider scope than it delivers. Yet the authors have made a strong case for buttressing the role of the teacher in the secondary prevention of mental illness. Perhaps a major value of the book is that the Joint Commission goes on record as being sympathetic toward comprehensive goals for an educational system which in the eyes of some critics is already too responsive to the pluralistic aims of a heterogeneous society.

## Duel of Duality

Étienne de Greeff

*L'Homme et son Juge.* (Textes et Études Anthropologiques). Brussels: Editions Desclée De Brouwer 1962. Pp. 182. B.Fr. 135.

Reviewed by NORMAN F. WATT

*The author, the late Étienne de Greeff, had a long career as Professor of Psychology and Criminology at the University of Louvain. A productive author, particularly in the field of criminology, he is perhaps most likely to be known to American readers as a novelist. The reviewer, Norman F. Watt, has been doing graduate work in clinical psychology at Ohio State University and, by the time this review is cast into print, will have received his PhD, but in absentia, since he is in Switzerland where he holds a fellowship at the Institute of Technology at Zurich. He has an abiding interest in language, and is working at Zurich in the area of psycholinguistics. Also he resonates to problems of personality theory and is curious about the tenets of existential psychology.*

IT WOULD be most unfortunate to be tried in a court of law presided over by a judge even remotely like the one Professor de Greeff believes to reside deeply within each of us. He would be neither impartial nor rational, he would always deliver a verdict of guilty, he could not be impeached or bribed, and his judgment could not be appealed to a higher court. However, the reader should not be misled by the title of the book; this is not a biography of man's villainous homunculus, but a personalized neuro-psychological treatise on the nature of man and his intrapersonal politics.

The theme is approached by way of the observation that an individual's appreciation of himself and his efforts is not based only on social, familial, or professional norms. Another, more important basis for evaluation is according to "internal norms," which are



purely subjective, private, and incommunicable. The author proposes to give a glimpse of this interior visage of man and to explain why success or satisfaction (by these personal standards) is impossible.

The theoretical premises of the book reflect the influence of Jung's individual psychology, phenomenological analysis, and existential philosophy. Prior to conscious awareness man experiences "the presence of being." This fundamental experience of life is, by its very nature, unlimited either in time or space. It is felt simply as "expansive movement." With the emergence of consciousness this vital tension acquires a spatio-temporal formulation in thought, which constitutes one's image of himself, the awareness of *his* existence.

At the origin (both neurologically and historically) of the conceptual process lie the "internal norms," which are not themselves concepts, but tensions in the process of taking form. The only form they can take to become conscious is as thought, tied to words, but since this process in itself limits and distorts them, the internal norms can never be fully expressed nor entirely comprehended. The diverse forms which they take are influenced by the specific course of events in one's life, but they develop outside the reach of his desires and intentions and, once developed, change very little. They are translated affectively by the sentiment of being satisfied or not, and this sentiment is experienced as singularly authentic and indisputable, because it stems from the very core of one's being. The internal norms constitute a person's inner, potential self, characterized as "a judge whose visage he does not know, a judge that he identifies as himself, but who he is certain, at the same time, corresponds to an order in itself, an absolute order, a truth and a justice absolute."

The author's message is clearly pessimistic. Since man experiences life simultaneously as infinite presence and as existence (i.e., limited being), he is condemned to live a contradiction. He can neither satisfy nor silence the demands of his judge. Consequently, the inevitable result of all of man's efforts is failure.

Occasionally the author's discussion of the internal norms sounds less like an important new way of describing human experience psychologically than like an announcement of a newly discovered hormone—albeit a pretty fancy, psychological hormone—and it *must* originate sub-cortically! He is fully aware of the discursive difficulties in portraying man as fundamentally dual in nature, and for the most part he handles them very adroitly. However he declines to explain a crucial step in the development of the thesis: how a basic "expansive movement," which can only be described as formless and without social referents, comes to acquire specific existential meaning, namely, the feeling of "having significance, being destined for something, being awaited by the world." The author admits that this transformation remains a mystery.

The book includes some research studies in support of the theory. At least by modern American standards, these are astonishingly naive. The sampling is unsystematic, the instruments are transparent questionnaires, and the analysis of results does not include a single test of statistical significance. For example, the attitudes of two age groups are compared "quantitatively" by examining the proportions of subjects in each group who endorsed the alternative answers to nine questions about their experiences of failure. A Chi-Square test (computed by the reviewer) of the *largest* difference found by the author failed to reach the .05 level of significance. In addition, by qualitative analysis the author is able to draw impressionistic conclusions which the quantitative results, even by his liberal interpretation, do not permit.

Fortunately the theme doesn't need the support of the research because it is developed thoroughly and persuasively, and accompanied by illustrative clinical examples. Professor de Greeff proposed to sketch a portrait of the interior visage of man. However frightening it may appear, many readers should find the face familiar. If not, they can at least enjoy the art work.



*How violent are the tempests aroused by the passions.*

—NIKOLAI GOGOL

## To Discover the Real

Austin M. Des Lauriers

*The Experience of Reality in Childhood Schizophrenia.* New York: International Universities Press, 1962. Pp. 215. \$5.00.

Reviewed by MARY ENGEL

*The author of this book, Austin Des Lauriers, is a Canadian whose PhD from the University of Montreal was followed by training at the Menninger Clinic, at Bellevue Hospital and at Topeka State Hospital. Since 1961 he has been Chief Clinical Psychologist at Michael Reese Hospital. The reviewer, Mary Engel, was born in Budapest. She has studied at Vanderbilt University and at George Peabody College, receiving her PhD from the latter. She held a post-doctoral fellowship at the Menninger Foundation from 1956-58. Then she was a supervisor in the USPHS Post-Doctoral Program in the Department of Child Psychology at Michael Reese Hospital. Since June of 1961 she has been Assistant Professor in the Graduate School of Education at Harvard University and Clinical Associate, Judge Baker Guidance Center.*

WITH this forceful and challenging book, Austin Des Lauriers joins the ranks of those who have permitted themselves to be pushed to the limit of perplexity regarding childhood schizophrenia. His theoretical orientation draws on the formulations of Freud, Federn, Piaget, Schilder and others. Thus, while the author's theoretical framework is not new, it is a lucid restatement of previous conceptualizations of schizophrenia as not a withdrawal from reality but as a total loss of reality experience, originating in "a severe diminution of narcissistic cathexis of . . . body boundaries." The behavior of the schizophrenic is seen " . . . as a disorganized and frantic effort at discovering himself . . . at establishing the bounds and limits of his reality . . . reality in which, alone, his . . . needs can be gratified."



The author describes the ego functions of the schizophrenic. These may be likened to the performance of musicians without a score and without a conductor. He insists that there is little sense in attempting to discover a melody in the cacophony, nor is it pertinent why there is no score and no conductor. What is important is to provide a conductor with a score, only then can the confusion become a unified melody.

From Des Laurier's views of the essence of the schizophrenic disorder there arises a rather definite method of treatment. The therapist, instead of studying the genetic and symbolic meanings of the patient's behavior and communications, in order to interpret these to him, refuses to "gratify his curiosity in the mysteries of the human psyche." Unambivalently and insistently he intrudes into the chaotic world of his patient and educates him directly in some of the most primary features of reality. He reiterates for the patient his weight, size, eye color, age and name, as these are different from attributes of the therapist. He marks for the patient those divisions in time and space which regulate not only the two person interaction but also more transcending aspects of human existence. The central assumption here is that the schizophrenic wants to know reality, but that he does not know how to gain this knowledge because of basic, structural ego deficiencies.

Thus, this method does not allow for the exploration of primary process, hallucinatory and delusional experiences. As the discussion of these is declared "off limits," the patient is relentlessly invited to attend to the surface of his body and "to each and every experience with the environment which . . . affects each part of his body." To the extent that the therapist fails to provide nutriment for the discovery of reality, he fails to address himself to the "essence" of the illness.

The book contains numerous examples from the four year treatment, by the author, of seven schizophrenic adolescent patients at Topeka State Hospital. Each patient was seen for an hour, seven days a week. With improvement, the frequency of the sessions was reduced.

Throughout the book, claims of proof of effectiveness are modest. Yet, most of the youngsters have been returned to the community. Interview excerpts, pre- and post-treatment psychological test data are provided. It is maintained that delusional behavior yields rather rapidly to Des Lauriers' method, and that in recovering, patients can be expected to pass through a number of phases. A symbiotic phase of "imprinting" is followed by imitation of the therapist, by acting-out, and finally, by identification.

The clarity of this book is marred by a few inadequately formulated ideas. The author's attempt to distinguish between intrinsic and external causes of schizophrenia is rather weak. The concept of acting-out is used broadly enough to include everything that adolescents can do or say that would evoke the resentment of ward personnel. The statement that in science "there is no possibility of finding anything . . . unless you are looking for it" is contradicted by a number of instances in the history of the sciences. But, from the point of view of clinical psychology, it is the presentation of test findings that is most cumbersome, leaving the reader considerable work in piecing together just what aspects of test production did or did not reflect changes in the patients.

The zest of face-to-face debates between Rudolf Ekstein and Des Lauriers (which took place in Topeka) is not reflected in the author's description of his theoretical differences between himself and Ekstein. In a sense, he is too impatiently busy showing how patients can be restored to normal living to engage in lengthy accounts of theoretical differences. While the scholarly temperament might well be left unsatisfied, the clinical impatience is easily shared by those who have ever attempted to cure a schizophrenic child. The occasional lack of elegance in conceptualization does not detract from the importance of this book. Even the most skeptical reader will be left with a feeling of therapeutic optimism.



*The past is never dead. It's not even past.*

—WILLIAM FAULKNER



*Second printing . . .*

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# Social Engineers at Work

Warren G. Bennis, Kenneth D. Benne and Robert Chin (Eds.)

*The Planning of Change: Readings in the Applied Behavioral Sciences.*  
New York: Holt, Rinehart & Winston, 1961. Pp. v + 781. \$8.50.

Reviewed by JOHN T. LANZETTA

*The editors of the present volume, Bennis, Benne and Chin, are identified by the reviewer, who is John T. Lanzetta. Dr. Lanzetta is normally Chairman of the Department of Psychology and Director of the Center for Research on Social Behavior at the University of Delaware; at the present, however, he is taking a year's leave to serve as liaison scientist with the Office of Naval Research in London. He received his doctorate with Launor Carter at the University of Rochester and ever since has occupied himself with small groups. The present review represents the product of his fourth willingness to stop what he is doing long enough to work for CP and its readers.*

I APPROACHED the reading of this volume with mixed feelings. In an age whose single constant is radical change a book with the stated aim of bringing about understanding of the potentialities, limitations and consequences of "planned change" is an important publishing event and bound to invite curiosity. But 700 double column pages, whatever the content, is as certain to invoke some dismay. Also, we have been overly maligned with heterogenous collections of papers loosely organized about a theme, the tenuous connecting threads often carelessly woven into a patchquilt by 'introductory' statements. A modicum of scepticism is surely justified when confronted with another "book of readings."

The present volume by Bennis, Benne, and Chin is similar in format

and intent to its predecessors and, like them, invites a mixed reaction. There is the familiar collection of previously published papers (there were 80; several others were expressly written for the book) grouped under major sub-themes of the general topic "planned change." Each of the four sub-sections, e.g., "the roots of planned change," "dynamics of the influence process," is preceded by a critical and theoretical introduction. The selections are, on the whole, of recent vintage and, consistent with the interdisciplinary orientation of the authors, drawn from diverse sources. They vary widely in length, in quality, and probably in familiarity. By judicious selection, the authors have attempted to bring together some current "conceptualizations of different aspects of application and change process." They are editorially and personally concerned with stimulating the application and adaption of theories of social and personal change to the special and important case of *planned* change; thus their target audience is the professional and the student in the various professions concerned with social change, e.g., social work, applied psychology, training, and psychiatry.

The authors bring to this "educational" task a wealth of personal experience as university teachers of courses in behavioral science, as trainers in numerous in-service education projects for administrators, educators, and managers, and as consultants on planned change in a variety of institutional settings. They have worked together for years in the Boston University Human Relations Center and

in the many laboratories in human relations sponsored by the Center and by the National Training Laboratories in Group Development. Bennis and Chin were trained in psychology while Benne was trained in philosophy and education (15 years ago he received the Kilpatrick award for distinguished contribution to American philosophy of Education). Two years ago Warren Bennis left Boston U. for M.I.T.'s School of Industrial Management where he is currently an associate professor. Kenneth Benne and Robert Chin remain at Boston University, the former as T. W. Berenson Professor of Human Relations and the latter as professor of psychology in the College of Liberal Arts and the Graduate School.

A BOOK of readings provides these experts little opportunity for expressing their own hard won integration of the valuational, theoretical, and practical issues which constitute a working theory of change. Facets of their position are revealed in the introductory sections and in a few papers, but on the whole the position is sketchily developed and remains indistinct. Often it is difficult to separate the editors' stand from that of the contributors; theoretical and valuational statements at times appear to be presented as a distillation of positions advanced by contributors but it often proves difficult to trace their genesis.

Such confusion is especially evident with respect to the concept of 'planned change,' the concept which serves as a central focus and presumably provides the guiding criterion for the selection of papers. For the editors, planned change "is a *deliberate and collaborative process* involving *change-agent* and *client-systems*. These systems are brought together to solve a problem or, more generally, to plan and attain an improved state of functioning in the client-system by utilizing and applying *valid knowledge*" (pg 11). Even ignoring the somewhat distinctive connotations attributed to the italicized phases, planned change, so defined, is surely not what many of the contributors have in mind. When Mannheim



elaborates the thesis that 'planning' is realized when processes of selection are narrowed by regulatory intervention and the forces at work are consciously controlled, he makes no mention of an existential and voluntary relationship between client and change-agent or of a mutual determination of goals. And surely when Merton and Gaudner describe the problems and prospects for applied social research they imply something less than a deliberate and collaborative process—a process involving mutual determination of goals, equal opportunity for influence, and emphasis on method rather than content—and something less than "an applied social science that in specific situations can select from among variables those most appropriate to a specific local situation in terms of its values, ethics, and moralities" (pg 16). One surmises that if given an opportunity many of the contributors would take issue with some of the editors' fundamental tenets concerning the change process and the role of social scientist as a change agent.

As a sourcebook for students, the selections capture nicely the scope, tentativeness and sometimes contradictory character of the present "state of the art." Some of the papers also provide useful introductory statements to major theoretical positions. Unfortunately such "key" papers are not accompanied by bibliographies, a lack that carries a danger for an audience of widely diverse backgrounds. It would be disastrous, for example, if alternative theories of social change were evaluated on the basis of the included papers when a major theorist such as Talcott Parsons is represented by a two page excerpt.

As a sourcebook for practitioners the volume will surely intimate all but the most arrogant for it presents an intellectual vista of enormous scope. One could not hope to become even superficially conversant in a lifetime with the disciplines represented. Yet some measure of intimidation is probably in order. When professionals become involved in the process of effecting social change, it behooves us all

that they be aware of the responsibilities and complexities of the task they undertake.

## Vive la Difference

Leopold Bellak (Ed.)

*Contemporary European Psychiatry.*  
New York: Grove Press, 1961.  
Pp. vii + 372. \$7.50.

Reviewed by HENRY P. DAVID

*The editor of this volume, Leopold Bellak, is identified by the reviewer who might well have pointed out that Bellak has a present affiliation with the Elmhurst General Hospital on Long Island where he is Director of Psychiatry. The reviewer, Henry David, likewise needs no long introduction here. This is his sixth review for CP. He is still Psychology Consultant for the New Jersey State Department of Institutions and Agencies, at Trenton and is currently editing a volume on International Resources in Mental Health.*

LEOPOLD BELLAK is no stranger to *CP*. Born and educated in Vienna and the United States, member in good standing of all three APA's, his considerable contributions to psychology, psychiatry, and psychoanalysis have been frequently cited in these pages. On this occasion he offers a platform to nine distinguished European psychiatrists and limits himself to a perceptive introduction and provocative comments on the processes of scientific creativity.

The nearly 400 page volume, with a soft cover edition priced at \$3.95, effectively bridges the language barrier and offers a comprehensive view of psychiatry in "the historically and geographically circumscribed schools of European psychiatric thought." Included are France (P. Pichot), Germany and Austria (H. Hoff and O. H. Arnold), Great Britain (A. Lewis),

Italy (U. Cerletti), Scandinavia (G. Langfeldt), Soviet Union (V. A. Gilyarovsky), and Switzerland (G. Benedetti and C. Müller). Most contributors adhered to the editor's instructions to consider first the historical development of psychiatric treatment, training, and research, then to deal with forensic aspects and relations between psychiatry and other disciplines. Each chapter is greatly enhanced by extensive references and valuable listings of training and treatment centers, often complete with address, director's name, and descriptions of available facilities.

While it is difficult to quarrel with the editor's invitations, it is regrettable that Belgian and Dutch contributors could not be included. Austrians were asked to report on Germany because no German was believed "to have the necessary detachment to give a balanced view" of the many schisms still dividing that land. Finland, Greece, Iceland, Ireland, and Spain are not represented. Unfortunately, the Soviet review fails to cite current trends in other Eastern European countries.

As BELLAK succinctly observes, "American and European psychiatric scenes are quite different from one another." A descriptive, genetic-organic trend appears prevalent in most European centers; there is relatively little interest in psychoanalysis and less concern with psychodynamics. With a history of social welfare legislation, European psychiatrists tend to be identified with institutions and teaching, with description and classification, and with rehabilitation and community programs. There is more emphasis on psychoses, less on neurotics and private practice. Progress in clinical psychology and social work has been slower.

There is pardonable pride in the national presentations, and puzzled questioning of the supposed American overemphasis on psychoanalysis. It is sobering to recall that many major contributions to psychiatry originated in Europe. In recent years, tranquilizers and the therapeutic community have been imported. True, American sophistication in evaluation and improvement refined some European hunches and proved others mistaken, but the fact



remains that our European colleagues tend to work in an atmosphere that somehow appears more conducive to creativity. Their accomplishments are all the more remarkable when one considers the lack of massive governmental financing or support from large foundations, and the limited opportunities for personal advancement in "schools" that are often dominated for decades by one professor.

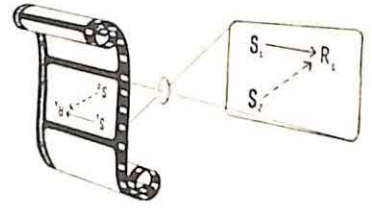
The kind of—well supported, professional and scientific—clinical psychology we know in the United States hardly exists in Europe. In Switzerland most of the early contributions to the development of clinical psychology were made by physicians (Rorschach, Oberholzer). Although psychology was classified under philosophy in most Swiss universities, psychologists have obtained clinical training and are practicing, particularly in children's clinics. In France and Italy, clinical psychologists are frequently physicians, with graduate degrees in psychology, e.g., Pierre Pichot, who represents French psychiatry in this volume, also heads the Institute of Psychology at the Sorbonne. He reports that a 1953 Paris court decision declared psychotherapy medical practice and limited psychologists' activities.

In Germany major emphasis is on work with children and empirical investigations of personality and not on professional practice; Austria has very limited facilities for training clinicians. Psychological services are expanding in Denmark, Norway, and Sweden. In the Soviet Union, Pavlovian concepts dominate psychiatry and psychology, with research focusing on physiological studies; there is no private practice. In the United Kingdom a vigorous clinical psychology training program has developed at the University of London. While the National Health Service does not employ non-medical psychotherapists, qualified psychologists are practicing in child guidance centers.

For jointly attaining their goals of facilitating international communication and broadening perspectives, the editor, contributors, and translators deserve plaudits. Though not quite a substitute for "Europe on 5 Dollars a Day," the prospective traveler would be well advised to stash this informative volume in his flight bag.

# INSTRUCTIONAL MEDIA

Edited by A. A. Lumsdaine



## Programming's Present's Prologue

John E. Coulson (Ed.)

*Programmed Learning and Computer-Based Instruction.* New York: John Wiley & Sons, 1962. Pp. xv + 291. \$6.75.

Reviewed by NANCY S. ANDERSON

*Dr. Coulson, the editor of this triple-faceted entering wedge into the history of an imponderable intellectronic future (Ramo's term), got a Columbia MA ('53) and PhD ('56) following a 1952 Arizona BA. For the past several years he has headed the System Development Corporation's work on programmed instruction. The original Coulson-Silberman team at SDC published (1959) one of the early experiments on programming variables, then expanded to pioneer in evolving a one-S computer-mediated program into a 20-S "Class" facility for further experimentation. Coulson recently reviewed Finn & Perrin's survey of the teaching-machine industry for CP (Oct., 1962, 7, 385f.). Dr. Anderson earned her qualifications for reviewing such a book as this partly through her own even earlier pioneering in using a computer as a teaching machine when, in company with Rath and Brainerd at the IBM Research Center, circa 1958, she used an IBM 650 to teach binary arithmetic—work they reported in the first book on programmed instruction published (edited by Galanter, also for Wiley: see Rothkopf's CP review, Apr. 1960, 5, 104f.) Reviewer, like editor, got a BA in the West (Colorado) in 1952 before migrating east (to Ohio State) for her '53 MA and '56*

*PhD. Currently she is Associate Professor at the University of Maryland.*

THE surge in the area of teaching machines and programmed instruction is still increasing as is evidenced by new journals, new books, numerous meetings, Wall Street expectations, and the fact that book companies now ask a prospective author if he's programming any course material, in addition to writing a textbook. In the late 1950's many of us in this field wondered how a computer might be used in this promising venture. Some of us had been carefully taught that the computer was a "general purpose tool," merely waiting for us to use it in our laboratory work as well as for our computational needs. After all, the general purpose computer can do almost anything; it can recognize handwriting, recognize speech from certain limited vocabularies, and even "learn" to do these and other complex tasks. However, many of us also learned that one of the difficulties of using a computer in the laboratory was in the formulation of the research problem so that it could be programmed on the computer. Then, how could the computer be used as a teaching machine?

The title of this book indicates that the computer is being used as a teach-



ing machine, and it is to the book's credit that it does present much of the research on the present day use of a computer as a teaching machine. The title further implies that the book also says something about programmed learning; however, this part of the book is disappointing. It should be noted that the book is actually a collection of papers—that is, the Proceedings of the October, 1961, Conference on Application of Digital Computers to Automated Instruction, co-sponsored by the Office of Naval Research and the System Development Corporation.

As is often the case with other conference proceedings or publications of similar conferences, the book lacks continuity from chapter to chapter. However, the editor is to be given credit for his organization in that the book is divided into three parts with papers on (1) Theory and Experimentation in Programmed Instruction, (2) Computer-Based Instructional Systems, and (3) Computer Technology in Automated Teaching. An advantage of this kind of organization is that a reader can choose the section he wishes, depending on his interests.

This reviewer is not convinced, however, that Part I (which contains half of the book's 300 pages), adequately represents a state-of-the-art summary about basic problems in programmed instruction. The twelve papers are more a state-of-the-art for twelve people in the field. It is hard to believe that the entire area of programmed instruction is represented here; there are some obvious omissions. One might hope though, that what would be presented would be those areas of programmed instruction relevant, in some way, to computer based programmed instruction. In this reviewer's opinion, this is not true of these papers, nor is it clear that Part I represents much more than a set of twelve papers by twelve people who were invited to participate in the conference.

These arguments may not represent serious drawbacks, for it is clear that it is becoming more popular to publish conference proceedings in hard covers. Also, those interested in the area of programmed instruction who did not attend the conference need only purchase the book to find out who in the field said

what about programmed learning. Although the area of programmed instruction is not adequately represented, there are worthwhile papers and ideas in this book for the interested researcher. Research on the form of response, size of step, and individual differences is reported by several authors. Silberman presents a short but clear review of 80 or so experimental studies. Lumsdaine describes the theoretical and practical problems one faces in doing research in this area. This reviewer agrees with him that, "the sterility of much of the formal experimentation on instructional media is not to be denied..." Other papers are also worth reading; for example, Dear and Atkinson's mathematical model for allocation of items for branching is a refreshing solution for a problem previously not analytically formulated.

**P**ART II (about 100 pages) contains six papers describing operational computer-based programmed instruction systems. Here is a collection of papers that describe the state-of-the-art in computer-based instruction. The papers show what teaching is being done by computer devices, and each of the authors presents his view of the problems he has encountered. Some of the ideas for development of instruction systems are similar for Uttal and his colleagues using the IBM 650, Coulson with the Bendix G-15, Bitzer, et al. using the ILLIAC, Licklider with the PDP-1, and Chapman and Carpenter using the Thompson Ramo Wooldridge RW-400. Most of these papers assure the reader that it is possible to use a computer as a teaching machine, given the following: an interested researcher, a computer, the appropriate input-output equipment, intelligent computer programmers, subject matter experts, and last but not least, a large (but finite) amount of money to pay for computer time. Their authors emphasize the advantages of the "general flexibility" of the system (including the computer with its general-purpose computer-programming language). The advantages they point to for programmed instruction are: (1) adaptability of computers for sequencing materials in a large number of ways to adjust for individual differences, (2) ability (through multiplexing and/or

time-sharing equipment) to teach two or more students simultaneously, (3) ability to compute scores and statistical analyses, (4) ability to use keyboard input-output devices and cathode ray tube displays, (5) adaptability for altering presentation of items in a large variety of ways, and (6) ability of the computer to modify its own program by "learning" and other "artificial-intelligence" procedures.

Thus, the computer is a large, general purpose, adaptable teaching machine. It is the reviewer's suspicion that the computer-teacher's salary is greater than that of the highest full professor in the world. But, certainly the computer is more flexible for computation and has an infinite memory with relatively fast access, etc. Is the computer to be used as a general-purpose adaptable teaching machine? Several papers throughout the entire book talk about enhancement of the interactive feature of the teacher-student process by using the computer as a "teacher." Senders makes a plea for trying to reproduce the "adaptive teacher." In order to accomplish this, it may not be necessary to wait for an adequate learning theory of behavior, for what the programmed-instruction researcher is concerned about is a training theory. What is needed is an adaptive computer which manipulates the environment to maximize the rate of increase in student performance.

However, adaptability will cost money. The cost factor involved in using a computer could be reduced if several students can use the same machine simultaneously. After all, the computer is "faster than thought" and students need time to think before responding. Part III of this book (30 pages) summarizes our hopes and fears of achieving this goal—that is, an adaptive computer-based instruction system with multiple student stations. Computer engineers will have to build faster switching circuits and, as Estrin implies, only then can we teach many students at once. But Teager warns that computer "software" (a sophisticated programming language) is a problem too, and Huskey's answer is to use a dialect of ALGOL, the general-purpose computer language.

Sometimes it appears that further development of computers is all that



# EUGENICS

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Published by

AMERICAN

EUGENICS

SOCIETY

230 Park Avenue, New York, N. Y.

Subscription, \$7.00 a year

Membership \$5.00 (foreign \$2.50)

is needed to solve all the problems of automated instruction. But would this solve our problems? The reviewer does not think a favorable gain in the learning process by use of computer flexibility in relation to the cost involved has as yet been adequately demonstrated, except for specific materials which are not adaptable to programmed texts. For certain verbal and arithmetical materials, when one uses the flexibility of the computer, is the teaching process now a better one than with a linearly programmed textbook?

One often hears that bright students get bored with programmed instruction. As noted earlier, some of the researchers emphasize the advantages for computer-based instruction to be gained from a student-teacher interactive process. All of the applications in this book give control over this interaction to the computer and its program. Why not allow the student some additional interactive control? At present, the student can only skip items, branch, close the book, turn off the machine, or leave the room. Why not let him ask questions, interrogate the computer, request further practice, request a change in materials to be studied? J. Swets, A. Weisz, J. Senders, and others at Bolt, Beranek, and Newman are pursuing some research problems in this area, such as, allowing the student to choose his mode of practice rote versus corrective) for "sound learning" and visual discrimination learning. The idea of student-controlled interaction with a computer as well as computer-controlled interaction was also mentioned by Coulson and others during a symposium on programmed instruction at the 1962 annual convention of the American Psychological Association in St. Louis.

In this reviewer's opinion, further research is needed in this special area of programmed instruction, and it can be rather easily accomplished on present computer-teaching machines. Because of the computer's flexibility, it can be used as a general-purpose laboratory tool; however, researchers are just beginning to learn to use the computer effectively in programmed-instruction applications. Another area where the computer could help the pro-

grammed-instruction researcher is in the preparation and organization of items. This application has been suggested at other conferences, and it appears from this book that the Evans-Homme-Glaser Ruleg system, with Dear and Atkinson's model as one approach for allocation of items, is waiting and ready for a bright computer programmer. Furthermore, in addition to needing a new computer language for the control of a computer teaching machine, perhaps what is needed even more is a computer-compiler language which would operate on subject matter rules, and parameters of optimum allocation of items, so as to give at least a first draft of the subject matter program—and possibly then to revise it after use and finally, after adequate testing, to compute a statistical summary of normative data to be presented with the programmed-instruction material.

The numerous examples of computer use described here indicate that the latter half of the book is a reference source which includes several self-contained descriptions of computer-based instruction applications, as well as presentations of technological hardware and computer software problems. The reviewer feels that the efficiency of using a large computer to control a slide projector may be questionable except as a laboratory tool. As a laboratory tool, the computer's flexibility for research purposes may allow it, during the same time period, to solve differential equations, teach many different students, interact with each student, allow each student to interact with it by controlling some part of his learning process, and generate new programs or revisions of prior programs. Such a system would be an ideal and efficient computer-based teaching machine.



*The more we examine the mechanism of thought, the more we shall see that the automatic, unconscious action of the mind enters largely into all its processes. Our definite ideas are stepping-stones: how we get from one to the other, we do not know: something carries us; we do not take the step.*

—OLIVER WENDELL HOLMES, SR.





# ON THE OTHER HAND



NOT I, SAYS WRENN

In a curious sort of way the review of Kate Hevner Mueller's *Student Personnel Work in Higher Education* (Robert Ross in *CP* 1962, 7, 271-272) gives my introduction credit for a point of view that I think belongs to the author of the book. I am said to have an "awareness of the personnel worker as a teacher that is not reflected throughout the book." This is in error. Whatever I said in the introduction was based upon Mueller's presentation as basic themes of the book that there is an emerging awareness "that the teaching and learning processes outside the classroom are prime responsibilities of college personnel workers," and that "the integration of goals in higher education can be vitally interwoven with the specific functions which personnel workers perform." The quotes above are not mine but from the review and in the context that the author's treatment was not based upon these premises. I could use these same quotes but say that they are the strength of the book. We couldn't be further apart.

How could this be? It could be that reviewer and editor were working from different referent points of expectation—I finding more than I had hoped for in the treatment of basic sociology and philosophy (more than any other book in the field, I am sure), the reviewer finding less than he had hoped for. Or perhaps I am more impressed than the reviewer with a psychologist who writes also as a scholar in sociology and higher education.

The review leaves one with the feeling that Dr. Mueller's treatment is lacking in psychological sophistication—chapters on mental hygiene, emotional factors in discipline situations, the mental health of the personnel worker, etc. notwithstanding. I too wished for more attention to college counseling but the separate literature on this topic is extensive and can be filled in more easily than can the literature on college personnel topics that

are treated in this volume and not analyzed elsewhere.

C. GILBERT WRENN  
University of Minnesota

## GERMAN AND AMERICAN REVIEWS

It may be of interest to *CP* readers to know that the German journal *Archiv für die gesamte Psychologie* occasionally reviews some books which are also reviewed by *CP*. In the main the reviews seem to be thorough abstractions, enumerating a book's content in a sequential order. For instance, F. Attneave, *Applications of Information Theory to Psychology* was considered extremely useful, thorough, and of interest to psychologist as well as non-psychologist by Guttman, the German reviewer. This critique was similar to H. Quastler's (*CP*, Jul. 1960, 5, 233) who found the same book readable, skillful and a blend of solid science with artistic elegance. In the *Archiv* positive critique is generally added by a few short phrases at the beginning or end of a paragraph. Thus a chapter of a reviewed book may be introduced as very useful, detailed, carefully planned, stimulating, clearly written, free of university jargon, etc. Each book is usually compared with the content and quality of other books in its area. Negative criticism is less frequent but well substantiated when given. The author of a statistical book, for instance, is criticized for "taking over the symbols and the mistakes" of another text. Specific errors are pointed out by page and sentence. Another author in the area of psychological testing is criticized for the lack of controls, for the absence of correlations, and for confounding psychometric and clinical data with the reviewer asking: "Hat sich der Autor nicht zwischen zwei Stühle gesetzt?"

Besides Attneave's book, other reviews in common to the two journals are K. H. Winkelsträter's, *Das Betteln der Zoo-Tiere*, and Karl Bühler's, *Das Gestaltprinzip im Leben des Menschen und der Tiere*. The *Archiv* review of the former

was brief but similar to Toback's review (*CP*, Nov. 1961, 6, 391) "On the Other Hand." Bühler's book received an entirely different review in the *Archiv* from that in *CP*. A Rüssel (Braunschweig) found none of the limitations which were pointed out by D. E. Berlyne (*CP*, Feb. 1962, 7, 45). As might be expected, the German reviewer did not note any lack in operational definitions or any discontinuity in the presented material.

Of course the majority of reviews in the *Archiv* are of German publications, but American, Dutch, French, and Swiss works are examined. Each issue has a review section discussing about 30 books yearly, treating a particular book one year after publication in approximately 600 words. There is no information about the author's background or the reviewer's other than their name and locale. Some short reviews are "Autor-referate" written by the respective authors themselves. Perhaps the Germans really are more casual about "just another book" than their American counterparts.

FRANK WESLEY  
Portland State College

## CANDOR, NOT SHALLOWNESS

Counselors are humane people and Earl A. Koile's review of *Problems in Counseling: A Case Study Approach* by James F. Adams (*CP*, January, 1963) sets a properly charitable tone before essaying an evaluation of the book. However, other users of this casebook may not agree that: "It communicates very little about the actual process of counseling or about the behavior of a perceptive and able counselor." Or "In only one instance is a case reported in sufficient depth and detail for discernment of what the counselor is doing to facilitate client self-exploration, discovery, and change" (*Idem*, p. 16).

Clear but unpretentious case reports titled "I've Been Bored" and "He Had What It Takes" convey more than a little understanding of what it is like to work with junior high youth.

As the title indicates, Dr. Adams' book presents a case study approach to problems in counseling. To this end the author has collected and edited a number of cases by different counselors at different levels of experience and education in a variety of settings. Quite a few seem to be public school teachers or would-be 'teacher-counselors.'

Dr. Koile finds not uniformly high the degree of psychological understand-



ing and theory behind the cases reported in the book. This is just about what one might expect to find if somehow one could secure a representative sampling of what passes for counseling in many places in our nation. In no sense am I pleading that counsel educators should be content with mediocrity in case handling or theorizing. But it seems amiss to expect any highly developed or consistent rationale to underlie the work of many different persons. In a word, I find Adams' casebook valuable for reporting with candor the reciprocal behavior of a number of counselors and clients. In NDEA Institutes and graduate courses for neophytes Adams' book has been well received by students.

JOHN W. REID  
Indiana State College, Pa.

#### DISSENT ON CONFORMITY AND DEVIATION

Harold Gerard (*CP*, Mar. 1963, 8, pp. 12-13), in his review of *Conformity and Deviation* (I. A. Berg and B. M. Bass, Eds.), says "Pauline Pepinski (sic) examines the relationship between conformity and creativity, where the creative individual is seen as a nonconformer (another meaning of deviation?). The experiment, which grew out of her 'abstruse' theory based on the analysis of several field studies, is entirely unrelated to the original problem. She finds that if you reward a person for conforming, he will conform, whereas if you reward him for not conforming, he will not conform. What this has to do with creativity eludes this reviewer" (p. 13).

For the sake of accuracy—

(1) My paper did not undertake nor purport to deal with "the relationship between conformity and creativity," but with conditions antecedent to behavior referred to as "nonconforming" and "productive." As I have repeatedly emphasized elsewhere, so-called "creativity" may conceivably be included within, but certainly does *not* correspond to the conjoint category as explicitly defined. I am not against "creativity;" it is simply not my word. It is not now, nor has it ever been, the subject of my study.

(2) The paper in question specifically disavows "deviation" as an index of "nonconformity" in the context of my own research.

(3) "The" experiment mentioned by the reviewer (actually one of two for which results were reported) did not pretend to "grow out of" any "theory" whatever, but was designed to test an

hypothesis that came directly from the second of three summarized exploratory field studies.

(4) What that experiment "has to do with creativity" eludes me, too. Such an aim was neither claimed nor intended. Unfortunately, it is rather "the original problem" itself that seems to have eluded the reviewer.

And, finally, in a later paragraph he begs his question when he asks why the book is "dignified" by a "hard cover binding and a flashy title." True, the cover is pretty hard, but what's so flashy about "Conformity and Deviation?" The contributors to the symposium—and other interested readers of *CP*—would have found more helpful a critical assessment that was both informed and constructive.

Oh yes, my name happens to end with a "y," not an "i."

PAULINE N. PEPINSKY  
Worthington, Ohio

#### GEORGE KELLY ON A TANGENT

After reading George Kelly's review of Szasz's book, *The Myth of Mental Illness*, in *CP*, October 1962, I had to re-read the book. I believe Kelly's review is tangential to the book's kernel theme and therefore, falls short of evaluating the role and contribution of the work on several counts.

First, Kelly did not cast the book in the historical perspective as suggested in your "Comments to the Reviewers." I am rather certain that Szasz's book is the first to wrestle with the much discussed issue of "whether mental illness is really illness" (like physical illness), and to explore this in all its ramifications. This fact alone should make the volume an important contribution; but there is more. The volume vividly, though a bit turgidly, discusses the historical and socio-cultural conditions of the 19th century which led the physician to declare that psychological problems (which Szasz says are really the problems of social living, ethics, values, etc.) are "illness." Szasz maintains that at that time such a declaration was a "progressive-humanitarian" — although not a scientific—step since the mentally disturbed were considered to be "possessed." Such a declaration freed the "mentally ill" from witch hunts, but now, due to vast cultural changes, such a concept of "illness" only serves to shroud the clearer understanding of the problems of interpersonal relationships, values, ethics, etc. During the 19th century, it was the physician who declared that psychological

troubles were illnesses, and he had no choice but to mold the mental in the cast of the physical. Szasz traces this history beautifully and shows how the humanitarian step of the 19th century has run into conflict with the scientific endeavors of the 20th century and has thus resulted in the logical, social, legal and other confusions in which we now find ourselves. But, by reading Kelly's review, one gets the impression that, for all these muddles, Szasz holds a single person, Freud, responsible. Actually, Szasz does not quite say that (although there are some criticisms of Freud); rather Szasz credits Freud with having vaguely seen the contradictions, but he was too much of a prisoner of the sociology of knowledge of his era and ethos to do much about it (p. 170). It is Kelly, and not Szasz, who uses Freud as the scapegoat and carries his personal vendetta against Freud to the pages of *CP*.

Secondly, your reviewer criticizes Szasz because he "does not question the concept of illness" (*CP*, p. 363, column III); that he "doesn't go all out in . . . debunking mental illness . . . both mental and illness" (p. 363, column II). One really wonder what Kelly would have wanted Szasz to say, since the main stream of Szasz's argument, and even the title of the book, clearly says what Kelly maintains Szasz should have said. It is perhaps true that at various places Szasz could have taken one more logical step in his argument, e.g., the inter-professional conflict about who can do psychotherapy, etc. (pp. 292, 296), but all that is minor and should really be the onus of psychologists themselves.

Kelly further bemoans the fact that Szasz has discussed only hysteria in his work. It appears to me that Kelly has missed the significance of the book. As I read the volume, the implication seems to be clear that hysteria can be better understood (and therefore better managed), if one tries to understand hysteria in terms of social communication between people, and the ethics and values of a culture, etc., rather than in the framework of orthodox analytic-hydraulic model. The further implication of Szasz's work is that, like hysteria, other "psychopathologies" can also be better understood by following a similar model, e.g., the double-bind phenomena and schizophrenia. I am afraid this aspect of Szasz's work is entirely missed or ignored by Kelly.

In an attempt to show the shortcomings of the book, Kelly's review runs into



a contradiction. On p. 363, column III, he maintains that "he [Szasz] doesn't question the concept of illness," while on p. 364, column III, he says that "the major part of his book has to do with the models which may be used to replace the illness model . . ." If the bulk of the volume is devoted to developing a substitute model, then it is obvious that Szasz is questioning the 'illness' model. But that is exactly the issue on which Kelly takes Szasz to task—for not doing what he did do in his book.

Contrary to Kelly, many of us evaluate the book as an important contribution to the field of social sciences since

for the first time it takes the issue of "mental illness and health," "psychopathology," "psychotherapy," etc., out of the medico-physio-anatomical realm and places it squarely where it belongs, namely, in the context of socio-cultural and human relationship. I believe that Szasz's work compares with those rare works in the field of social sciences—like C. Wright Mills' *Power Elite*, Erik Erikson's *Childhood and Society*—which have molded the thinking of our times. I believe that the history of social science will bear me out.

SOLAN LAL SHARMA  
Los Angeles, Cal.

## Recent Slavic Books in Psychology

Compiled by JOSEF BROŽEK  
Lehigh University  
with the assistance of STEFAN SLAK

### Bulgarian

EMANUILOVA, EKATERINA. *Suvremenna materialisticheska psikhologiya* (Contemporary materialist psychology: Bibliographical index, 1948-1956). Sofia: Bulgarian Academy of Sciences, 1959. Pp. 230.

*Izvestiya na pedagogitsheskiya institut* (BULLETIN OF THE PEDAGOGICAL INSTITUTE, Vol. IX). Sofia: Bulgarian Academy of Sciences, 1960. Pp. 200.

NASTEY, G., and P. KOINOV. *Suznaniye i retikularna formatsiya* (Consciousness and reticular formation). Sofia: Bulgarian Academy of Sciences, 1961. Pp. 78.

PIR'OV, G. D. *Detska psikhologiya s defektologiya* (Child psychology, normal and abnormal; 2nd ed.). Sofia: State Publishing House "Science and Art," 1959. Pp. 555.

### Czech

BOUCHAL, M., and R. KONEČNÝ. *Psychologie v lékařství a psychiatrická pedagogika* (Psychology in medicine and introduction to psychiatry). Prague: State Pedagogical Publishing House, 1961. Pp. 160.

HOLAS, E. (Ed.). *Sborník prací pedagogických a psychologických. I.* (Pedagogical and psychological studies). Acta Universitatis Palackianae Olomucensis Paedagogica-Psychologica. I. Prague: State Pedagogical Publishing House, 1960. Pp. 88.

HOSKOVEC, J., and P. ŘÍČAN. *Psychologické diagnostické pomůcky* (Catalogue of the Archives of Diagnostic Aids). Prague: Czechoslovak Psychological Association, Czechoslovak Academy of Sciences, 1961. Pp. 39.

HOSKOVEC, J., and Z. KAŇKA. *Wechslerovy zkoušky W-B, WAIS, WISC: Bibliografie* (Bibliography on D. Wechsler's intelligence scales). Prague: Czechoslovak Psychological Association, Czechoslovak Academy of Sciences, 1961. Pp. 63.

VYMĚTAL, OSVALD (Ed.). *Neurózy*. (Neuroses: Collected works from the Psychiatric Clinic). Acta Universitatis Palackianae Olomucensis, Supplementum VI. Prague: State Pedagogical Publishing House, 1960. Pp. 167.

WOLF, E., and O. JANOTA (Eds.). *Neuroses* (Proceedings of the 1959 psychiatric congress, in Russian and English). Prague: State Medical Publishing House, 1961. Pp. 332.

MACHAČ, M. *Některé typové zvláštnosti dominant u člověka* (Some typological characteristics of the cortical dominants in man). Transactions, Series of Social Sciences, Vol. 71, No. 3. Prague: Czechoslovak Academy of Sciences, 1961. Pp. 120.

MELZER, J. *Psychologie v železniční dopravě* (Psychology in railroad transportation). Prague: Transportation and Communications Publishing House, 1961. Pp. 62.

MENERT, E. *Fysiologický základ logického myšlení* (Physiological bases of logical thinking). Prague: Czechoslovak Academy of Sciences, 1960. Pp. 214.

Psychologické Studie (Psychological Studies). Acta Universitatis Carolinae Philosophica et Historica, No. 1. Prague: Charles University, 1961. Pp. 127.

ROUBÍČEK, J. *Experimentální psychosy* (Experimentally induced psychoses). Prague: State Health Publishing House, 1961. Pp. 282.

SOVÁK, M. *Výchova leváků v rodině* (Training of left-handed children in the family). Prague: State Pedagogical Publishing House, 1961. Pp. 83.

*Symposium on the Pathogenesis of Essential Hypertension* (in English). Prague: State Medical Publishing House, 1961. Pp. 476.

VANÝSEK, J. (Ed.). *Electroretinographia* (Electroretinography, proceedings of a 1959 symposium). Brno: Medical Faculty of the University of J. E. Purkyně, 1960. Pp. 374.

### Polish

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GURYCKA, ANTONINA. *Zainteresowania uczniów* (The interests of apprentices). Warsaw: State Educational Publishing House, 1961. Pp. 158.

KOWALSKI, S. *Różwój mowy i myślenia dziecka* (Development of speech and thinking in children). Warsaw: State Scientific Publishing House, 1962. Pp. 310.

LEONTIEW, A. N. *O rozwoju psychiki* (On the development of the mind; translated from Russian). Warsaw: State Scientific Publishing House, 1962. Pp. 207.

OKON, J. *Analiza czynnikowa w psychologii* (Factorial analysis in psy-



chology). Warsaw: State Scientific Publishing House, 1960. Pp. 272.

PIETER, J. *Psychologia uczenia się* (Psychology of learning). Warsaw: State Educational Publishing House, 1961. Pp. 320.

SLOBODZIAN, ZOFIA. *Twórcza praca dziecka* (Creative activities of the child). Warsaw: State Educational Publishing House, 1961. Pp. 125 + 54 drawings.

SLONIEWSKA, HELENA. *Psychologiczna analiza zainteresowania* (Psychological analysis of interests). Poznań: Society of the Friends of Science, 1959. Pp. 110.

SPIONEK, HALINA. *Powstawanie orientacji w prawej i lewej stronie schematu ciała w ontogenezie* (Development of the perception of right and left side of the child's body). Warsaw: State Scientific Publishing House, 1961. Pp. 194.

*Studia psychologiczne* (Psychological studies, Vol. 3). Wrocław: Polish Academy of Sciences, Section of Pedagogical Sciences, 1960. Pp. 222.

SZCZERBA, V. *Zagadnienia wychowania moralnego* (Problems of ethical education). Warsaw: State Educational Publishing House, 1961. Pp. 359.

SZEWCUK, W. *Analiza psychologiczna zawodu kierowcy suwnicy* (Psychological analysis of the work of crane operators). Kraków: State Scientific Publishing House, 1959. Pp. 65.

SZEWCUK, W. *Badania eksperymentalne nad rozumieniem zdań* (Experimental investigations on the comprehension of sentences). Kraków: Jagiellonian University Press, 1960. Pp. 229.

WITWICKI, T. *Badanie praktycznej sprawności umysłowej metodą wkładanek* (Examination of practical mental ability by form board tests). Toruń: Polish Academy of Sciences, 1962. Pp. 100.

WŁODARSKI, Z. *Zaburzenia równowagi procesów nerwowych u dzieci* (Disturbances of the equilibrium of nervous processes in children). Warsaw: State Scientific Publishing House, 1960. Pp. 145.

*Zagadnienia dynamiki rozwoju człowieka* (The dynamics of human development: Proceedings of an Interdisciplinary Conference). Warsaw: State Scientific Publishing House, 1960. Pp. 248.

ZAGALA, B. (Pictures) and S. SZUMAN (Text). *Malujemy: Album sztuki dziecka* (We are painting: An album of child art). Warsaw: Nasza Księgarnia, 1961. Pp. 27 + 151 colored plates.

ZAKRZEWSKI, P. *Współdziałanie w przebiegu młodocianych i dorosłych z nieletnimi* (Collaboration of young persons and adults in the criminal activities of youth under 17 years of age). Kraków: Jagiellonian University Press, 1960. Pp. 160.

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#### Serbo-Croatian

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#### Slovak

CIGÁNEK, L. *Die elektroencephalographische Lichtreizantwort der menschlichen Hirnrinde* (E.e.g. responses of the human cerebral cortex to light stimuli). Bratislava: Slovak Academy of Sciences, 1961. Pp. 149.

DUDA, P. *Izmenenia interoseptivnykh uslovnykh reflexov posle raboty* (Alterations in conditioned interoceptive reflexes brought about by physical work; in Russian). Bratislava: Slovak Academy of Sciences, 1961. Pp. 58.

JANÍK, A. *Poruchy psychomotoriky pri psychózach* (Psychomotor disorders in psychoses). Bratislava: Slovak Academy of Sciences, 1961. Pp. 220.

*Psychologické štúdie, II* (Psychological Studies, II). Bratislava: Slovak Academy of Sciences, 1960. Pp. 265.

*Psychologické štúdie, III* (Psychological studies, III). Bratislava: Slovak Academy of Sciences, 1961. Pp. 284.

RUŠČÁK, MICHAL. *Über die bedingte Reaktion auf den erhöhten Muskelstoffwechsel* (Conditioned metabolic responses induced by increased muscle activity). Bratislava: Slovak Academy of Sciences, 1961. Pp. 108.

WOODWORTH, R. S. *Experimentálna psychológia* (Experimental psychology; translated from English). Bratislava: Slovak Academy of Sciences, 1960. Pp. 1022.

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TOLIČIČ, I. *Otroka spoznamo v igri* (We get to know the child in games). Ljubljana: State Publishing House of Slovenia, 1961. Pp. 120.

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(Preparation of this list was facilitated by the National Science Foundation grant G 19469.)

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*Contemporary Psychology: A Journal of Reviews* is published monthly. The subscription per year is \$10.00, foreign \$10.50, single number \$1.00.

Published by the American Psychological Association at 2210 W. Dallas, Houston, Texas and 1333 Sixteenth St., N.W., Washington 6, D. C.

Second-class postage paid at Houston, Texas and at additional mailing offices. Accepted for mailing at special postage rate stated in section 132.122, Postal Manual.

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# Contemporary Psychology

A JOURNAL OF REVIEWS

VOLUME VIII

June, 1963

NUMBER 6

## Hubbub at the Hub

Ives Hendrick (Ed.)

*The Birth of an Institute: Twenty-fifth Anniversary of the Boston Psychoanalytic Institute.* Freeport, Maine: The Bond Wheelwright Co., 1961. Pp. v + 164. \$5.00

Reviewed by SAUL ROSENZWEIG

The editor of this volume, Ives Hendrick, received his medical degree at Yale, then spent some time at the Berlin Psychiatric Institute before becoming, in 1932, Training Analyst at the Boston Psychoanalytic Institute. In 1944 he became Director of the Harvard Teaching Unit at the Southard Clinic and since 1954 has been Clinical Professor of Psychiatry at Harvard Medical School. The reviewer, Saul Rosenzweig, is Harvard all the way, receiving his PhD in 1932. He says that from the beginning he has had a predilection for the history of the field, especially in the area of clinical psychology and psychoanalysis. He has carried this interest with him and has combined it with others during periods at Worcester State Hospital and Clark University, at the Western State Psychiatric Institute and the University of Pittsburgh and, since 1949, at Washington University, where he is now Professor of Psychology and Medical Psychology. He has worked systematically on the verification of psychoanalytic concepts through laboratory methods and his contributions to frustration theory and to psychodiagnosis are well known. Presently he is continuing his concern with idiodynamics through a focus on the conception and development of ideas in the person-culture context.

VIRGIL has Aeneas say to his companions in exile: "Forsam et haec olim meminisse iuvabit." ("It will perhaps be pleasant one day to look back on these events.") and the professionally ostracized Freud in his cryptic, self-analytic "Screen Memories" (1899) quoted the words as he expounded his thesis that what one recalls and what one has actually experienced have a complex rather than a simple equivalence. Later, in both his *Leonardo* and in his *Autobiography*, he added the corollary that the re-creative journey of the historian is far more intricate than it is naively assumed to be. These references are invoked as auspices for this review of a psychoanalytic history that has a special interest for the psychological laity.

The book edited by Ives Hendrick contains the papers presented at the twenty-fifth anniversary celebration of the Boston Psychoanalytic Institute on November 30, 1958. In addition to the major chapter by the editor, which bears the title of the book itself, there are two full-length contributions by Bertram Lewin and Karl Menninger. Lewin provides a general background concerning psychoanalytic education in Europe and America; Menninger, under the heading "Footprints," informally reminisces and looks ahead. But it is Hendrick who

gives the book its essential character when, with the missionary sense of a Geoffrey of Monmouth, he recounts and documents the New England history. One is tempted, even at the risk of seeming to invoke a heterodox theory, to re-title the volume "The Trauma of the Birth of an Institute" since in the main it chronicles the Herculean travails of the Boston Society in a mortal struggle with the dragon of lay analysis.

This is the heyday of psychoanalytic anniversaries in America. Scarcely had the centenary of Freud's birth been commemorated in 1956 than a spate of celebrations occurred in Boston, New York and elsewhere, followed by various publications giving permanence to the passing event. The annual meeting of the American Psychoanalytic Association in 1961 was dedicated to the fiftieth anniversary of its founding and the January 1962 issue of the Journal of that organization included the papers presented on that occasion. The longest article was "American Psychoanalytic Education: Historical Comment" by Bertram Lewin. In 1960 this same writer, in collaboration with Helen Ross, produced the definitive book of nearly 500 pages entitled *Psychoanalytic Education in the United States*, remarkable for the fact that in it a total of one page is devoted to lay analysis. Mention is made of Freud's *The Problem of Lay Analysis* (1926) and its repudiation in New York. "The Chairman of the Comitia Minora of the New York County Medical Society informed the New York Psychoanalytic Society that the Medical Society disapproved of non-medical psychoanalytic practice and of the training of non-medical persons as psychoanalysts. The New York Society adopted a constitutional provision requiring membership in the County Medical as a prerequisite for membership in the New York Psychoanalytic Society" (p. 33). Then there is *Fruition of an Idea: Fifty Years of Psychoanalysis in New York*, Martin Wanh, M.D., Editor



(1962), which roughly corresponds to the Boston volume edited by Hendrick. In it Lewin, the ubiquitous dean, pays particular tribute to A. A. Brill, the senior U. S. Freudian and Freud's American translator (who is now being systematically displaced by the English translator James Strachey). In conclusion it is fitting to mention a volume of broader scope, *Psychoanalytic Education*, edited by Jules H. Masserman (1962). Its breadth is, however, somewhat defeated by its formal organization, with interesting chapters, that have little, if anything, to do with education or training as such.

TO APPRECIATE the events involved in Hendrick's history in the years on either side of 1930 it is important to recognize Freud's persistent and unqualified stand in favor of lay analysis. According to Ernest Jones, this posture was part of Freud's aim to see psychoanalysis become an independent profession and not a mere ancillary of medicine or psychiatry. Hendrick all but ignores this fact. (In social action silence is the language of repression.) However, in his presidential address of 1955, under the title "Professional Standards of the American Psychoanalytic Association," he zealously recalled the conquest of the "wild analysis" (mostly lay) of the twenties. (See *J. Amer. Psychoanal. Assoc.*, 1955, 3, 561-599, esp. 564.) Jones has devoted a full chapter and other portions of his Freud biography (Vol. III, Ch. 9, and *passim*) to the contention over lay analysis, involving particularly the American group. In 1926 Freud published his dialogue devoted to this issue. The occasion was a suit brought against Theodor Reik in Vienna on grounds of malpractice. Largely through Freud's intervention the charge was dropped. But it is worth noting, especially since Jones and others have failed to do so, that the germ of Freud's position on lay analysis was already in evidence in 1909, at the time of his only visit to the United States. In opening his first lecture at Clark University he digressed to say: "But first just one word. I have learned, not without satisfaction, that the majority of my listeners do not belong to the medical profession. Do not in any way fear that a medical education is necessary to follow what I shall have to say. We shall

now accompany the physicians a little way, but soon we shall take leave of them and go with Dr. Breuer on a way that is quite his own." (Translated from the German by the reviewer.) Of like interest is the status of the argument that Freud naturally rejected the medical profession since medicine had rejected him in the early days of his discoveries. (Should it not be axiomatic that *logical* explanations are powerless to cope with Freud's basic attitudes and behavior?) Hendrick in his popular text *Facts and Theories of Psychoanalysis* (1958, p. 355) perpetuates this oft repeated rationalization when in the context of medical versus lay analysis he states: "In Vienna and elsewhere he [Freud] had encountered a wall of medical hostility, especially from the universities." But Jones (*ibid.*) goes flatly on record as not subscribing to this explanation and adopts instead the above mentioned view that Freud's position on training stemmed from his goal to establish psychoanalysis is an autonomous discipline.

The problem of lay analysis was by no means settled by the decision in Reik's favor or by Freud's strong stand on the matter in his book. In the years that followed, the topic recurred with heat on both sides. Important developments occurred in New York (as above), in London ("the psychoanalytic charter" procured by Jones, 1926-29), and at Innsbruck (at the 1927 International Congress of Psychoanalysis). In all this commotion Jones played a part that, according to his own statements, was highly suspect to Freud; and the role of Ferenczi, completely on Freud's side, was complex and involved. But Jones pleads innocence of all charges, confesses his proneness to "midway positions," and points out that the British Society, under his leadership, has always had a liberal attitude on lay analysis. Individuals in less exalted positions than Freud's, says he, had to make concessions to professional and legal considerations with which Freud could afford to be impatient. These considerations were nowhere better exemplified than in the formative years of the Boston Institute described in our book.

The section by Hendrick is the *pièce*

*de resistance* and will be emphasized here. It is a remarkable document, a labor of Eros, supported by quadruped emendations. Footnotes at the bottom of the page are supplemented by three sets of appended notes and references to both published and unpublished materials. The form alone would make it clear to a psychodiagnostic eye that the writer has a special libidinal investment in this production and the content fully bears out this surmise. As one reads and rereads—a necessity in order to find one's way in this tangle of detail—one comes to agree with the writer's description that the status of the Boston Society was "confused" in 1932. This particular confusion was not fully dispelled until November-December 1933, when that organization reconstituted itself and was accepted by the American Psychoanalytic Association—the date celebrated as the twenty-fifth anniversary of the Institute (pp. 38-39). Lay analysis was responsible for all this hubbub. The pattern had been forged in 1922 by the American Psychoanalytic Association and in 1926 by the New York Society, both of which had outlawed lay analysis. But Freud's exactly opposite stand kept the issue alive. When in May 1932 Franz Alexander was about to leave Boston, where he had served as chief psychoanalytic educator for some months, and the prospect of his being replaced by one of Freud's close disciples Hans Sachs was being digested, the problem flared up. For Sachs was a lay analyst, with Freud's full sanction. In the ensuing months the battle raged above and below ground. A dramatic moment was the resignation of M. Ralph Kaufman and Ives Hendrick in May 1932 from the Boston Society. "But that was not to be for long," we are told, "and they again became active in the Society the following year." The history leaves no doubt that these two youthful champions of the New York position were the Boston standard-bearers to whom any available honor is due. It is noteworthy that while Hendrick took responsibility for introducing and editing the present book and wrote most of it, the toastmaster of the anniversary evening program was Dr. Kaufman.

To psychologists it is of special interest that at this juncture Henry Mur-



ray played an active part in the doings of the Boston group. Not only was he one of the ten charter members at the eventful end of '33 (p. 39n.) but in the period 1932-34 he served as Chairman of the Society's Education Committee (p. 42) and made available the quarters of the Harvard Psychological Clinic in Cambridge for some of the training seminar sessions. Says Hendrick: "Henry Murray . . . now looks back on these meetings . . . as 'Case Presentations Plus'! by Hendrick, Kaufman, Herman, himself, and also by his lay associate Mrs. Christiana Morgan . . ." (p. 43). Of like interest is the advent of Erik Ericson, a non-medical child analyst, trained by Anna Freud, who came to the Boston area in 1934 for about two years (p. 55) and has, despite his limited credentials, achieved an eminent place in American psychoanalysis.

Throughout these stormy times Isador Coriat, Boston's opposite number to Brill of New York, was buffeted in a vain struggle to maintain an untenable position. In two trenchant pages (46-47) Hendrick summarizes the lifework of this man who, President of the American Psychoanalytic Association in 1925 and successor of James Jackson Putnam as President of the Boston Society 1928-32, was for years "the single qualified apostle of Freud" at the Hub. The moment of triumph for the vanguard over Coriat is recounted by Hendrick in historic and geographic detail: on the stairway to the mezzanine of Boston's Hotel Statler, at the meeting of the American Psychiatric Association in 1933, Coriat and Hendrick, who had for three years been at odds, met. "Coriat pulled me aside and gazed at me squarely. 'I want you to know,' he said, '*whatever is good for psychoanalysis I will support!*'" (The italics are part of the quotation.) It is characteristic of this inimitable history that a footnote follows, at the bottom of the page, reading: "Dr. Coriat, who had no children, had also experienced the personal loss of his devoted wife a few weeks previously." Rarely has a dynamic gloss been so doubly informative.

THE Constitution of the Boston Society, adopted on March 22, 1932, in-

cluded two features that Hendrick lauds. One of these is the "faculty principle," the other, the "pledge of good faith." The pledge required the training candidate to acknowledge in writing that he would not represent himself as a psychoanalyst without permission of the Education Committee. The faculty principle placed psychoanalytic education exclusively under the Education Committee and denied "any individual member" the right to exercise authority without such Committee approval. Hendrick compares this principle to certain clauses of the Constitution of the United States which also illustrate how "an abstract principle actually determines future events and mores." The United States aside, it is evident that for the Boston Society these two principles were the basis on which the militant group consolidated its authority and exercised its will against Freud himself. One might be tempted to invoke the oedipus complex to account for this symbolic slaying of the father (along with the dragon) were it not for the obvious fact that the sons were here behaving far less by Freudian than by proto-Marxian principles.

The story of Martin Peck is that of a conciliator who may have had some difficulty in reconciling himself to his role. Peck was Coriat's successor as President of the Boston Society and served from 1932 to 1937. He was willing to accept the office, according to Hendrick, "only if it 'served a conciliatory purpose'" and he is described as "the most truly kind and friendly man in the history of our Society." In view of the fact that in the spring of 1932 the possibility of appealing to Freud for a decision when "tension and factional cleavage rapidly intensified" was briefly considered—to which Hendrick adds (p. 32): "it was well this complication was not invited"—it is of interest that in 1937, at the close of his presidential service, Peck arranged to see Freud in Grinzing and published the interview. This short paper (unmentioned by Hendrick) is entitled "A Brief Visit with Freud" (*Psychoanal. Quart.*, 1040, 9, 205-206). It rounds out the history of the Boston Institute with a vignette of Freud the man that it is hard to match for vividness and cogency.

Two points from this article are particularly relevant here. The first is put by Peck in these words: "The essence of his [Freud's] comment was that in America medical application of psychoanalysis was the rule, and contributions to its structure were the exception. He used the term 'medical fixation' for the American scene, and regretted the alliance between psychiatry and psychoanalysis. He made frequent reference to the 'core of psychoanalysis' and expressed his belief that this core should be kept separate from other disciplines for a long time to come. He stated with deep conviction that 'psychoanalysis is part of psychology and for its proper development, it should be kept free from biology, philosophy,—and also psychiatry.'" It is in this light remarkable that Hendrick in the section of his monograph headed "Achievements" not only takes stock again of the great advantages that have accrued from the "faculty principle" and the "pledge of good faith," but takes equal, if not greater, pride in the absence of dissident factions. He writes (p. 64): "It is also a fact that there have been among the psychoanalysts in Boston no coiners of new technical vocabularies, that unfailing pathognomonic symptom of every new-school founding deviate in psychoanalytic history." The second point made by Freud is essentially a commentary on this boast. After Peck had stated the American view that the time had come for closer cooperation with medicine, Freud replied "that there was implicit in this argument a false assumption that the validity of psychoanalytic findings and theories was definitely established, while actually they were still in their beginning, and needed a great deal of development and repeated verification and confirmation." When Peck stated in conclusion that he would take pleasure in presenting to his colleagues the substance of the interview, "Freud said most amiably that it would be a very nice thing to do, but that it would accomplish no good whatever" (p. 206).

ONE practical upshot of the events exemplified in the birth of the Boston Institute is evident in certain of the figures cited by Bertram Lewin in his



chapter of the present book. He states (p. 109) that in 1958 there were in the United States 888 acknowledged students of psychoanalysis of whom only 15 (or 1.7%) did not have medical degrees. These exceptions were "research or special students in non-medical professions." In official Freudian circles the judgment of Sigmund Freud has been overwhelmingly overridden. An international exception is, of course, Freud's daughter Anna, a lay analyst in London. (Jones credits her with having "saved the situation" at Innsbruck in 1927 and thus prevented a fatal breach between European and American analysts over lay analysis.) She was invited by the Boston Society to be a guest of honor at its twenty-fifth anniversary celebration but gracefully declined in a brief congratulatory message.

And so this faithful chronicle exhibits Boston's Institute of Psychoanalysis as

well established at 15 Commonwealth Avenue, within walking distance of Cornhill which, we are told by Hawthorne, once knew the footsteps of Anne Hutchinson. By an inversion of St. Peter's paradox the stone that the master rejected has become the head of the corner. Hendrick's meticulous account of the Institute's birth and first actions will greatly aid future historians. To this text may be added one bit of context. In 1911 (the year in which Ernest Jones organized the American Psychoanalytic Association) Freud published a book review entitled "Great is Diana of the Ephesians." (These words are from an experience of St. Paul reported in *Acts* 19.) He used excerpts from the recent monograph by Sartiaux on extinct cities and religious cults to show that one church builds on the ruins of another and perpetuates the old techniques in new form.

doing testing by mail. He is likely to be confused when he finds his own metalanguage invaded and many of his carefully qualified terms, bandied by the author in cavalier and dogmatic fashion.

What is the mission of the book? It seems to be to set the American public straight on that dreadful evil, testing. The indictments are many. First, with regard to the tests themselves, it is alleged that few are reliable and none valid. Second, the testers have become all-powerful Machiavellian wielders of influence, changing men's lives and firms' destinies. Third, the influence of the testers is all-pervasive. Their machinations are taking the creativity out of Madison Avenue, the ingenuity out of our schools, and entrepreneurial aspirations out of our salesmen. Fourth, as a culminating indictment, tests are seen as an invasion of privacy.

The book places in public purview some truly serious issues with regard to tests, ones which psychologists have been grappling with for a long time. Certainly the empirical evidence on validity of inventory-type personality and motivational measures is controversial at best. Few if any of them have a predictive preciseness that warrants their use in definitive selection decisions. Another unresolved problem is that of keeping tests out of the public domain. Many widely-used measures are readily available to the layman. Indeed paperbacks containing scoring keys of some tests are available at newsstands. Still another issue stems from man's unending quest for a simple way of understanding himself better. This has provided an almost limitless market for the test charlatan. Another problem, only too well known to psychologists, is that test results are certainly vulnerable to misinterpretation. The Gross book contains some horrible examples of this.

YET another problem, related more to testers than to tests, is that of dual allegiance. For example, if the psychologist is retained to screen candidates for a prospective employer or to assist a corporate client in upgrading employees, he must uphold the best interests of the business which he serves. On

## Who's Watching the Watcher of Watchers?

Martin L. Gross

*The Brain Watchers.* New York: Random House, 1962. Pp. 287. \$4.95.

Reviewed by W. J. E. CRISSY

The author, Martin L. Gross, is a journalist who graduated from the City College of New York and who has worked as reporter and later as editor on several weekly newspapers in and around New York City. In recent years he has published in national magazines a number of commentaries on health, education and other assorted national affairs. This is his first book. The reviewer, W. J. E. Crissy, completed his graduate training at the University of North Carolina then sailed off for three years of WW II duty in the Navy. Since then he has taught at Queens College, served as President of Personnel Development, Inc., and has functioned since 1960 as professor in the Graduate School of Business Administration at Michigan State University. Long a

member of the Psychometric Society and once a reviewer for *The Mental Measurement Yearbook*, he still retains interest in problems of testing.

THIS is a book about tests, testing and testers. In content and organization it is more of a mishmash than a potpourri. It certainly is neither a systematic treatise, text book, nor research work on the subject. The viewpoint can be inferred from the very first sentence, "This book is about brain watching, the art, its practitioners, and its subjects, some 50,000,000 hapless Americans." The professional practitioner engaged in personal evaluation is going to be irritated to find himself indiscriminately grouped with proprietary firms



the other hand, he has an ethical as well as a professional obligation to try to help the applicant or the employee involved. Should he become privy to personal information that might from a managerial viewpoint eliminate considering an applicant further or cause serious doubts about an employee's promotability, he must determine whether or not it was provided on a confidential basis by the person involved. The stickiness of this issue is reflected in part by the treatment of it in the A.P.A. *Code of Ethics* which, incidentally, is not referred to in the book.

The psychologist interested in personality dynamics might, in reading the book, find himself free associating about the adjustment mechanisms of the author. Did he have a traumatic experience with tests? A colleague whose interest is in written communications might want to make a content analysis to test the hypothesis that the less you know about a subject, the more certainty you can have in writing about it. Psychologists in schools, government, and industry who are concerned with the use of tests in selection and upgrading are likely to have a stronger drive to counteractions. As they read the book, they must formulate rejoinders to the questions the book is bound to raise in the minds of the clients whom they serve.

What will be the impact of the book on the general public? It is certainly likely to raise questions about the utility and value of tests. Some people may without further evidence discontinue testing. More sophisticated readers are likely to discern an axe-grinding in the diatribe. Perhaps in the long run the very attention focused on testing may be beneficial.



*With the best will in the world, we cannot always be completely truthful or consistently rational. All that is in our power is to be as truthful and rational as circumstances permit us to be, and to respond as well as we can to the limited truth and imperfect reasonings offered for our consideration by others.*

—ALDOUS HUXLEY



## A Decade of Rogers

Carl R. Rogers

*On Becoming a Person: A Therapist's View of Psychotherapy.* Boston: Houghton Mifflin, 1961. Pp. xii + 420. \$4.75.

Reviewed by ROBERT W. LEEPER

*The work of the author, Carl Rogers, is very well known to most psychologists, but perhaps not so well known are the biographical facts that he received his PhD from Columbia, was associated for a number of years with the Society for Prevention of Cruelty to Children in Rochester, New York, and was later Director of the Rochester Guidance Center. After teaching for some years at Ohio State University and at the University of Chicago he moved to the University of Wisconsin where he is Professor in the Departments of Psychology and Psychiatry. The reviewer, Robert Leeper, is Professor and Chairman of the Department of Psychology at the University of Oregon. His rich and varied history has seen him range geographically from Clark University, where he took his PhD, to Georgia's Paine College, to the University of Arkansas, to Cornell College and to the University of Aberdeen. Intellectually he has ranged from experimental psychology to cognitive theory to personality. Many of CP's readers know his 1959 book, written with Peter Madison, *Toward Understanding Human Personalities* (CP, Nov. 1960, 5, 353; Nov. 1961, 6, 414). His most recent writing has been a chapter for Volume 5 of the Koch Series *Psychology: A Study of a Science* and consists of a "reward-emphasizing, perceptual motivational theory" of learning that combines ideas from Köhler, Tolman, Lashley, Woodworth, Osgood, Lorenz, Lewin, Skinner and Guthrie. He probably would like it reported here that he is not responsible for the long delay between the appear-*

*ance of Rogers' book and the publishing of this review. It is so reported, and truthfully.*

PEOPLE have learned to look to subtitles for relatively informative even if relatively prosaic indications of the real nature of books. However, the rule does not apply here. Mostly this book is not "a therapist's view of psychotherapy," but a therapist's view of personality problems in general, with psychotherapy a special case within this larger and (even to Rogers) more interesting set of problems.

The book is a collection of papers, most of them not readily available elsewhere, written by Rogers over the last decade. Reading these papers (especially chapters 1, 5, 8-10, 13, and 16) is like sharing a highly personal document. Most of them were delivered as talks to special groups, but all of them are papers Rogers might have written for himself in an effort to think out, as fully and honestly as he could, the implications of his experience as a therapist and of his and the group's research on therapy.

To get the best picture of Rogers' recent thinking, this book ought to be combined with Rogers' chapter in Vol. 3 (1959) of Koch's *Psychology: a Study of a Science*. The present book is the more satisfactory of the two in many ways, with more of the flesh and blood of Rogers' work in it, whereas the Koch chapter is more the pedantic skeleton; but each has its place. Two other recent publications might well also be mentioned here; both are so



much in keeping with two phases of Rogers' position that, given a few other genes, he might well have written either of them—one the poignantly beautiful little volume, *Loneliness*, by Clark Moustakas, the other the superb systematic paper, "The Image of Man," which Isidor Chein gave as the last SPSSI presidential address (*J. Soc. Issues*, Oct. 1962). Had Chein's paper been available at the time, it would have been extremely fitting if it could have been printed in Rogers' book to round out some of its more theoretically oriented parts.

In these papers, Rogers has not tried to relate his ideas to concepts from the rest of psychology. In fact, in his preface, Rogers says that other psychologists typically seem so convinced that psychology's task is to learn how to predict and control the behavior of others that he feels a basic incompatibility between such thought and his own.

Probably it is fortunate that Rogers has been so independent of the work of most other psychologists. In science, as in everyday life, our explorations tend to stay within the limits set by presuppositions which, at some earlier point, we decided we would have to make if we were to explore effectively those problems where important uncertainties still prevailed. A main virtue of Rogers' work is that he has been determined to escape the usual presuppositions of psychology and of our background culture and that he has developed a type of work which has helped him to look at human life anew.

**E**VEN if Rogers has been wise to stake out his own territory and work it so independently, what seems important for the majority of us is to try to inter-relate his main ideas with our more usual psychological concepts. There are important parallels. For example, some of Rogers' main concepts can be seen as hypotheses about motivation (virtually, that three main motives in human life are the cravings to be understood, to be accepted, and to actualize one's potentialities). Other main discussions might be seen as dealing with techniques of interpersonal

relationships (e.g., that a main technique we tend to use is one of *evaluative* reactions to whatever is said or done by others, but that the better tactic would be the fundamentally different one of trying to 'understand' the other person (or group), in line with the importance of the craving of such others to be understood in a deep and empathic way). On the score of means of handling intrapersonal problems, Rogers is suggesting that a strategy of paramount value is that of learning to be sensitive to and accepting of those processes within ourselves which we tend to disavow, but which still are part of our full though not necessarily conscious experience, and which we must recognize and deal with if we are to achieve some real congruence and harmony within ourselves.

Admittedly such concepts deal with more subtle variables than academic psychologists generally have been willing to study or discuss. But, the development of any science moves from merely tangible variables more and more to the inclusion of those relatively intangible factors that could not be appreciated nor even discerned at first. Rogers therefore has important contributions for psychology.

There are some confusions and contradictions, however, which a more extensive interaction with other psychologists might have corrected. Thus, in different contexts Rogers uses one of his most central terms (the "self") in two fundamentally different senses. Talking about his research, Rogers says (p. 245) that the construct, "self," had to be defined as the individual's *conscious* perception of his own nature—otherwise there could have been no operational definition adapted for research needs. But, when Rogers is speaking more generally about personality and therapy, the same term is used in a basically different sense. Thus, in one chapter (8), which seems especially close to Rogers' heart, he suggests that the most adequate statement a person might have of the goal in life is "to be the self that one truly is." And, as Rogers makes abundantly clear over and over, the common type of statement from clients is such as these: "I can't understand myself any more,"

"I'm frightened by what I am finding in myself," and "I'm finding that what I thought was myself isn't really me—what am I?"

There are other complex conceptualizations, too, where Rogers' theory needs more adequate and consistent development. To be helped in this, Rogers' theory needs the rest of psychology, and the rest of psychology needs it. Here is one more area where some real efforts at integration are in order!

## Czechoslovak Applications

Veronika Kovaliková (Ed.)

*Využitie Psychologie v Socialistickej Spoločenskej Praxi (Application of Psychology in the Socialist Social Practice)*. Bratislava: Slovak Academy of Sciences, Section of Social Sciences, Psychological Laboratory, 1959. Pp. 408.

Reviewed by Jiří KOLAJA

V. Kovaliková, who edits the present volume, is associated with the Psychological Laboratory of the Slovak Academy of Science in Bratislava, Czechoslovakia. The reviewer, Jiří Kolaja, is a native of Czechoslovakia who attended the Masaryk (now Purkyně) University in Brno, Moravia and who subsequently came to the United States to attend Cornell University, receiving there the PhD degree in Sociology. He is now a member of the Department of Sociology at the University of Kentucky. His book *A Polish Factory: A Case Study of Workers' Participation in Decision Making* has been published recently and he has just finished a manuscript on the Yugoslav Workers' Council, based on field work in two Belgrade factories during the summer of 1959. The review was arranged for and edited by Josef Brožek, CP's consultant on Slavic books.

**T**HERE are 69 papers and one resolution in this volume; 16 of the papers are empirical research reports from the



fields of industrial, educational and medical (clinical) psychology. Most of the papers, some of them rather short, are concerned with the desired role of psychologists and psychology in a socialist (communist) society, with occasional references to the present situation.

The papers were presented at the First All-State (National) Conference of the Czechoslovak Psychologists, held in November 1957. The fact that such a conference was convened may be interpreted as a sure sign that psychology in Czechoslovakia has gained recognition, at least provisionally, from the Party and State executives. Nonetheless, the reader gets the impression of tense inquietude as some papers too eagerly emphasize the applicability and "practicality" of psychology.

In the section on the psychology of work, introduced by the major paper presented by J. Doležal of Prague, one reads about a program to explore interpersonal relations within work groups (p. 35) with no reference made to the work of American sociologists and social psychologists. On the other hand, most papers make a reference to Pavlov. W. C. Menninger is the only American who gets conspicuous recognition, though psychoanalysis as such is not recognized.

In the section on educational psychology, A. Jurovský from the University of Bratislava makes a plea for research on values and aspirations of the average citizen in order to establish an efficient adult education program and mental health consultation (p. 142 and ff.). One of the most interesting papers in this section, based upon a study of 180 children between 6 and 15 years of age, proposes that the differentiation of stimuli containing new signal meanings proceeds in jumps, not gradually, with increasing age as proposed by Kelley. The author, Karel Triska, stresses that this corroborates the soundness of the dialectical theory and invalidates the continuum concept as developed especially by Americans (pp. 198-200).

The section on medical psychology, introduced by R. Konečný of the University of Brno, contains a series of concrete proposals including one which specifies that in each mental institu-

tion there should be one psychologist per 50 patients.

The concluding resolution states that many contemporary difficulties in the construction of the socialist society are due to the underestimation of "subjective-psychological factors." It also stressed that psychology should be taught on the secondary school level. A strong plea is made for mental hygiene counseling, needed because of the rapid changes in many social institutions within the socialist society.

THE volume provides excellent material for study of the processes of professionalization of psychologists in Czechoslovakia. Within the context of the Marxist ideology that underplays

the significance of "subjective" mental processes, psychologists have not had a sympathetic climate for their work. In a paper written by Doležal, the subservient position of psychology is stressed: psychology is here only to assist and help other major sciences such as economics, an opinion probably not shared by the majority of participants of the Conference. Though containing little new as far as the psychological theory is concerned, the volume as a whole shows an increasing concern and growing need for the introduction or re-introduction of research-oriented social sciences such as psychology and sociology within Czechoslovakia and other Soviet-orbit countries.

(This review was prepared in the frame of activities supported by the National Science Foundation grant G19469, awarded to Dr. Josef Brozek.)

## End of Innocence

William Golding

*Lord of the Flies*. New York: Capricorn Books, G. P. Putnam Sons, 1959. Pp. 265. \$1.25.

Reviewed by WARREN G. BENNIS

The author, William Golding, was until recently a teacher in a British secondary school but now devotes himself full-time to writing. He did his undergraduate work in the classics and has written three other novels. The reviewer, Warren G. Bennis, has returned since CP last wrote about him (December, 1962) from a year in Switzerland to his professorship in MIT's School of Industrial Management. He is co-author (with Benne & Chin) of *The Planning of Change* (CP, May, 1963, 8, 214).

"IN our innermost soul we are children," Freud once said, "and remain so throughout our lives." This may partially explain the haunting endurance of children's novels—or more precisely those books written for or about the young, who are themselves often unable to reach the shelves adults place the books upon. American literature is

rich in this mythic prose: *Huckleberry Finn*, *Two Years Before the Mast*, *The Leather Stocking Tales* and *Catcher in the Rye* all qualify for this honor and their archetypal heroes depict childhood as the primitive drama of human experience.

Now from England comes—I should say "explodes"—*Lord of the Flies*, a book already something of a publishing and literary sensation since its inconspicuous release in this country in 1955. After a quiet start—no promotion or critical notice to speak of—it has snowballed in the last year or so into a collegiate best-seller; according to *Time* it has replaced *Catcher in the Rye* as the poetic touchstone of the college and secondary student. A British film studio has just completed a movie version in time for the 1963 Cannes festival. Perhaps more significant is the fact that the publishers have decided to re-release the book in hard cover,



after its paperback boom, to satisfy the readers who demand a permanent copy. Lionel Trilling has said of it that "it seems to have captivated the imagination of a whole generation."

The success of *Lord of the Flies* is peculiarly interesting. If we compare it to the usual fare of children's stories—with their emphasis on innocence, antics and romance, with their nerve-racking sequence of adventure-terror-escape-redemption, with their cotton-candy finales: boys tucked into bed "tired but happy" by clucking parents—then its meteoric popularity is remarkable. For *Lord of the Flies* draws its distinguished power from a hard, crude, unrelenting horror. It is painful reading.

On a South Sea Island a group of English schoolboys are stranded during an atomic war. Maintenance and survival of the group devolve upon an elected leader, Ralph, and his fat, asthmatic, intelligent lieutenant, Piggy. A fire burns as a rescue signal, rules evolve to preserve order, roles emerge to the demands of survival. A strained social structure develops. The novel tells of the progressive disintegration and fission of the group, the degeneration of the individual, and the spastic shedding of the layers of civilization. In a chilling and crazed ritual dance the visionary is murdered. The intellect is stalked and finally killed. Law is waiting in terror for certain death as a naval officer appears to "rescue" the boys. He comments, "Jolly good show, like the *Coral Island*."

This is the story, baldly summarized. And I should make clear that this is not "real" fiction, anymore than the boys or the naval officer are "real" characters. Golding has aimed at something more general, more abstract, more stark: a modern fable.

FABLES differ from ordinary fiction in a number of ways, primarily because it is their function to draw a conclusion.<sup>1</sup> They must be highly contrived. The author reduces complexity and uniqueness in the service of generality, using an abstract and skeletal plot. (Compare Kafka to Dostoevsky, Orwell to Dickens.) Fable has something

in common with controlled experiment where outcomes are predictable and anterior. Fiction, on the other hand, is more like a clinical study where truth emerges uniquely and surprisingly—perhaps in an unrecognized and latent form—to the participants engaged in the search.

If we view the *Lord of the Flies* as fable we can account for the author's pneumatic intensity and for its phenomenal impact. For fable is nothing if not a moral tool, and the morality is stark: that dark irrational forces of evil are in the nature of the individual; that political and social systems do not corrupt, they can only maintain order and security by the renunciation of instinct. This ancient moral is refueled by an irony which the author modulates to expand or contract horror. Irony breeds and plays in the crevice between appearances and reality (as when the naval officer recalls Ballantyne's *Coral Island*).

Nor, again, do the characters have that complexity that lends to the "reality" of fiction. None of the boys is "whole" and all seem to be lacking in some important attribute. Intelligence, bravery, vision, humor, ability, are so distributed that we are never quite sure if we are observing an allegory of the disparate parts of the personality or the disorganization of a community; the boys act as the modal impulses of a personality, or of a group, and one is uncertain which focus is most appropriate.

MY enthusiasm must be tempered to allow a future reader the pleasure and horror of encountering the book's surprises for himself. But I hope it is clear that this book contains many riches for the psychologist (and his students). Many of the sub-themes threaded throughout parallel standard chapters in a social psychology text. Group effectiveness, leadership, formation of norms, communication, social perception, functions of role, intergroup relations and more are reduced and scrutinized through Golding's allegorical bifocals.

Perhaps one example of his convincing skill in shaping symbols to behavior

is sufficient. The boys believe that a strange predatory beast is at large on the island. Their suspicions are confirmed when a dead parachutist floats onto the island and is propped up for all to see between the branches of a tree. They mistake this corpse for the beast and project their own evil vision on to this distended reality, the swinging, swaying dead parachutist. Anxiety is reified so that its evil can be extirpated. Only one boy senses the Truth in the beast's identity: "... maybe its only us." Only this boy looks inward, and he is killed.

I often wonder about the impact of this moral fury on our reading public. Is it possible that in less than one literary generation Holden Caulfield and his archangel siblings of the Glass family have fallen from grace? And are replaced by Golding's savage and "delinquent" boy-warriors? Or do these latter foreshadow an important redress of balance and a literary end to the mythic innocence and Rousseauian romanticism so dominant in children's stories? Perhaps these are questions for literary historians.

But one challenge psychologists cannot avoid involves the appropriateness of our slant on the etiology and cure of social and mental disorder, most particularly, juvenile delinquency. "Basically, of course, it was (his) mother who was at fault; but the precipitating factor came through his immediate community."<sup>2</sup> This thesis victimizes society in the eyes of the child and fashionably forgets Golding's cruel reminder, "the end of innocence, the darkness of man's heart."

#### REFERENCES

1. PETER, JOHN, The fables of William Golding, *Kenyon Review*, XIX, 1957, pp. 577-592.
2. ABRAHAMSEN, DAVID, *Who Are The Guilty*, New York; Grove Press, Evergreen edition, 1958, p. 15.



Wisdom consists not so much in knowing what to do in the ultimate as in knowing what to do next.

—HERBERT HOOVER







## MORE AND MORE BOOKS

EACH year each of the dozen editors of APA's dozen journals prepares a report for his eleven fellows who, very probably, read it, and who then, as the Council of Editors, meet to discuss, console, consider and recommend. The same report, along with the recommendations of the Council of Editors, goes to members of the Publications Board who meet to discuss, consider, to formulate wisdom and to move inexorably toward action. *CP's* 1962 operations were described in its official report, rendered in March 1963. *CP's* readers may find items of interest in the following excerpts from the official screed.

As far as is known to anyone in its editorial office, *CP* came off reasonably well in 1962. There were a number of minor troubles with the printers, a number of errors, mostly minor, in the editorial functions, a number of evidences of human fallibility among consultants, reviewers, authors, readers and editors. But many books were processed and a record number of reviews appeared in twelve on-time issues. While there is no available evidence on how many people actually read the journal with what amount of pleasure or profit, we do know that its subscription list did not evaporate.

There continues to be an increase in the annual number of books flowing into *CP's* maw. This increase leads on, with some naturalness, to increases in both the number of books listed and the number reviewed; and these increases in turn inevitably demand additional work from most of those involved in the production of the journal. So far, increases in the flow of books and reviews have been matched by gains in the ef-

ficiency with which *CP's* females do their jobs. So production rolls along. It will probably roll along without significant difficulty during 1963. After that, predictions cannot be so confident.

So all of *CP's* problems seem to be large ones. There are problems, for example, concerning the general utility of the enterprise, problems of the proper and potential role of *CP* in the whole of American Psychology, problems arising from the fact that many more psychological books of a sociotropic than of a biotropic slant appear every year. These larger matters may be worth discussion by the Publication Board. But before such discussion can proceed with greatest effectiveness, many of the issues will probably need intensive staff study.

The established consultative proce-

dures, in the eyes of the editorial staff, continue to work well. Our 31 consultants (named on the inside front cover) not only do wondrously well at giving good advice but also are often likely to handle their chores with a flair rising far above the grimnesses of duty to bring surprise and pleasure into activities that might easily bore any but the very bright.

In addition to its regular stable of consultants, including three new ones added in 1962 (Nicholas Hobbs, Frederick Wyatt and James Birren), *CP* has meted out work to a number of friends, hopefully without making enemies of any of them. The list of those who, with good cheer and great utility, have given their time and talents to the enterprise include:

Edith Annin  
E. G. Boring  
Leo Borstelmann  
E. J. Capaldi  
Ira Hirsh  
Robert Lindsay  
Francis Palmer  
Donald Taylor  
Robert Young

Table 1 presents data on the flow of books from publishers through the *CP* office and eventually, into reviews. The

TABLE 1  
Books Received, Listed, Reviewed

	1960	1961	1962
Books Received	592	669	764
Listed	512	595	630
Received, Not Listed			
Number	80	74	134
Per cent	14%	11%	18%
Listed, Not Reviewed			
Number	303	226	201
Per cent	51%	38%	31%
Books Reviewed or in Process <sup>1</sup>		369	429
Number of Reviews Published <sup>2</sup>	211	212	251
Number of Books with Published Reviews <sup>3</sup>	242	277	320

<sup>1</sup> Figures 1961 and 1962 include paperbacks. Figures for 1960 naturally not available.

<sup>2</sup> Figures include reviews in which multiple books were treated.

<sup>3</sup> Figures include paperbacks as well as other books reviewed in batches. The books involved here are not the same books that are counted under "books received" or "books listed" in the calendar years involved; of the 320 books treated in 1962 reviews, for example, a majority were received and processed in 1961.



data speak for themselves and say, in epitome, "increase." Compared with 1961 and earlier years, more books are coming to *CP*, and of these that do come, a larger proportion are reviewed.

This increase in the flow of books into *CP*'s channels presents, naturally, an annual problem. Many aspects of the operation would be simpler if there could be a constancy of this flow. While the workload changes gradually the work force at some point has to increase by discrete and relatively large units. So work and workers rarely come out even, and a great deal of the time either the work suffers or the workers do. If the number of books continues to increase over the next few years, there must be decisions concerning such matters as the optimal size of the journal, new procedures for employing new standards of eligibility for review or, if the volume of reviews continues to increase, new editorial arrangements. *CP*'s present editor does not feel that any of these issues, or related ones, is now urgent. Nor are they likely to become suddenly and unexpectedly urgent. But urgency may be in the offing.

Not presented in Table 1 are some facts concerning reviewers. A total of 246 separate reviewers contributed to the preparation of the 251 reviews that were published. There were 241 individuals who prepared one review each. Two pairs of reviewers worked on 2 reviews, and one group of 4 produced a single review of 8 books.

In addition to the single review of 8 books (these were introductory texts, all praised faintly) there were 12 single reviews of 2 or more books. *CP* tends to feel that the single review of 2 or more books, if it is well done, is an arrangement that can simultaneously save space and add depth. To produce a good review of more than one book, however, requires a reviewer of unusual ability.

Table 3 [part of report but not presented here] shows changes over the past few years in the use of space within the journal. Again there seems to be nothing here guaranteed to jolt a Publications Board, except perhaps the 65 page increase, since 1960, in space devoted to reviews. There may be significance also in the fact that we are printing only about half as many pic-

tures as Garry Boring put in the journal. And there may be significance in the 5 page decline in index space; these are the 5 pages that were formerly devoted to the annual subject index. (The editor can report on the reasoning and the facts, such as they are, involved in the decision to eliminate this feature.) The very appreciable increase in advertising in recent years has obvious significance, both of a negotiable and non-negotiable

kind. But there seems to be no imminent trouble in any of these trends.

On the basis of *CP*'s felt needs and with some empathy with those who must worry about APA finances, *CP*'s editor asked for *only* a \$170 increase for 1964 in the budget for the editorial office and declared a resolve to operate the journal in 1964 with the same number of pages it will have in 1963.

## Crime and Punishment

Norman Johnston, Leonard Savitz and Marvin E. Wolfgang (Eds.)

*The Sociology of Punishment and Correction*. New York: Wiley, 1962.

Pp. v + 349. \$4.25 (paper) \$6.50 (cloth).

Reviewed by CARMİ HARARI

*Norman Johnston, Leonard Savitz and Marvin Wolfgang, editors of this volume, are all sociologists, are all located in the Philadelphia area, Johnston and Wolfgang at the University of Pennsylvania and Savitz at Temple University, and all have been intimately concerned for some years with matters of crime, punishment and correction. The reviewer of this book and of another to be listed below, is Carmi Harari, a clinical psychologist, psychotherapist, Director of Community Consultation Services, and a lecturer at New York's Postgraduate Center for Psychotherapy. For some time he served as Chief Psychologist and Research Consultant to New York City's Children's Court. He is co-author of New Patterns in Mental Health Services in a Children's Court. (1958).*

sections under such headings as The Administration of Justice, The Prison Community, Treatment, Prediction and Prevention provide a well chosen selection. Reports of experimental research on prediction and treatment of delinquency and criminality and the inclusion of some methodological controversy appears extremely desirable for both student and professional. It presents a balanced selection of brief articles which have appeared chiefly within the last ten years in journals covering sociology, criminology, police work and correction; it culls also from books, from institutional and government manuals and reports. There is a good contemporary ring to the problems taken up and the expected advantage of introducing students directly to valuable journal and documentary resources for a further elaboration of their own training and study.

THIS volume, a book of readings, is intended by its editors as either a text or a supplement in general undergraduate or graduate courses in Criminology. It appears a useful volume for the professional psychologist who is called upon in any way to relate himself to problems of criminality or juvenile delinquency. The fifty articles in five

The delineation of an argument around the prediction of delinquency based on the well known work of Eleanor and Sheldon Glueck, along with representative rebuttal, brings attention to basic problems in behavioral prediction from nonbehavioral data. Prediction and its methodology, sources of error and safeguards appears a very worthwhile area for inclusion inasmuch



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University of Washington.  
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By MELVIN H. MARX,  
University of Missouri;  
and WILLIAM A. HILLIX,  
Navy Electronics Laboratory,  
San Diego.  
McGraw-Hill Series in Psychology.  
Available in June, 1963.

The primary purpose of this book is to provide the advanced undergraduate and the beginning graduate student in psychology with a single, up-to-date source containing the basic information about systematic and theoretical problems in psychology. The approach is scientific rather than subjective or clinical. The authors provide not only the basic tenets of various classical and contemporary viewpoints in psychology but also a philosophical framework within which the tenets can be evaluated.

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as it carries the reader directly into problems of scientific research.

The volume is enriched by including some British work particularly on their Borstal system, an institutional form for the rehabilitation of young people which has recently received some attention in this country through cinema and stage.

A good opportunity is provided the reader to follow the type of research problem presented to working professionals in the course of their practical administrative, therapeutic or other work with criminals and delinquents. It emphasizes the need for research as an active social process which may suffer weaknesses and strengths inherent in the reality and significance of the problem they seek to investigate.

The value of this collection is greatly enhanced by inclusion in brief and readable form of not only the classic English McNaughten case which since 1843 has provided the definition of insanity in relation to responsibility in criminal law, but also of the 1954 case of Durham vs. U.S. which broadened the concept.

Prevention and treatment receive fully half the volume's space and touch on prediction of delinquency, prediction of treatment success in institutional and other supervisory programs, and group and individual therapeutic approaches. Reflections are seen of important revisions and adaptations of classic psychotherapeutic approaches to the needs of lower socioeconomic class individuals prone to behavioral acting out of impulses and little capacity for delay of gratification. It is a pleasure to see represented here among others such familiar names as Gresham Sykes, Lloyd McCorkle, Lloyd Ohlen, Marion Stranahan, Cecile Schwartzman, and Solomon Kobrin.

Gordon Trasler

*The Explanation of Criminality.*

New York: Humanities Press,  
1962. Pp. vii + 134. \$4.00.

*The author, Gordon Trasler, is a lecturer in Social Psychology at the Uni-*

*versity of Southampton. He received his PhD in 1955 from the University of London where his research dealt with children in foster care. He has worked for a while as a prison psychologist and his earlier publications include the 1960 book In Place of Parents. The reviewer, Carmi Harari, is introduced above.*

GORDON TRASLER undertakes to explain Criminality in 123 pages on the basis of Mowrer's learning theory and Eysenck's views. The laborious construction of a set of hypotheses to explain behavior in general or deviant behavior in particular on the basis of conditioning and reinforcement is not original. He discards as unverifiable and unscientific all clinical, psychiatric, psychoanalytic and even psychological approaches to the understanding and modification of behavior. Thus he dismisses the most fruitful leads of the past fifty or more years, leads that have resulted in vast gains in the understanding and modification of human behavior. The notion of unconscious motivation is rejected out of hand.

In seeking laboratory rigor in explaining the conditioning or learning of deviant social behavior he loses sight of the fact that frequently, families of the lower socioeconomic class apply sufficiently vigorous punishments or pain stimuli to serve as behavioral deterrents, but still fail in their efforts. In contrasting middle and lower class discipline and punishment of children he loses sight of the fact that the most punished lower class children may become the most persistent offenders. He appears unaware, for example, that deviant behavior is at times paradoxically linked with the unconscious need for punishment in order to relieve anxiety, stress and guilt.

Personality theory is viewed archaically with an emphasis on a trait psychology featuring the polarities, introversion and extroversion. Psychiatric theory is seen by him as highly developed only to the extent that it is reduced to laws of physiology and biochemistry. The Maudsley Personality Inventory is held up as a valuable measure of extroversion while projective tests are dismissed in a few lines on the basis of their

doubtful validity.

He views the psychologist's role and function as limited to the administration of intelligence and vocational tests and ignores clinical psychology on both sides of the Atlantic. He appears unaware that a major area of professional growth in psychology has been in the area of clinical practice with a distinct emphasis on diagnosis, psychotherapy and research.

In touching on social class differences, the author fails to call attention to the problems of bias and distortion which can occur in the interpersonal relations between lower and middle class individuals. Psychologists, social workers, psychiatrists and teachers, as well as other middle class oriented individuals, need to check carefully the factors of bias and class difference in their dealings with a largely lower class clientele.

Although Trasler draws the lines sharply there need be no rejection of learning theory and conditioning in order to accept the view that unconscious and irrational factors may influence and determine behavior.

Most pointedly the volume fails to deal with the growing awareness that in clinical psychological work as well as in other interpersonal fields the need to "know one's self" is a vital step in refining and developing the key instrument which is the psychologist himself.

## Happy Patients, Happy Staff

Kenneth L. Artiss. Foreword by  
Dexter M. Bullard. Introduction  
by David McK. Rioch.

*Milieu Therapy in Schizophrenia.*  
New York: Grune & Stratton,  
1962. Pp. xviii + 169. \$6.00.

Reviewed by ROBERT B. ELLSWORTH

*The author, Kenneth Artiss, is Chief, Department of Psychiatry, Walter Reed*



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## LIFE STRESS AND MENTAL HEALTH

By THOMAS S. LANGNER and STANLEY T. MICHAEL, both of Cornell University Medical College. This is the second volume of the monumental project now famous as *The Midtown Manhattan Study*—the first large-scale investigation of the total prevalence of mental disorder (both treated and untreated) among adults in an urban community. Based on interviews of 1660 men and women between the

ages of 20 and 59, residents of *midtown*, this study aims to relate mental disorder to factors in the social environment. This book briefly reviews the first volume and then focuses on the stressful factors in the environment, other than social background variables, which may be associated with mental disorder. Approx. 528 pages. \$9.75.

## EMPATHY

ITS NATURE AND USES

By ROBERT L. KATZ, The Hebrew Union College—Jewish Institute of Religion, Cincinnati. This is an investigation of the empathic tool, which is too often undervalued in the sciences of man and in counseling. Professor Katz illuminates the origin and operation of empathy through a multi-

disciplinary analysis. He emphasizes its effectiveness in the counseling roles, the necessary conditions for its use, the dangers to be avoided, and guidelines leading to greater skill in empathic identification. Empathic experiences of artists, writers, and scientists are included. 224 pages. \$4.95.

## PSYCHOLOGY AND THE MEASURE OF MAN

A PHENOMENOLOGICAL APPROACH

By JOSEPH LYONS, Veterans Administration Hospital, Lexington, Ky. The author feels that clinical psychology in this country has become characterized by an observer making objective judgments about the observed—leading inevitably to a clinical situation in which the psychologist is operating in terms of power. He proposes an approach that will lead

to a meaningful encounter based on understanding of the patient and his existential reality. The book is controversial and provocative in its challenge to fundamental assumptions, and it is immediately relevant to the dilemmas of contemporary American clinical psychology. Approx. 208 pages. \$5.50.



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*Army Institute of Research. He is the author with Bushard, Erickson, Marlow and Rowe of The Symptom as Communication in Schizophrenia (1959). The reviewer, Robert B. Ellsworth, received his PhD from Pennsylvania State University and has spent the time since then in VA Hospitals, first at Fort Douglas, then at Fort Meade, South Dakota and now in Roseburg, Oregon, where he is Research Psychologist. For the past four years he has conducted a program in which the psychiatric aide was used as the key rehabilitation person with VA schizophrenic patients. This was started at the Fort Meade hospital where he set up matched control units and was allowed to modify the aide's role in the experimental unit. He is carrying on with this research at Roseburg and the research will eventually find its way into a book to be called The Psychiatric Aide and the Schizophrenic Patient.*

FOR ALL that has been written about milieu therapy in psychiatric hospitals, very little in the way of detailed accounts of introducing such programs has been published. Artiss is the second military psychiatrist to report his personal experiences in this area. Wilmer was the first (*Social Psychiatry in Action*, Charles C. Thomas, 1958). Both authors generally agree that a milieu program is essentially an attempt to create a therapeutic social environment. Such an environment or milieu communicates to the patient an expectancy for responsible behavior and assigns him a role of a participant member of a social group. Open and honest communication of both staff and patients is reinforced. One basic departure from traditional hospital psychiatry is a decrease in the absolute authority and status of the psychiatrist and a striving for co-equality in the expression of opinions and feeling for both non-professional staff and patients. The result of this seems to be the utilization of previously untapped sources for effective problem solving in patient rehabilitation.

In July 1956, the Walter Reed General Hospital set aside one 10-bed ward for an experiment in developing and

evaluating a milieu treatment program with schizophrenic soldiers. Artiss, as psychiatrist, was assisted by 12-14 aides (technicians), a social worker, nurse, and an observer psychologist. Between six and eight patients were usually in this unit. The criteria for selection were: (1) an independently achieved and unanimous diagnosis of schizophrenic reaction; (2) completion of high school; and (3) at least one year of Army service. The patient could remain in this unit for no longer than six months, at which time he either returned to active military duty or was transferred from the ward.

The first problem Artiss faced was the development of give and take communication between him and his psychiatric aides. Since the traditional role of the aide is one of listening *to*, rather than talking *with* the doctor, it took five months or about 200 hours before "... the technicians, one by one, gradually began to talk to me as if I were another human being" (p. 9). Later in the detailed presentation of two patients (Chapters 4 & 5), one begins to see the honesty and directness with which the aides interacted with patients. Artiss is undoubtedly right in speculating that most milieu programs never reach fruition because most psychiatrists hesitate to spend this amount of time in working through problems of communication with the ward staff.

Two chapters on group therapy and patient government were disappointing to this reviewer. One gets the feeling that the potential of the patients for active participation in either group therapy or patient government was never really developed. In group therapy, for example, it never became the 'patients' group' but remained the 'doctors' group.' Artiss felt that the group leader must stimulate individual patients to respond, usually by asking questions of each patient in turn.

Perhaps the most interesting chapter is the last, titled "Expectations and Assumptions." It is not until one reads this chapter that the book begins to come into focus. Out of extensive case history material comes an intriguing concept, identified in the development of schizophrenia, that Artiss labels 'the

naming prerogative.' From childhood, the patient is reported as defining his own reality in relation to his parents. Artiss does not permit the reader to judge for himself the validity of this concept since no data are presented nor the interview method outlined.

As a matter of fact, the research oriented reader will find most of the 'research' data relatively meaningless. For example, 63 patients were admitted to the ward, eight were lost because of an unexplained 'research design change,' seven were not considered schizophrenic (after presumably receiving a unanimous independent diagnosis elsewhere), and six were still on the ward. Of the remaining 42 patients, 27 (64%) returned to duty. But one finds himself asking, 'What is the base rate of return to duty for such highly selected patients?' and, 'Why didn't the Army research unit set up comparable wards with different programs if they were interested in assessing the effectiveness of the present procedure?' One must also wonder how realistic a program this is for other institutions, in light of the very favorable ratio of staff to patients.

To this reviewer, then, the book is a not-too-well-done representation of a significant new development in hospital psychiatry. It will be primarily of interest to those concerned with milieu programming, for it does indeed present some interesting concepts and practices. From the research standpoint, it does not approach in quality the work of such psychologists as Saunders (Philadelphia) and Fairweather (Palo Alto). And from the standpoint of milieu therapy in action, Wilmer's work remains the book of choice.



*As quickly as we form other ties, so we easily take on other habits; just as inconstancy is natural to man, so our life is a small thing even in the hearts of our friends!*

—FRANCOIS-RENE DE CHATEAUBRIAND





# Structure without Embodiment

J. A. Deutsch

*The Structural Basis of Behavior.* Chicago: University of Chicago Press, 1960. Pp. vii + 186. \$3.50.

*Reviewed by M. R. D'AMATO*

*J. A. Deutsch, the author, is Associate Professor, Department of Psychiatry and Psychology, Stanford University. He did his undergraduate and graduate work at Oxford where he served on the faculty from 1951 to 1959. After spending the year 1959-60 at the Center for Advanced Study in the Behavioral Sciences, he joined the Stanford faculty to continue his research and writing on the relation of physiology to the general area of experimental psychology. The reviewer, M. R. D'Amato, has been since 1954 a member of the faculty of the Department of Psychology at New York University. Before that he was a graduate student and undergraduate at the same institution. His publications have dealt with learning but he nonetheless keeps alive an interest in behavior physiology.*

MOST of the material in this interesting book derives, often verbatim, from a series of papers published by Deutsch and his colleagues during the years 1953-1959. Since almost all of these papers are scattered among British journals, the book represents a convenient, and integrated, account of the author's point of view.

Deutsch starts things going with a discussion of explanation and its clarification, feeling about psychologists that, "not only do they disagree about the explanation of their findings, but they are not clear about what it would be to explain them." Accordingly, "it is not sufficient only to put forward a theory to explain the facts; it is also necessary to put forward a theory to

justify the type of theory put forward." What follows is an unextraordinary analysis of theories into structural and non-structural ("generalizatory") types, i.e., those that postulate an underlying structure or system from which the behavior to be explained may be deduced, and those that do not. But then quite extraordinarily the author, disturbed that the creation of structural theories might result in the "creation of physiological mythology," aligns himself "against speculating about the mechanism underlying behavior, but not against attempting to infer the type of mechanism or the system producing behavior." The psychological theorist, according to Deutsch, "need only speculate about the system and not its embodiment."

This view appears to be at variance with the expressed aim of the book, which is the presentation of a set of "structural models or explanations of behavior in terms of postulated mechanisms, such as might be found in the nervous system of animals or such as might be built as machines to manifest the same behavior as animals." To posit an explanatory structure or mechanism "such as might be found in the nervous system" and then stop short forever of speculating as to its physiological "embodiment" seems logically inconsistent, and for the theorist concerned, an unnecessary frustration as well. Almost three years have passed since the publication of this book, however, and recently there appeared evidence (Deutsch, J. A., A system for shape recognition. *Psychol. Rev.*, 1962, 69, 492-500) that Deutsch may now think differently on this issue.



J. A. DEUTSCH

THE SET of structural models advanced by the author consists of a learning (and motivation) model, also capable of handling 'innate' behavior patterns, and four, not altogether independent, perceptual mechanisms, dealing with shape recognition, slant discrimination, figural after-effects, and after-effects of seen movement.

The learning-motivation model has a number of ingenious aspects, and the range of phenomena to which the model is applied is astonishing—imprinting, drive arousal and reduction, intracranial self-stimulation, simple learning, extinction, curiosity and exploration, latent learning, reasoning and insight, and so on. In dealing with these various areas Deutsch's strategy often is to introduce a competing theory (usually Hullian), to show by a searching analysis that the theory is incapable of explaining a certain body of data and, finally, to present his own formulation. One consequence of this approach is that somewhat too much time is spent on familiar foils.

Deutsch is quite expert in putting his finger on the weak spots of rival theorists. One is not convinced, however, that if the same finger were pointed at *his* theory, it would come off quite so favorably. Though formulated in numbered paragraphs of reasonably



rigorous language, the theory has sufficient 'play,' always present in verbally stated postulates, to allow for both flexibility and a measure of ambiguity. Nevertheless, the panorama of data which Deutsch has found applicable to his learning-motivation model makes his general approach, if not this specific 'embodiment,' interesting and provocative.

The perceptual mechanisms, which account for only 16 per cent or so of the volume, seemed to this reviewer to have great potential significance. Since Lashley, there has been in this country a vast neglect among learning theorists of the 'input' side of the organism. The analysis of discriminative capacities and mechanisms has received scant attention even from those most closely concerned with discrimination learning. Perhaps because of a different tradition, psychologists like Deutsch, Sutherland, and Dodwell seem to have escaped our learning-perception dichotomy. The dimensions along which animals classify stimuli, and the mechanisms involved, apparently interest them no less than problems relating to learning and their mechanisms. Surely it is a reasonable assumption that both areas are related, that an understanding of the former will, for example, facilitate an understanding of discrimination learning, even though the perceptual and learning mechanisms involved might develop independently and not articulate directly.

At a higher level of integration, however, perceptual and learning mechanisms should dovetail. Toward this more difficult level of communication Deutsch's perceptual mechanisms have less to offer. Though they stand side by side with his learning-motivation model there is, as yet, no point of contact. Still, the possibility of bridging learning and perception is unquestionably enhanced when problems from the two areas are viewed through the same pair of eyes.

The question inevitably arises as to the validity of the proposed structural models. The shape recognition model, the most carefully constructed of the perceptual mechanisms, has already succumbed to a drastic reformulation

(*op. cit.*). The other perceptual mechanisms are less clearly drawn, hence less secure. And despite the author's rather sanguine account of the possibilities of his learning-motivation model, it seems likely to this reviewer that it also is destined to suffer major revisions before gaining general appeal. This is not

surprising, considering the dimensions of the task that Deutsch has set himself.

If nothing more—and there is a good deal more—this book shows Deutsch to be a deft critic with a far-ranging and creative imagination. Hopefully there shall be reformulations and revisions, and many of them.

## Both Progress and Revision

Donald E. Super and John O. Crites

*Appraising Vocational Fitness: By Means of Psychological Tests.* Revised Edition. New York: Harper, 1962. Pp. v + 688. \$8.75.

Reviewed by WILLIAM C. COTTLE

*Donald Super, the senior author here, is, as is well known, a counseling psychologist and a Professor in Columbia University's Teachers College. Not so well known is the fact that he was educated in France and England before coming to Columbia for his PhD and that he speaks French, Spanish, Polish and Japanese as well as the language of psychometrics. His collaborator here, John Crites, is Assistant Professor of Psychology at the University of Iowa and Senior Counselor in the University Counseling Service. The reviewer, William C. Cottle, did his graduate work at Syracuse University and taught for a while at the University of Kansas before moving to Boston College where he is Director of Counselor Education, Director of the Counseling Psychology Program and Professor of Education. He is author of The MMPI: A Review (1953).*

THIS BOOK, like the earlier edition, is essential for the library of the counseling psychologist. It would make a useful companion text for a book like that of Cronbach or Anastasi in a course dealing with understanding and using group psychological tests in vocational appraisal and counseling. It can also serve as a review for psychologists working in other specialties of major

developments in the area of vocational testing.

The authors of the revised edition have wisely elected to change as little as possible of the excellent material in the 1949 edition. Only those changes required to make the text current have been introduced.

Many changes have been made in the chapter dealing with multifactor batteries, in the one on measurement of interests, and in the treatment of measurement of personality, attitudes and temperament; the revisions reflect the changes in vocational testing over the last twelve years. The general bibliography of the earlier edition has been omitted and appropriate references are included at the end of each chapter instead. This is a major change in format from the earlier edition, as is the change from a general index to separate indices for authors, for occupations, for subjects, and for tests.

Most of the material in Chapters 1 to 13, and in Chapter 15, is the same as that of the earlier edition with the change of a sentence here or there. The same is true for Chapters 20-22, although four new pages are added at the end of Chapter 20 to bring the discussion of "appraising individual vocational promise" up to date. Three cases have been omitted from the present Appendix A which was Chapter



23 in the 1949 edition. Appendix B is old Chapter 24 with minor changes. Appendix A of the older edition, dealing with statistical concepts, has been omitted in the current text.

Chapter 14, dealing with multifactor test batteries, still discusses only the General Aptitude Test Battery and the Differential Aptitude Tests as the two batteries in general use, mentioning others but stating that "...as yet they do not have substantial amounts of validity data." The material on the General Aptitude Test Battery now includes a much more complete discussion of the basic developmental work as well as additional validation data appearing since the 1949 discussion. Research published since 1946 on the Differential Aptitude Tests is also discussed and the general use of these two multifactor batteries for vocational counseling in various settings has been emphasized.

**W**HAT amounts to a major rewriting has been done on the chapters on vocational interests in order to include the extensive research that has been published since 1949. The discussion and conclusions are generally appropriate for these data with perhaps a little too much emphasis on the relation of interests and endocrine factors in light of the limited data available. The authors do a professional job of reviewing the research they have chosen to include in these chapters. They include a synthesis of "inherited neural and endocrine factors, on the one hand, and opportunity and societal evaluation on the other" (p. 410) as the multiple elements interacting in the development of vocational interests. This synthesis could well have had more space than one page devoted to it. The effect of response set on inventoried interests could have been indicated. For example, attention is called (p. 447) to the unusual frequency with which women get high scores on nurse, office worker, elementary school teacher and housewife scales of the Strong Vocational Interest Blank for Women. It might have stimulated research into this phenomenon if the authors had pointed out that the high scores seem

to be related to the combined effects of "like" and "indifferent" responses to the items scored for these scales. It has been found that if one marks a blank all "like," another all "indifferent," and a third all "dislike," effects of response set manifest themselves. Another place where discussion of response sets could have enhanced the material is in the discussion of the Occupational Level scale of the Strong Vocational Interest Blank for Men (pp. 390-1). The major research on measurement of vocational interests has been covered adequately.

The treatment of personality, attitudes and temperament in Chapter 19 is based on the statement (p. 517) that, "personality as defined in this discussion determines the kinds of adjustment problems which the worker will encounter and the role he will play in any occupation he enters." In this frame of reference the chief instruments discussed are the Minnesota Multiphasic Personality Inventory, the Edwards Personal Preference Schedule and the Rorschach Inkblot Test, with minor space devoted to the Kuder Preference Record-Personal, the Thematic Apperception Test and the Incomplete Sentences Test. The synthesis of research on the MMPI and the EPPS contains the major points the counselor needs to consider. The content of the chapter highlights the current trend toward more sophisticated self-report instruments while it attempts to forecast increasing use of projective devices in vocational counseling. However, after devoting fifteen pages to a discussion of the Rorschach, the authors conclude (p. 575), "the technique can be considered only an instrument which may be worth using in validation studies, as one which research may yet prove valuable in vocational counseling and selection, but about which too little is now known to justify its use in practical counseling or personnel work." One could not help wishing that the Rorschach had been given the minor treatment of the other projective devices which would have left space for a discussion of the California Psychological Inventory and the Guilford Zimmerman Temperament Scale which Super and Crites say they wished they could have included.

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# Enlisting Military Aid

Robert Glaser (Ed.)

*Training Research and Education.* Pittsburgh: University of Pittsburgh Press, 1962. Pp. v + 596. \$11.00.

Reviewed by JOHN D. KRUMBOLTZ

The editor, Robert Glaser, took his PhD at Indiana University and taught at the Universities of Kentucky and Illinois before moving to the University of Pittsburgh where he is now Professor of Psychology and Director of the Measurement Laboratory (which consists of three entities, the University Testing Service, the Programmed Learning Laboratory and the Engineering Psychology Laboratory). He is co-author with Lumsdaine of *Teaching Machines and Programmed Learning* (1960). The reviewer, John D. Krumboltz took his PhD (1955) in Educational Psychology at the University of Minnesota. Since then he served as a psychologist in the Air Force and taught for a while at Michigan State University before moving, in 1961, to Stanford University where he is now Associate Professor of Education and Psychology in the School of Education. He is author with W. W. Farquar and C. G. Wrenn of *Learning to Study* (1960), he has worked on selection and classification problems as an Air Force psychologist, has taught teachers at Michigan State and has done research on various ways of constructing more effective programmed learning sequences.

READING this book is like being struck by a falling apple. If you have certain Newton-like qualities, you may generate a number of new insights. If not, you may gently rub an aching head.

Although the title of the book may at first mislead you to think that one can now train both research and edu-

cation, the book is actually designed to provide an account of representative research and thinking that has gone on in the context of military training and to examine the implications of this research for education in general. The book does provide an excellent account of much military training research. A psychologist concerned with improving education can find hundreds of provocative studies, hypotheses and ideas which, if he thinks about them, could be tried out in schools and colleges. The responsibility for perceiving the applications, however, rests with the reader since they are not spelled out in detail by the authors.

As G. L. Bryan points out in his chapter, "It would indeed be unfortunate if the vast fund of information produced under military sponsorship continued to escape the attention of the academically-oriented researcher." Many of the research findings and ideas in this book have appeared previously only in military publications which seldom reach civilian psychologists and educators. The integration of these relatively inaccessible sources with other previously published material from journals and conferences provides a rich source of ideas for any psychologist seriously interested in improving the quality of education.

For a compilation of 18 chapters by 19 authors, this book is remarkably well organized. Virtually every chapter represents high standards in scientific reporting. Periodic cross references and a consistent behavioristic viewpoint among all authors yield a well integrated product.

The content ranges from a notation that whiskey tasters make unreliable judgments to J. B. Carroll's discussion of a mathematical model for prediction of success in foreign language training. Here is a potpourri of ideas and findings paraphrased from assorted authors:

(1) The behavior known as "understanding" may be produced by continuous variations of the stimulus context in which the student responds. (Glaser)

(2) A possible cause of a novice's disorientation on the job may be the absence of exercise material requiring him to scan and search for relevant cues during training. (R. B. Miller)

(3) The use of a regressed score, or residual, has distinct advantages as a measure of change in a learning situation. (P. H. DuBois)

(4) Tests that predict initial performance in a complex task are different from the tests that predict terminal performance. (E. A. Fleishman)

(5) The effect of stress will depend upon the components involved in the skill being performed, and stress may, therefore, either facilitate or inhibit skilled performance. (J. Deese)

(6) Simulators promise great usefulness to professional schools for the analysis of professional activities and the development of effective operational doctrine. (R. M. Gagne)

(7) Familiarization training with component parts is more effective if given in the context of, and just prior to, training in the utilization of each task element than if given as a completely separate, prior procedure. (A. A. Lumsdaine)

(8) Ratings are no more valid when raters are "sure" they have correctly observed the ratee than when ratings are performed without this assurance of observation. (C. L. Wilson)

CHAPTERS whose organization and content contributed particularly well to the purpose of the book are provided by Lumsdaine, who critically examined the effects of variations in stimulus and response features of instructional sequences, and by A. F. Smode, who compiled and evaluated new techniques of providing and evaluating learning experiences. Glaser's analysis of the components of the in-



structional process and Miller's discussion on the parts of tasks provide suggestive leads toward the development of a task taxonomy. Some interesting examples of ways of assessing performance through eliciting lifelike behavior are described by Frederiksen. Factors to be considered in administering training research and identifying worthy problems are discussed by Vallance and Crawford.

What chance is there that a book like this will accomplish its ultimate objective—the improvement of education? Not much, if statements appearing in the book are accurate. Lumsdaine concludes that instructional research seldom will be applied when its sole product consists of principles, conclusions, or scientific "laws." And R. M. W. Travers, in a perceptive historical analysis of the relationship of educational research and practice, finds that it is not the research scientist who brings reform to education. Indeed, if Travers is correct, facts about improved teaching methods not only have no impact on the behavior of classroom teachers, but they have no impact on the teaching behavior of educational researchers!

As long as educational and psychological researchers reinforce each other for merely writing and reading their research findings, the evidence indicates that their effect on educational practice will be infinitesimal. However, the writers of this book provide at least three suggestions, which, if heeded, may markedly increase the influence of research on practice:

1. On the basis of research findings, develop tangible products (programed books, films, simulators, etc.) to which teachers may delegate control for part of the school day and which by their successful use in practice can engender a demand for themselves. (Lumsdaine and Travers)

2. Initiate research on the problem of changing teacher behavior. (Travers)

3. Develop an organization whose only goal is educational pay-off and insure its support of every stage of the research effort from uncommitted inquiry through development, design, field testing, training and follow-through. (T. F. Gilbert)

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# Piaget: Critique and Interpretation

William Kessen and Clementina Kuhlman (Eds.)

*Thought in the Young Child: Report of a Conference on Intellectual Development with Particular Attention to the Work of Jean Piaget.* Lafayette, Ind.: Society for Research in Child Development, Inc., 1962. Pp. 176. \$3.50.

Reviewed by ELEANOR E. MACCOBY

William Kessen, one of the editors of the present volume, did his graduate work at Brown and Yale Universities. Throughout his career he has maintained a research interest in emotion and learning in the newborn human being but has branched over easily and often into a concern with early cognitive development. Clementina Kuhlman took all of her education at Radcliffe (PhD 1960) and since has served as a postdoctoral research fellow with the Judge Baker Guidance Clinic and as a research associate at Harvard. The reviewer, Eleanor Maccoby, now an associate professor at Stanford, first became active in child psychology when she joined the staff of the Laboratory of Human Development at Harvard to work with Robert Sears and John Whiting. There she became the Maccoby of Sears, Maccoby and Levin who produced *Patterns of Child Rearing* (CP, Dec. 1957, 2, 305). She moved to the Stanford campus in 1958 and for the last four or five years has been concerning herself with cognitive abilities and their relation to the development of inter-personal behavior.

tinct as Piaget had claimed, and that the age level he had designated for the acquisition of certain concepts were not accurate for new samples of children. An attitude of skepticism about his work grew up, and there was a long period during which Piaget published prolifically but had very little influence in American psychology. During this time, the "language" barrier grew worse, for even in translation, much of Piaget's central terminology (e.g., assimilation, accommodation, reversibility) seemed either obscure or dissonant with other current usages of the same terms.

In recent years there has been a renaissance of interest in Piaget's work, occurring as part of a general resurgence of thinking and work on the development of cognitive processes. The Kessen-Kuhlman monograph adds greatly to this new body of both supportive and critical thought, for it offers analysis of Piaget's theories at a sophisticated level, and includes some attempts to reformulate them in terms of the sets of concepts which are prevalent among American psychologists.

The monograph contains a series of papers (together with a report of the discussion which followed them) which were presented at a conference on intellectual development convened in Dedham, Massachusetts, in the spring of 1960 under the aegis of the SSRC Committee on Intellectual Processes. The papers were not all focussed directly on Piaget, but some of the main themes for discussion were established

in the initial paper by Barbel Inhelder, who represented the Geneva school, and the editors have kept these themes clear in their summaries.

A first theme, central throughout the monograph, is the issue of stages. Kessen devotes his paper, "Stage and Structure in the Study of Children," to an exposition of the meanings this concept can have, and the group continually raised questions concerning the nature of the evidence that would be necessary to demonstrate the existence of the sort of discontinuity of developmental processes that the theory of stages implies. Wohlwill, in his paper "From Perception to Inference, a Dimension of Cognitive Developments," refers to the large body of Piaget's untranslated articles on perception, and points out that Piaget holds to a theory of stages in *intellectual* development, but not in *perceptual* development, and that in general he attempts to divorce these two kinds of processes. Wohlwill attributes the difference to the fact that, in his studies of intellection, Piaget has taken as his task the specification of the "formal properties of the products of thought" at different ages, a procedure which leads almost inevitably, Wohlwill believes, to a picture of "successive metamorphoses." Wohlwill emphasizes the continuous interplay between perception and conception, and suggests that the degree and kind of involvement of the former in the latter changes with the developmental level of the child. He holds that there is a progressive change (not a stage-wise one) in the degree to which behavior is dependent on information from the immediate stimulus field, and gives an original and fairly detailed description of the ways in which he believes the child's information processing reflects this change.

A SECOND main theme of the conference concerned "transition rules." American psychologists, having been almost universally schooled in learning theory, are vitally interested in the processes of *change*; they want to know what conditions govern transitions in the child's mode of responding to the environment. Piaget has described some of the different modes of cogni-

WHEN Piaget's early books were translated into English, there was a flurry of interest among American psychologists and some research ensued in which Piaget's methods and concepts were employed. The results showed, to the satisfaction of most American readers, that the "stages" of concept development were not as dis-



tive functioning that characterize successive ages, but has said little concerning the conditions that govern movement from one to the other. He neither undertakes experiments designed to change the child's behavior, nor studies the correlates of individual differences in the rate of acquisition of particular functions, both of which are procedures widely employed in this country to study "antecedent—consequent" relationships (or transition rules). The difference in method, then, reflects a difference in the nature of the tasks which have been undertaken and the problems that are considered important. At the Dedham conference this "gap" (from the American point of view) in Piaget's work was clearly recognized; his recent attempt to deal with transition problems through the concept of "equilibration" was discussed; and there were efforts by participants to apply existing models for the study of change to the material Piaget has described. Notable here is Stevenson's paper "Piaget, Behavior Theory, and Intelligence" in which he suggests that S-R learning theory may be applied to the transitions through Piaget's sensorimotor stages, if one assumes a "need for sensory stimulation" and rapid satiation to individual stimuli.

The editors note that there appears to be growing up in some quarters a kind of mystique of Piaget worship, "made up of equal parts of adulation and awe." They regard this attitude as one not any more likely to produce real understanding than the earlier narrow skepticism. To the psychologist who is not in either of these extreme attitudinal camps, their monograph should provide great assistance in the task of finding what is most useful in Piaget.



*It might be formulated as a law of public pressure that the strength of interest is as twice the strength of numbers: those with a vague and intermittent goodwill are helpless against a sleepless and consuming want.*

—THOMAS GRIFFITH



## All the News is Fit to Print

Emory L. Cowen, Rita P. Underberg, Ronald T. Verillo and Frank G. Benham

*Adjustment to Visual Disability in Adolescence.* New York: American Foundation for the Blind, 1961. Pp. v + 239. \$4.50 (cloth) \$2.50 (paper).

Reviewed by IRVING LAZAR

*Emory Cowan, the senior author here, took his PhD from Syracuse University in 1950 and since then has been at the University of Rochester, where he is Professor, Assistant Chairman, and Director of Clinical Training in the Department of Psychology. He is primarily interested in the underlying processes of personality, including anxiety, the self-concept and rigidity. The other three authors, Underberg, Verillo and Benham, were doctoral candidates at Rochester when they engaged in research presented in the book. The reviewer, Irving Lazar, received his 1954 PhD from Columbia University and since then has had unusually extensive experience with physically handicapped children; he has organized, directed, and consulted in programs for the exceptional child in New York, Illinois, Nevada and, most recently, in California. He has been particularly concerned with problems of adjustment to physical disability. He serves as Director of Special Services at the Foundation for the Junior Blind in Los Angeles and also serves as Director of Research and Counseling for the Family Research Foundation and Trustee of the California Association for Neurologically Handicapped Children.*

personal adjustment from normal, fully sighted youngsters.

The subjects used in this study were a group of 71 blind and partially sighted adolescents who were living at home, a group of 56 blind children at 4 residential schools for the blind, and a group of 40 sighted children who were in public day schools and living at home. An attempt to match these groups on age, grade in school, IQ (Stanford-Binet Vocabulary subtest), and an index of socio-economic status was undertaken, but was only partially possible. The grade-level differences were in the direction one would expect, with the children in residential school about a year behind the others. The class level differences indicate that the visually impaired who were at home were of higher status than either the controls or the institutional sample—again a common difference between families who keep their handicapped child at home—with the attendant expense this involves—and those who place their children in public residential institutions.

This reviewer finds himself wondering, not at these slight (although statistically significant) differences, but rather at the choice of variables for the equating of the two "experimental" groups. There seems to be no data telling us why the hospitalized group were hospitalized. Presumably the instrumental data would give some leads on this.

The instruments chosen were somewhat of a surprise to the reviewer. The authors present a cogent rationale, pose a clear-cut set of questions, and then use a set of measures whose relation-

USING a variety of specially defined measures of interpersonal perceptions and ratings, and a relatively small group of subjects, the authors of this statistically conscientious study come to the conclusion that visually impaired adolescents whether at home or in a residential school, do not differ in their



ship to adjustment seems more inferential than direct. These measures are as follows:

(1) A questionnaire combining items from the PARI, the F-scale of the MMPI, an Anti-Minority Scale, and an Attitudes Toward Blindness Scale. These were administered to parents. Unfortunately the reporting of combined scores made impossible any direct comparisons with other studies using, for example, the PARI scale as a separate instrument.

(2) A Situations Projective Test which consisted of pictures of blank-faced children and adults, and for each, a set of four statements about the feelings of one or another of the figures pictured. While the controls took this as a paper and pencil test, the blind subjects had the pictures verbally described to them. The four choices presumably reflect Pity, Overprotection, Rejection, and Acceptance. A second form of this test was administered to the parents.

(3) Each parent was presented with a set of statements in a modified Q-sort and a self-ideal score was derived for these.

(4) A teacher's rating scale was completed by the children's teachers, and this rating was defined as a measure of adjustment.

Intensive statistical manipulation of these data constitute the findings of this study. Nowhere do we find a clinical interview with either a child or his parent; nowhere a clinical evaluation based on a standard battery of diagnostic tests, and nowhere an inquiry into the background of the families involved. Nowhere do we learn why the institutionalized children were institutionalized; nowhere do we find any inquiry or search for children who have been excluded from public school classes and are *not* in institutions. Nowhere do we come to grips with a clear cut theoretical position on the authors' concept of adjustment.

ON the other hand, we find a marvelously honest and thorough piece of reporting. When the authors "goofed," when they realized omissions too late, when they suffered the tribula-

tion of arrangements and location of subjects, they carefully reported these. Indeed, it seems to this reviewer that a major value of this volume is as a tool for the teaching of research methodology and the problems of carrying out a research plan in service agencies.

The limitations of this study reflect the "state of the art," and a kind of "Zeitgeist" in psychology that so often presses us into the use of highly sophisticated statistical techniques on relatively unsophisticated measures of conceptually vague clusters of traits.

The positive aspects of the study outweigh these negatives. In addition to being a real model of careful, full, and intelligent scientific reporting, its basic findings can have an important effect on current attitudes toward the blind.

First of all, the data clearly show that there are no important differences between sighted and visually impaired adolescents, or in the attitudes of their parents on these measures. This finding is consistent with the observations of many workers in this field. It is consistent with this reviewer's own data on 400 blind youngsters, and it stands pretty firmly in the face of doctrinaire assertions that there *must* be a maladjustment if there is a physical difficulty.

Secondly, the data on parental attitude indicate that, again, despite the assertions of many clinicians, the parents have attitudes and percepts not at all different from those of sighted youngsters. This too is consistent with such data as is available. Indeed, a recent unpublished\* study using the PARI with 250 mothers of blind youngsters found that the only significant difference between these mothers and a control group was that these mothers had a higher mean score on the variable "comradeship and sharing."

Thirdly, one can only hope that this careful study will set a standard for other research with the blind—indeed with other handicapped groups too. Despite its lack of either behavioral or clinical data, it represents a refreshing and wonderful contrast with the bulk of work in these areas, and the authors deserve the thanks of all of us.

\*Foundation for the Junior Blind—Demographic Indices, 1962

## Two Useful Sprawls

Warren R. Baller

*Readings in the Psychology of Human Growth and Development.* New York: Holt, Rinehart and Winston, 1962. Pp. xiv + 689. \$4.75.

Judy F. Rosenblith and Wesley Allinsmith

*The Causes of Behavior: Readings in Child Development and Educational Psychology.* Boston: Allyn and Bacon, 1962. Pp. v + 514. \$6.50.

Reviewed by JOHN P. McKEE

Warren R. Baller, editor of the first collection, is Professor of Educational Psychology at the University of Nebraska. In a varied career he has been a high school principal, a high school superintendent, and a dean on at least two occasions. Judy F. Rosenblith and Wesley Allinsmith, who collaboratively edit the second book, and both of whom have long been interested in developmental matters, coincided in Cambridge, Massachusetts in the late fifties, while Dr. Rosenblith was learning at Radcliffe and teaching at Brown, and Dr. Allinsmith was, until 1960, associated with Harvard's Laboratory of Human Development. John McKee, the reviewer, became interested in child psychology in 1939 because Kurt Lewin taught a MWF course at 10. He received his 1949 PhD in Child Welfare from the State University of Iowa after exposure to such fountains of knowledge as Robert Sears, Beth Wellman, John Whiting and Vincent Nowlis. Since then he has been at Berkeley where he has spent nine years with Harold Jones at its Institute of Child Welfare and is now Associate Professor of Psychology. He has plied many trades in the field of child development. His latest creation is a chapter on orality, done with Marjorie Honzik, in Leo Postman's *Psychology in the Making* (1962).

IF PROOF is needed that psychology is a sprawling enterprise, these two use-



ful collections of readings in the same general area provide it. Though published within a couple of months of each other—so we may presume that selections in one were chosen independently of those in the other—the overlap is almost nil: Baller's forty-eight choices include only one of Rosenblith and Allinsmith's eighty! The divergence is all the more remarkable for the fact that sixty-nine percent of Baller's selections and eighty-six percent of Rosenblith and Allinsmith's were first published between 1950 and 1961.

These two compilations differ in several ways. In general, though not in every case, Baller's choices are longer, easier, more edited, and more traditional than Rosenblith and Allinsmith's. Baller has aimed at sophomores and should hit his mark. Among his authors, but not Rosenblith and Allinsmith's, are J. E. Anderson, L. Carmichael, H. B. English, D. B. Harris, J. E. Horrocks, A. T. Jersild, H. E. Jones, R. G. Kullen, Dorothea McCarthy, and Celia Stendler. Baller also includes a selection from Gesell, but Rosenblith and Allinsmith reprint Lois Stolz's rather critical 1958 *CP* review of Gesell, Ilg and Ames' *Youth: the Years from Ten to Sixteen*. Rosenblith and Allinsmith include many psychoanalytic choices (some only a paragraph or two in length), but Baller's closest approach to psychoanalysis is a selection from Percival Symonds. Finally, Rosenblith and Allinsmith include seven papers in a section on "Learning as a Determinant of Behavior" plus several others that owe an intellectual debt to learning theory; none of Baller's choices are explicitly concerned with learning. Students ought to know what is in Baller before they tackle Rosenblith and Allinsmith.

Much as they differ, these two collections still have some things in common. Both include selections from the group at Fels, from Sears, Maccoby and Levin's *Patterns of Child Rearing*, and sections on intelligence, motivation, and biological aspects of development, as well as social and cultural aspects. Both contain brief explanatory and introductory statements before major sections. Additionally these two volumes have two identical omissions: Piaget and his

students and Heinz Werner and his. Rosenblith and Allinsmith do include a previously unpublished report by M. Ezer which he considers contradictory of Piaget's position on the development to causal thinking, and sections of a paper by Susan Ervin that is partly favorable to Piaget.

INDIVIDUAL items in collections such as these are hardly a valid subject for a reviewer's comment. Perhaps the slightly raised eyebrows implied by the mention of the neglect of Piaget and Werner are also inappropriate. But when a whole substantive area is omitted, a reviewer probably should call attention: in these collections there is only one empirical study of schoolroom learning, and even this paper (by Grimes and Allinsmith) gives only a very sketchy account of the two different methods used to teach reading in the third grade.

Now this is most peculiar; after all, the subtitle of Rosenblith and Allinsmith's collection includes the expression *Educational psychology* as does their *Preface*. Baller's *Preface* makes clear that he too is concerned with a "psychology for teachers," and Baller and Charles' *The Psychology of Human Growth and Development* (Holt, Rinehart and Winston, 1961)—for which the *Readings* is intended as a companion piece—"is primarily for students who are preparing to teach" (v). Both collections include a number of papers about classroom *behavior*, teacher-child understanding and so on, but they do not provide material for students who want first-hand accounts to help them justify this, that or the other method of teaching spelling, the N basic arithmetic facts, or what-have-you. Neither is there first-hand material about the effectiveness of introducing the various school subjects at different ages. In other words, there is a conspicuous absence of that aspect of educational psychology which laymen, parents and the reviewer (and some authors of the *Educational Psychology* sections of the *Annual Review of Psychology*) think of as the heart of the field. Which is too bad, because seven or eight quite good collections of readings in 'development' are already available. To be sure, they

are not quite so up to the minute, but revisions are on the way.

The omission of schoolroom learning may not be the fault of the editors. The reviewer's experience in doctoral examinations in educational psychology leads him to wonder just what is known about the field—and who knows it. Where is evidence about the effectiveness of phonics, drill, the meaningful method, different algorithms, length of class period, audio-visual aids, class projects, book-reports, science laboratories, homework, special rewards or privileges for high achievement, group vs. individual competition and goodness-knows what other methods that have been used to teach reading, spelling, grammar, mathematics, history, geography, biology, physical science, foreign language and all the rest of the school curriculum? Shouldn't such evidence be included in collections such as these?

Admittedly, teaching is an applied art, and practical schoolmen deal with problems whose social significance and real life settings make elegant research very difficult to achieve. But there is empirical material on these matters. Even if the material is not conclusive, wouldn't it be desirable for students to know at first hand just how inconclusive it is? Conclusive evidence should of course be made available to students—particularly to those in educational psychology. Furthermore, such a "Readings in Schoolroom Learning" would fairly approximate a gold mine for the editor and publisher.

Any takers?



*Why should everything come to consciousness that lies in the mind since, for example, that of which it has already been aware, the whole great realm of memory, only appears to it illuminated in small areas while the entire remaining world stays invisible in the shadows? And may there not be a second half world of our mental moon which never turns toward consciousness?*

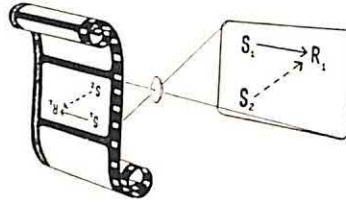
—J. P. F. RICHTER





# INSTRUCTIONAL MEDIA

Edited by A. A. Lumsdaine



## TV Effects: London View

Hilde T. Himmelweit, A. N. Oppenheim, and Pamela Vince

*Television and the Child.* (A study sponsored by the Nuffield Foundation.) London: Oxford University Press, 1958. Reprint, Chapters 1-4, 1961. Distributed by The Television Information Office, 666 Fifth Avenue, New York 19.

Reviewed by MARGARET MEAD

*The reviewer is, as everyone knows, author of a number of anthropological works (Coming of Age in Samoa, Sex and Temperament in Three Primitive Societies, and others.) Since her 1929 PhD from Columbia, she has acquired half a dozen or more honorary degrees and has held an impressive number of guest lectureships and visiting professorships of institutions in Europe and the United States. Less well-known than her books are several films which she has written and narrated, including Character Formation in Different Cultures, First Days in the Life of a New Guinea Baby and several others. Dr. Mead is Associate Curator of Ethnology of the American Museum of Natural History and adjunct Professor of Anthropology at Columbia University.*

THE publication, in pamphlet form, of the first four chapters of the large-scale study, which appeared in 1958, makes it possible to place an English study beside the American study made by Schramm, and his associates along very much the same lines. Schramm, Lyle, and Parker, *Television in the Lives of Our Children*: Stanford University Press, 1961: see Bailyn's

review, *CP*, March, 1962, 7, 116-117. The English study is simpler in design. It deals with a narrower class spread (blue collar and white collar), with only two age groups (9-11 years and 13-14 years), and with four areas (London, Portsmouth, Sunderland, and Bristol). It consists of one large study of individuals matched by age, sex, class, and a measure of intelligence, and one study of a group before and after they obtained television sets. The work was supplemented by special interviews. For background on the English study, there is also the more intensive study with a wide class spread of adults and children, made by Geoffrey Gorer in 1957-58 (*Television in Our Lives*: The Sunday London Times, May 1958).

In spite of the marked differences between England and the United States in patterns of family life and leisure time and in the programing of television—with much less viewing time and less program choice in England—the similarity of the results obtained in the English and the American study indicate the extent to which the mass media have come to dominate life in both countries and attest to the uniformity of television, in particular, as a medium.

Both studies found the age and the

intelligence of the child viewers to be the selective factors. The Himmelweit study stresses the fact that class is relatively less important than is either intelligence or the atmosphere of the home. It lays less emphasis on emotional adjustment as a determining factor than the American study does, and it discusses, rather simply, the values children absorb, whereas the American study distinguishes between fantasy productions within the value structure of the popular mass media and reality—documentary news and high art programs. Because of English viewing conditions, the English study could do more with the effect of single-channel viewing as a widener of taste, suggesting that multi-channel choice perpetuates a low level of taste.

Both studies found that television must be considered within the context of all mass media and that the extent to which television substituted for other types was proportionally related to an individual's or a family's dependence on mass produced entertainment. Both studies concluded that young children and dull children gained from television; in the English study this was estimated as a factor of about five months. But the brighter and the older the child, the more did television—with the greater expenditure of time necessary, as compared to reading, to acquire the same amount of information—appear to detract from the intellectual level. Both studies emphasize that what must be taken into account is not the single program but rather the cumulative effects of the values expressed and the level of excitement. Both conclude that television viewing does not create crime or delinquency.

CERTAIN specific results of the Himmelweit study are interesting. Television permanently replaces comic books in a child's repertoire. But book reading, initially reduced by television, increases among those children who view television most frequently; in the end, therefore, reading increases among the children from the most impoverished environment. Both studies show little effect of television on the level of children's activity; it stimulates rather than motivates. Both give much the same



advice to parents and teachers: avoid excessive viewing alone in the dark; look at and discuss television with your children; integrate it into teaching; and realize that children cannot be protected from adult programs—and so the emphases of adult programs must be alertly watched.

These studies are a culmination of a long series of inquiries into the mass media—comic books, popular magazines, movies, radio, and now television—in which various social scientists have attempted to assay the effects on character or culture of some one of the mass media. The results of such studies have been dismally similar. The effect of the particular medium is slight; what it is depends on the selectivity of the individual child and the specific home. The discussion of the entire pattern of leisure time in the Himmelweit study, and to a lesser extent in the Schramm study, forces upon one the conclusion that this method of studying only one medium produces considerable obfuscation. It is the effect of the mass media as a whole (including advertising), not of any one of them, which is changing the characters of our children and the values of our culture. Each medium echoes each other one, and any one can replace another for a variety of technical and psychological reasons. We are indeed rearing a generation by the mass media, taken together. Emphasis on the relatively greater importance of television is, of course, useful—first, because it concentrates the attention of responsible educators on the significance of this huge slice of children's time and, second, because it increases our awareness of the very early age at which the child is exposed to comprehensible influences from the outside world.

The Himmelweit study, like the Schramm study, fails to come to grips with one possibly genuine difference between television and the other mass media. This is the matter of addiction. Himmelweit defines as addicts the thirty per cent of the group who were the most frequent viewers, and she fails to analyze the break between those who use the medium to fill time and those who are addicted to viewing. Geoffrey Gorer concluded that about one-sixth of his viewers had lost all discrimination in their devotion to whatever ap-

peared on the screen. Schramm regarded addiction as a function of emotional maladjustment and withdrawal from other children, and he saw this in turn as the result of a home lacking in love and acceptance. But it may well be that there are specific elements in addiction to this particular medium, miniature images viewed in a darkened room, which call for another type of study.

## TV Effects: Boston View

Ralph Garry (Ed.)

*Television for Children.* Boston: Foundation for Character Education, 1962, Pp. 55.

Reviewed by FRANK STANTON

*The report, edited by Dr. Garry of Boston University, is based on conferences held 1958-60 at Boston University and attended by a dozen psychologists and other academicians, as well as several representatives of the broadcasting industry in the United States and other countries. The editor is Professor of Education at Boston University. He took his doctorate at Stanford in 1950 and has, since 1954, been associated with the Foundation for Character Education. The reviewer, Frank Stanton, is well known as President of the Columbia Broadcasting System. A psychologist with a doctorate from Ohio State in 1935 and APA Fellow, Dr. Stanton is also a trustee of The Center for Advanced Study in the Behavioral Sciences and of The Rockefeller Foundation as well as Board Chairman for The RAND Corporation in Santa Monica. In 1961 he was awarded the George Foster Peabody Public Service Award for his efforts to bring about the "great debates" on television. He is co-author, with Paul F. Lazarsfeld, of several books on radio and communications research.*

ALMOST a third of this 55-page booklet is given over to pictures of children which have no discernible relation to the text. In the remaining pages the editor attempts to examine such broad

and complex topics as the production of television programs; adult misconceptions about children; the "intellectual, social, and emotional" development of children from infancy to adolescence; certain effects of television with emphasis on "incidental learning"; effects of particular program content and production techniques; and the need for broadcasters to make certain "choices" and assume certain "responsibilities" in regard to programming for children.

The editor fails to deal responsibly and adequately with this wide range of subject matter primarily because the brevity of the text precludes the documentation and discussion required of a serious work. Instead of a logical development of significant points, the highly compressed text emerges as a nebulous collection of far-reaching declarations, most of which are presented without qualification or sufficient exposition.

Lack of space would have been less of a problem had the editor chosen to run more text and fewer pictures. The need for 17½ pages of pictures of children in various activities and moods—none except the cover picture related to television—is not clear. Whatever the reason, they do not contribute to the scholarly quality of this book.

Because of its nebulousness, *Television for Children* defies classification and evaluation; and one is inclined to wonder, when comparing the pamphlet with several much more extensive works in the same area, what purpose it can serve. For the serious researcher it contains little that is new and specific; for the broadcaster it contains little information that is developed fully enough to be of practical use; and for the lay reader, its lack of detail, documentation and exposition is likely to result in confusion and misconceptions.

THE character of *Television for Children* is further weakened by its origins and manner of preparation. The booklet states that the material in it was selected from the transcriptions of meetings in which scholars discussed the topic of television for children. The information, according to the text, was selected and prepared for distribution to "a broader



professional audience" by Ralph Garry, Professor of Education at Boston University. As a result of this process, *Television for Children* cannot be regarded as a definitive report on specific research findings; nor is it clear whether the statements it makes represent a consensus of authoritative opinion, individual opinions or untested hypotheses.

In fact, no study, finding or authority is identified throughout the booklet except for the list of 19 individuals who participated in an indeterminate number of meetings. There is no indication as to which of these authorities took part in the meetings actually transcribed. There is no way of telling whether the material is a product of empirical research, or of disciplined observation, or merely of exploratory, informal discussion.

Even where references are made to "research" or "evidence," the source is not identified. This is of particular concern when the booklet makes such sweeping but undocumented claims as the statement on page 33: "There is ample evidence that television is an excellent teacher. In this instance it has the power to combine the teaching of violent behavior with the fanning of aggression. Recent research indicates that viewing violent behavior in television film increases aggression levels in children, and the higher the initial level the greater the effect."

The editor of this booklet may feel that he has qualified this statement by including the following on page 28: "Much of the discussion about television and aggression in children suffers from oversimplification. Although much painstaking research is needed before our knowledge is adequate, enough is now known to suggest some of the complexities." However, separating the claim and the qualification by five pages greatly reduces the effect of the qualification and leaves the impression that the statement represents established fact in an area where scientific consensus has by no means been attained.

The practice of making far-reaching claims without documentation, and then either not qualifying the claim or qualifying it only indirectly or in some other passage is characteristic of *Television for Children*. On pp. 35-36 the booklet



TELEVISION FOR CHILDREN  
(THE COVER PICTURE)

says in regard to what children learn from television: "The range of this incidental learning is great and not immediately manifest. Much is likely to be stored up to be used as the occasion arises." On page 38 the booklet states in regard to television drama: "The immediate rewards of getting the money, or owning the ranch, or running the town, are more impressive than the delayed punishment heralding the ending seconds of the program." Also on page 38 there is the statement that children "develop concepts of various occupations—doctor, lawyer, scientist, railroad engineer or pilot—which influence their interests and aspirations. Evidence indicates that these stereotypes created in the absence of real experience resist change."

Professional researchers, familiar with the literature, may be reminded by some of the statements in the booklet of studies which bear on such topics as the effects of violence, the degree and nature

of incidental learning and the likelihood of its being stored up, the relative impression made on the child by immediate rewards and delayed punishments, and the likelihood of stereotypes of occupations being developed and being resistant to change. But the lack of citations makes it impossible for the researcher to check or evaluate the claims the booklet makes, or to weigh them against other and possibly conflicting findings.

THE difficulties in trying to reconcile, qualify, develop and present transcriptions of discussions as meaningful printed text are illustrated not only by the superficiality of this booklet, but also by what appear to be examples of fuzzy thinking and confusion. For example, it is stated on page 45 that "children like their characters to be sharply etched in black and white, for the shadings of grey are difficult for them to understand." Four pages later the booklet proposes that,



to relieve tension, the "villain and his cohorts . . . be characterized by a slightly clownlike aspect . . . which makes them slightly ridiculous" (page 49). But on the same page it is stated that "characters, adult or child, in documentary or drama, should be real . . . stock characters lack conviction." It would tax even the magic of television to create villains who are clearly black and slightly ridiculous, but nevertheless realistic and not like stock characters.

The unscientific character of this booklet is further reflected in the "solution" discussed in the last chapter, "Choices and Responsibilities." Drawn from a report of a Senate subcommittee investigation of comic book publishers, the idea is advanced that each individual in the chain of production and distribution should take on his own shoulders the burden of judging and rejecting material he considers as undesirable for children.

This is obvious nonsense. To have every cameraman, stagehand, director, actor, writer, producer and executive along the production line applying his own rule-of-thumb judgment based on what he happens to think is "good" or "bad" for children could result only in chaos. What we need is not more opinions and judgments by unqualified laymen, but rather more solid, definitive research.

Broadcasters are contributing to the expansion of research through cooperating with the Department of Health, Education and Welfare in furthering study of the effect of television viewing on children. NBC has retained a psychologist to advise on children's programming; and CBS has created an office of social research headed by an eminent sociologist to pursue and encourage future studies. CBS, too, through a grant, has helped make possible a study of audience reaction by Columbia University's Bureau of Applied Social Research (*The People Look at Television*, publication date: February 25, 1963).

In the interest of more light and less thobbing it might be urged that the research on which the claims and implications in *Television for Children* are based be identified. The findings could then be properly evaluated and interpreted by professional researchers, broadcasters and the public.

## ON THE OTHER HAND



### A RESEARCH MASK?

The review by Eli Bower of Haring, N. G., and Phillips, E. L., *Educating Emotionally Disturbed Children*, (CP, Feb. 1963, 8, 62) is as emotionally disturbing as the book itself.

There is certainly room for *opinion* on the question of "permissiveness" versus "structure" in the classroom full of emotionally disturbed children. Some *research* into the topic would be most significant and useful. There is, however, good reason to raise serious objection to the use of Haring and Phillip's "research." The "anti-permissive" forces need not be given this kind of pseudo-support.

Bower reports extracts from the "study." He indicates that Group I, the experimental group (the one subjected to the "structured approach") had an *N* of 45. It did not. It was a class of 15 children, which was compared to two other classes, each of which had an *N* of 15. (No other demographic data on the groups are reported in numerical fashion.)

Bowers goes on to say that tests of academic achievement and behavior ratings showed the experimental group to be "more constructive and tractable . . . academically more proefficient, behaviorally more organized, more goal oriented." None of these statements are actually supported by the "study" reported in the book. The book's authors indicate that the Group I people gained 1.97 years of grade level, compared to a gain of 1.02 and .70 years, respectively, for Group II (regular classroom, disturbed) and Group III (permissive classroom, disturbed). We note, however, that Group I, on the pre-test, was at the 2.19 grade level, compared to the pre-test grade levels of 3.03 and 5.36 for Groups II and III. We also see higher variance in gain scores for Group I. If, then, one wishes to say that one grade level between grade levels 2 and 3 is equivalent to one grade level between grade levels 5 and 6; and if one wishes to ignore the differences in variance, he might be willing to accept the reported results as

"proof" of greater academic gains for the "structured" classroom.

Haring and Phillips offer numerically-based evidence for Group I's gains in the non-academic areas by referring to results obtained from a behavior rating scale. (Although a reference in the text sends us to Appendix C for "A Proposed Rating Scale . . .", the text's description of the rating scale used in the study does not correspond to what we find in the Appendix. Why the discrepancy?) The suggested gains no doubt encourage Bower to attribute positive-valued behavior change to Group I. Our first concern, as might be any scale constructor's would center around the legitimacy of combining items of the type given in the book's Appendix into a composite score. I can see no point in carrying on a further discussion of results obtained from this "scale" before this concern is dissipated.

Haring and Phillips should be free to express their *opinion* on the need to introduce more "structure" into the education of disturbed children—the concept of "structure," after all, seems to be gaining popularity and psychologists need not always be the "bad guys." But why does their opinion need the illusory backdrop that their "research" provides? Why did the reviewer support their illusion?

JAMES C. MANGUSO  
State University of New York at Albany

### IT TAKES ONE TO KNOW ONE

The problems of a psychologist in *full time* private practice are entirely different from those people who are in part time practice or engaged in such pursuits as a "side line." Although it is not necessary to have been or to be psychotic to understand psychotics, it is my opinion that in order to understand and make a judgment of a book concerning full time private practice the reviewer should be conversant with the problems of a private practitioner, and really the only way to be conversant is to have practiced full time. Siple's review of Molly Har-



rower's book *The Practice of Clinical Psychology* (CP, Oct. 1962, 7, 368-9) is a case in point. Molly Harrower is a respected clinician and knows the problems involved in the full time practice of psychology. Whatever may be the professional virtues of Siple, he has never been in full time private practice and consequently is really not justified in making such statements as "The reader will also be made aware that these practices are not a model of what private practice should be" (referring to the practice of a group of psychologists in New York City). He further goes on to say "It does not ask the would be practitioners to think seriously about the fact that research has not given us a sound basis for most of the things we do in private practice. It does not tell us how and in what kinds of problems our personality theories are more or less adequate for understanding the range of human problems which come to us. It does not ask us why, when we attempt psychotherapy, we fall back more readily on our basic humanness than on a firm body of scientific knowledge, nor what we should do about it." (All mine.)

I presume that the reviewer uses the first person plural in the sense of a full time private practitioner. If this is the case, a false impression is created. Most local organizations devoted to the welfare of psychologists in private practice consider as private practice 50% or more, full time and a judgment of various problems therein involved discussed by Harrower such as "cash on hand, community needs, relations with other professions" can only be made by someone who has experienced them. There is a great deal of controversy about this, but I think most people in full time private practice adhere to this viewpoint.

HAROLD GEIST, *Chairman  
Bay Area Psychologists in Private Practice*

#### ERRORS IN PAPERBACK LIST

We note with interest the listing of current paperbacks in Psychology in the January, 1963 issue of CONTEMPORARY PSYCHOLOGY.

We feel obliged to bring to your attention errors appearing in the listings of *Random House Studies in Psychology*. They are as follows:

English, H. *The Historical Roots of Learning Theory*. \$.95—listed as \$.85

Fulau—should be Eulau—also wrong number PS37—should be PS42.

Robert Plutchik's *The Emotions* wasn't listed.

Muus—correct spelling is Muuss—wrong number PP21, should be PP22 and gave incorrect price as \$1.45—\$1.25 is the correct price.

On page 12 you list *Papers in Psy-*

*chology*, Random House, 457 Madison Avenue. The correct listing is *Random House Studies*, 501 Madison Avenue.

ROBERT WEISS,  
*Random House, Inc., Alfred Knopf, Inc.*

## Recent Russian Books in Psychology

Compiled by JOSEF BROŽEK

Lehigh University

with the assistance of

STEFAN SLAK and AKIYO NISHINO

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(Acquisition of these publications was facilitated  
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Contemporary Psychology: A Journal of Reviews is published monthly. The subscription per year is \$10.00, foreign \$10.50, single number \$1.00.

Published by the American Psychological Association at 2210 W. Dallas, Houston, Texas and 1333 Sixteenth St., N.W., Washington 6, D. C.

Second-class postage paid at Houston, Texas and at additional mailing offices. Accepted for mailing at special postage rate stated in section 132.122, Postal Manual.

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# Contemporary Psychology

A JOURNAL OF REVIEWS

VOLUME VIII

July, 1963

NUMBER 7

## Beyond Empiricism

James G. Taylor

*The Behavioral Basis of Perception.* New Haven, Conn.: Yale University Press, 1962. Pp. vii + 379. \$8.50.

Reviewed by CARROLL C. PRATT

*The author, James G. Taylor, did graduate work at the University of Aberdeen, later worked with C. S. Myers, and, since 1924, has been at the University of Cape Town, South Africa. The academic year 1962-63 he spent on sabbatical at the Psychoacoustic Laboratory at Harvard. Carroll Pratt, the reviewer, is well known to CP's readers, especially those who keep up with books on perception. Recently retired from a Professorship at Princeton, he reports 1) that everybody thinks he has time for everything, 2) that he has no increased capacity to say no, 3) that he has no leisure at all and 4) that he thinks he will unretire next year and go back to teaching.*

NOT LONG ago I had occasion in these pages to express regret that the author of an excellent book on perception (CP, Feb. 1963, 8, 38-9) had taken a theoretical stand midway between two extremes—a safe and perhaps correct position, but one that lends itself neither to serious objection nor to profitable attack from experts. Professor Taylor by contrast has taken a position so bold and extreme that his views will almost certainly arouse lively and valuable debate.

Taylor will have little to do with empiricism, and nothing at all with nativism. It is perhaps unfortunate that he has helped to keep the words "nativism" and "empiricism" in circulation, especially since he gives them somewhat unexpected meanings. Nativism for him seems to be identified with consciousness and special creation, and empiricism with an anemic behaviorism constantly on the defensive against infiltration by primitive forms of consciousness. But that's all right. "When I use a word," said Humpty Dumpty, "it means just what I choose it to mean—neither more nor less."

The book is painstakingly put together and represents years of dedicated labor on the part of the author. It deserves careful study not only by students of perception but also by everyone concerned with the top reaches of psychology, for Taylor in working out his behavioral theory of perception has really built a system of general psychology at a high level and has been courageous enough to deal with some of the broad philosophical implications of his point of view. Readers may be bothered by his use of the symbols and notation of set theory. The going is rough in places, but in view of the impressive contribution Taylor

has made to psychology it is important to understand exactly what he means. Perhaps the author will consider the preparation of a new edition in which he will use ordinary English words with whatever meanings he chooses to give them, and then like Humpty Dumpty insist that they mean neither more nor less than he says. Such an edition would surely be read by a much larger number of psychologists and philosophers. It could also be printed at smaller cost, a matter of some concern to those scholars who do not belong to the small fraternity of affluent professors.

Taylor regards Gibson as the most distinguished of present-day exponents of nativism and offers an engaging and ingenuous footnote in this regard. "The gap between our theories is aptly expressed in a remark Gibson made to me in conversation: 'You are more wrong than anyone else I know.' Presumably what he meant was that whereas his other opponents make some concession to nativism, I make none. Others are wrong in part, in that they claim a larger share of the territory than Gibson is willing to concede to empiricism; I am the most wrong because I claim the whole territory and would leave none to him" (p. 301).

THE TERRITORY claimed by Taylor includes about everything: (a) selected aspects of the environment, (b) afferent impulses set into operation by the stimuli, (c) afferent functions produced by the posture of the body and the orientation of the receptor organs, (d) innumerable stages of efferent events, (e) drive functions brought about by the needs of the organism, and most important of all, (f) the mechanisms of conditioning that together with (g) appropriate engrams reduce the needs or the critical state





HORTENSE POWDERMAKER

introduced in this study of cultural change are predominantly issues at the sociological level of analysis, and when psychological issues are raised they are treated in terms of a slightly watered-down version of the anthropologist's favorite psychological framework, psychoanalytic theory.

THE SPECIES of psychoanalytic theory employed here is identified by the author as that of Erik Erikson. For better or for worse (other readers may decide for themselves) Erikson's ideas are employed neither conscientiously nor with rigor. Indeed, about all of Erikson to be found in the book is the notion of ego identity, which is introduced all too briefly in an otherwise excellent introductory chapter dealing speculatively with social and individual change. Ego identity doesn't reappear until some 250 pages later (and then in amended form), where Powdermaker makes an interesting attempt to account for individual differences in acculturation in terms of differences in ego strength. Behavioral change in the direction of Europeanization is there hypothesized to require "an ego sufficiently strong to accept the risks and anxieties which always accompany change." It is further hypothesized that the "intransigents," or those who continue to manifest traditional behavior-patterns in the face of opportuni-

ties to become literate, to engage in new (European-like) forms of social relationships, etc., are persons who lack sufficient ego strength to enter into a way of life in which rewards are uncertain. No stringent test of this provocative hypothesis is presented, but some generally supportive evidence may be inferred from the reported tendencies of the intransigents to drink heavily and otherwise engage in non-productive but possibly anxiety-reducing or escapist activities.

The major scientific value of this book is not to be found in tests of hypotheses, however. Rather it lies in the illustrative accounts of significant behavioral events, covering a broad spectrum of situations, which are reported with meticulous detail. Lengthy conversations recorded in settings where traditional and "modern" social forces co-act are reported as nearly verbatim as possible. The history of a miners' strike is told vividly through discussions among individual strikers, whose various interpretations seem to reflect their diverse cultural orientations. Similar treatment is given to an interaction between a husband and wife, concerning the wife's temporary return to her tradition-dominated village to visit her family of origin, to several disputes among mining town residents over alleged adulterous activities which seem more likely to occur in the mining town than in traditional homesites, and to comments among members of an outdoor movie audience, some of whom seem easily to identify with the cowboy hero, while others loudly decry the distortions of reality produced by this European magic.

These accounts are more than fascinating vignettes of contemporary, urban African life. They serve as vivid illustrations of the pressures of rapid cultural change and the diverse reactions to those pressures. The individual differences in response to those pressures cry out for investigation by the psychologist who purports to be interested in relations between culture and personality variables. For an accurate picture of the cultural context within which African personalities today interact, and for provocative hypotheses about those interactions, this book is an excellent source.

## Very Medically Speaking

Nathaniel M. Levin (Ed.). Foreword by Mary E. Switzer

*Voice and Speech Disorders: Medical Aspects.* Springfield, Ill.: Charles C Thomas, 1962. Pp. v + 996. \$27.50.

Reviewed by JOSEPH M. WEPMAN

The editor of this volume, Nathaniel M. Levin, is Clinical Associate Professor of Surgery (Otolaryngology) at the University of Miami School of Medicine. Also, among other things, he is director of a post graduate course in esophageal speech and organic voice problems at the University of Miami. The reviewer, Joseph Wepman, is a clinical psychologist and student of many aspects, both normal and pathological, of the linguistic process. He presently bears the improbable but distinguished title of Professor of Psychology and Surgery at the University of Chicago. Among his many excellencies is his frequent willingness to review for CP. His most recent production appeared in the February 1963 issue.

THE EMPHASIS on the psychogenic nature of so many of man's problems including those of communication, places in rather bold relief this volume which is more or less dedicated to the structural and physiological nature of the tools of expression. It has been a long time since this reviewer has seen a book devoted to the anatomy of speech and to its counterparts, voice and language. Dr. Levin, a noted otolaryngologist, best known in the field of speech and audiology perhaps for his sponsorship of annual training courses for teachers and therapists interested in esophageal speech, has served in this instance as a rallying point for the medical viewpoint on a variety of speech and hearing problems. As is true of most compendia, there is a tendency for unevenness in presentation in the present encyclopedic venture. This, your reviewer believes, is to be



expected. Some "experts" write better than others, and 36 experts could not be expected to be equally lucid in their expositions. This volume, however, has it appears to me, some additional problems, not brought on by the uneven quality of its contributions, but by what seems to have been a compulsion to include too many contributors in a single book.

Your reviewer, after scanning the multifaceted contents, lapsed into a state of fantasy. The manifest content of this dream placed him as the editor of a volume devoted to just such a book as this. Originally, the idea was to edit a book by physicians who understood from their studies the basic anatomy of voice and speech. The audience was to be other physicians interested in this complex problem. But basic principles and medical viewpoints and even the select audience faded as other aspects of the problem clamored for attention. From medical aspects, this fanciful book went on to paramedical aspects, from structure to therapy, from concrete statement to abstraction, from training to surgery, from disorders of voice and speech to a little bit of the public speaker and the professional singer. Chapters kept getting added about subjects of less and less relationship to the original purpose until the editor simply gave up and added chapters of no relevance at all. At this point in my reverie, the book fell off my desk and landed open at a page concerned with Delayed Verbal Language where under the heading *Neurologic Damage* (as a cause of the condition) the text began:

"Injury to the central nervous system may be so devastating that infant, child or adult cannot survive."

This Aristotelian non sequitur seems to set the pace for a taxonomic listing of almost-anything-can-happen-after-this which may cause delay in the acquisition of verbal language. Certainly the prime condition—a lack of survival—produces a considerable delay for the "infant, child or adult" concerned.

THE BOOK, where it maintains its original purpose, is excellent; its con-

tributors are modest, and its erudition notable. For an audience of physicians and surgeons, chapters on surgery of head and neck, surgery of maxillofacial oropharyngeal areas, and surgery of the larynx, trachea and neck, modern surgical correction of otosclerotic deafness and tympanoplasty all seem quite appropriate. Your reviewer objected somewhat to an interlarding of these fine technical discussions with other chapters on "the voice of the speaker and singer," "the professional singer," and "building correct singing habits." Further, the inclusion of chapters on disorders of articulation and stuttering seemed to indicate a strained determination to say something about everything in speech. Finally, the inclusion of a section on "Habilitation and Rehabilitation" without reference to either medical aspects, voice or speech disorders, took the book quite out of its original cast and made it another over-reaching attempt to include something for everybody.

Perhaps the most glaring deficiency in the book is that though its length is more than ample and space appeared to be no problem, there is an almost total lack of attention to the psychiatric or psychologic problems of voice and speech. Some passing lip service is offered in many chapters, but even in the one devoted to "Functional Disorders of Voice," one finds the statement "functional and/or organic factors, technical problems and nervous and emotional imbalances are present in each of these cases, in varying combinations."

Lest the reader get the impression by the length of these criticisms that the book is valueless, let me hasten to correct him. There are many good chapters, some appearing in areas never before covered in a comparable volume.

Examples are Luchisinger's chapter on "Voice Disturbances on an Endocrine Basis," Levin's chapter on "Laryngeal Paralysis, Stenosis and Webbing," and Robert's fine chapter on "Dysphasia from a Neurological Viewpoint." In fact, most of the 'medical aspects' chapters are good ones and will add to our knowledge. This reviewer just wishes the editor could have contained himself and stuck to his purpose. He

would then have had a fine book. As it is, the valuable parts are likely to be lost in the welter of the unimportant.

## Compendium on the House

Daniel M. Wilner, Rosabelle Price Walkley, Thomas C. Pinkerton and Matthew Tayback. With the assistance of Marvin N. Glasser, John M. Schram, Carl E. Hopkins, Charles C. Curtis, Alan S. Meyer and Joseph R. Dallas.

*The Housing Environment and Family Life: A Longitudinal Study of Housing on Morbidity and Mental Health.* Baltimore: Johns Hopkins Press, 1962. Pp. v + 338. \$7.50.

Reviewed by KURT W. BACK

All of those who participated in the creation of this volume were at one time associated with the School of Public Health at Johns Hopkins University. Daniel Wilner, the psychologist who directed the study reported here, has since moved across the continent to become Professor of Public Health and Associate Research Behavioral Scientist at the UCLA School of Public Health. Rosabelle Walkley is now a lecturer at the same institution. Thomas Pinkerton stayed at Hopkins as a member of the Department of Biophysics and Matthew Tayback is Assistant Commissioner for Research and Planning of the Baltimore City Health Department. The reviewer, Kurt Back, is, as he was the last time he reviewed for CP (February 1963) and as he is elsewhere in the present issue; Professor of Sociology and Associate Professor of Psychiatry at Duke University. His most recent book, *Slums, Projects, and People: Social Psychological Problems of Relocation in Puerto Rico*, is reviewed on p. 270.

AMONG all the contributions to social welfare of the community, the provision of adequate housing has al-



ways had a central place. This is, of course, based on the assumption that housing will have some effect on health, life satisfaction, and social conditions of the people involved. What these effects were has never been proven conclusively, and the recent sentimentalizing by successfully mobile persons of their slum childhood has even put the original assumptions in doubt. The study reported in this volume is an attempt to establish in controlled conditions the effects of housing on selected parts of family life.

The background and design of this study are impressive. It was planned by committees of the American Public Health Association and the National Association of Housing and Rehabilitation Officials in cooperation with the local Housing and Health Department and the Johns Hopkins School of Hygiene and Public Health. The authors represent several disciplines: psychology, statistics, and public health. The design of this study was ingenious. Successful applicants for a newly-built housing project were matched with other applicants who were the next alternates. They were well matched on important characteristics as well as on their desire to get adequate housing. The latter factor resulted in the condition that some of the control families secured better housing during the experimental period, but this difficulty was met by an ingenious system of reducing the original sample size so that there was finally an effective reduced sample of 300 experimental and 100 control families. These families could be followed up through interviews over a period of three years. Sample design, interviewing, and processing of data were all carried out with extreme care. Even the review of other studies was done on a careful sample basis, and the data were summarized. The first three chapters of the book, describing the background of the study, are a model of description of the design of a field experiment.

The remainder of the volume describes the data in the different fields of investigation. On each question of the interviews and on each item of the records being checked, we find comparison of the experimental and control

groups, either before and late, or before and in the middle point and late. The results show that move to a housing project is not a panacea for all ills; however, there are some significant differences in favor of the experimental group. The facts, impossible to summarize further in this small space, are all there. They are presented lucidly in the tables and are described briefly at the end of each chapter. Beyond that—nothing.

As the dust-jacket states, "because of its dimensions and its cost—\$500,000—this experiment is unlikely to be duplicated soon." In this case it is legitimate to query why no analysis beyond the original breakdown of before, middle and after, experimental and control, was attempted. There is no internal analysis and there are no additional controls. Thus no interpretation is possible and no connected picture emerges. The reason for this failure is unlikely to lie in the authors, who have shown their ability in their work in other contexts. It seems to be a function of the particular research set-up under which the study was conducted. The multidisciplinary staff, the diverse advisory committees and sponsoring committees, almost guarantee that the finished product will lack direction. If it had been a psychological, sociological or statistical study, conducted within the framework and control of a discipline, it would have investigated competently the influence of the housing situation within a specific conceptual framework. The group set-up guarantees that there will be minimal testing of individual ideas and that the contributions of each discipline will represent the lowest common denominator. The psychologist describes the differences in the answers to question after question, and the statistician notes that the t-tests are computed with the aid of nomographs, making the book no more than compendium of fact. It has value as a reference, but one could wish that the authors had used a fraction of their ingenuity in analyzing the data.



*Hunting is not all catching.*

—RUDYARD KIPLING



## Two Second Views of Educational Psychology

Henry Clay Lindgren

*Educational Psychology in the Classroom.* 2nd Edition. New York: Wiley, 1962. Pp. vii + 574. \$6.95.

Glenn Myers Blair, R. Stewart Jones and Ray H. Simpson

*Educational Psychology.* Second Edition. New York: Macmillan, 1962. Pp. v + 678. \$7.00.

Reviewed by HERMAN ROEMMICH

Henry Clay Lindgren, the author of the first book, is a Stanford PhD who is now Professor of Educational Psychology at San Francisco State College and author of many articles and books, the most recent of which is *Psychology: An Introduction to the Study of Human Behavior* (with Donn Byrne, 1962). All three authors of the second book are at the University of Illinois and all are Professors of Educational Psychology there. The reviewer, Herman Roemmich, received his PhD degree from the University of Washington and is now Professor of Education at San Diego State College and also Test Officer of the college. He has both taught and administered in secondary schools, has worked on training evaluation for the U. S. Navy, and has taught in the Education Departments of a number of universities.

THE WRITER of an educational psychology text faces important choices regarding content, emphasis, and approach. Whatever his choices, the result should be a book about psychological principles and facts as they relate to the process of education.

Lindgren's choices in the selection of topics and emphasis clearly aim to reflect the current trend, emphasizing social and emotional factors in learning. His concentration is upon the application of psychology to classroom learning. The selected references are germane to the discussion, but do reflect



the point of view of a clinician. In spite of his focus on application, Lindgren writes about "psychology."

Blair, Jones, and Simpson select topics according to their applicability to education. Topics are clearly separated into such traditional headings as growth and development, learning, adjustment and mental hygiene, etc. Basic studies in education and psychology are cited. References selected support the viewpoint that both biological and sociological forces influence the growth process. The discussion of growth, development and motivation is especially good in presenting and integrating psychological facts and principles. These strengths are not sustained in later chapters. Parts four and five appear to be more about education than about psychology.

The omission of references to Guthrie and Skinner in part three, on learning, appear serious in Blair, Jones, and Simpson. Also references to studies within the last five years are few, yet a stated objective of their book revision is to incorporate new research and recent advances.

Both texts include actual classroom examples for illustration and application. Lindgren manages to integrate these more smoothly, and his selection of examples appear more appropriate. The problem of organizing and integrating material from several contributors is apparent in Blair, Jones, and Simpson, especially in later chapters.

It appears especially pertinent that in both texts the authors reflect the conviction that effective teachers need to be behavioral scientists. Further, the recognized importance of personality variables in students and teachers in classroom learning, in both texts, support the notion that teaching children is of great concern to both educators and psychologists.

Advanced students with background courses in psychology will find the applications discussed in Lindgren valuable and stimulating. Beginning students will find Blair, Jones and Simpson more comprehensive than present texts in providing basic information on growth and development. The discussion of other topics in the latter book, especially measurement and evaluation, is disappointing.

## Pre Oedipal Confrontation

Harry Guntrip

*Personality Structure and Human Interaction: The Developing Synthesis of Psycho-Dynamic Theory.* New York: International Press, 1961. Pp. 456. \$7.50.

Reviewed by JOSEPH ADELSON

*The author, Harry Guntrip, is a British psychologist who is further identified in the review. Joseph Adelson, the reviewer, is Associate Professor of Psychology at the University of Michigan and serves besides both as coordinator of the clinical training program and as Assistant Chief of the Psychological Clinic. His recent writing includes a piece "The Teacher as a Model" appearing in Nevitt Sanford's The American College (CP, Nov. 1962, 7, 363), and a book with E. Douvan, Themes in American Adolescence, soon to be published by Wiley.*

MANY of us would agree that three major directions have emerged in psychoanalytic theory since Freud's death: the elaboration of ego psychology, as in the work of Hartmann, Kris, Rapaport, and others; the epigenetic theory of Erik Erikson, with its emphasis on universal psychosocial dilemmas; and the "internal-objects" school, centered in Great Britain, and represented most forcefully in the writings of its leading figure, Melanie Klein.

The first two of these have been developed in this country, and are familiar to those American psychologists who keep up with psychoanalytic thought. But the Kleinian revolution—and that is not too strong a term—has proved peculiarly non-exportable. If we think of Melanie Klein at all, we think of her as one of those exotic, intemperate creatures who appears on the psychoanalytic scene every so

often. We first boggle and are then transfixed by her more startling ideas—especially by the contents and capacities she imputes to the infant mind. Yet these dubious and uncomfortable ideas are in some respects beside the point; she has made an enormous impact on psychoanalysis not because of them but despite them; if we allow ourselves to get behind these ideas we find an original and provocative vision of human development. It is a vision we may finally reject, but we first ought to know what it is. As matters now stand, the work of the English school—Klein, Fairbairn, and others—remains unknown in this country, and when known ignored, and when not ignored then patronized. We have not yet confronted it, and it is time we did.

One reason we have not confronted this work is that no general introduction to it has been available. The ideas of the English school have stirred so much controversy that many of the key writings have reflected the heat and tendentiousness of disputation; their tone tends to be polemical, strident, ingrown. The curious outsider, eager to know what is going on, feels himself sometimes to be a witness at a long-standing family quarrel, in which both the tacit assumptions and the accumulated grievances are known to the arguers, but not to himself. So we have long needed a book which would provide a conceptual and historical context for the English school, which would undertake to trace back its sources and spell out its implications. That is the book Dr. Guntrip has now given us.



Guntrip is a British psychologist in the psychiatry department of the University of Leeds. He came to psychology from a background in religion, philosophy and the humanities. The issues he encountered in these fields led him to the psychoanalytic literature and, in time, to the writings of W. R. D. Fairbairn, with whom he undertook a personal analysis. To say that this book reflects Fairbairn's influence is to understate the case considerably, for what we have here is nothing less than a partisan history of psychoanalysis, written in the conviction that Fairbairn's work is the culmination of all previous effort, the peak to which we have been laboriously ascending. Let me state the worst first—Guntrip apparently cannot bring himself to imagine that Fairbairn has ever been seriously mistaken about any matter of importance; and long (and exasperating) portions of this book are devoted to proving it. In short, a true believer; in short, a panegyric.

All this may make the book sound unpromising, if not unnerving. Yet strangely enough, Guntrip's violent partisanship, though tedious and annoying, does not seriously diminish the book, which must be accounted an extremely impressive achievement. It is alive with viewpoints, arguments, analyses; very few of the central issues of current psychodynamic theory remain untouched, and since Guntrip's approach is not generally known in this country, what he has to say will be fresh and exciting for most American readers. Even his partisanship works for him, in that it gives his book structure and direction, and his prose a bite and clarity in sharp contrast to the sodden shilly-shally of so much psychological writing today.

**G**UNTRIP divides the history of psychoanalysis into three major phases. He argues that Freud's early writings were not truly psychodynamic. Freud's background in the natural sciences disposed him to develop a "process theory," i.e., an impersonal theory of mental functioning, as against a "personal theory," one which stresses the individuality of the self in human relationships. (The distinction is originally Marjorie Brierley's.) This background also led Freud to stress instincts and their regulation

as the key phenomena of mental life, and to neglect the development of a theory of the total personality. As a result, certain aspects of human behavior—above all, sexuality—were overaccentuated, while such significant problems as interpersonal relations tended to be ignored.

These weaknesses in Freud's "psychobiological" theory produced the counterrevolution of the second major phase, in the appearance of the "culture-pattern" analysts—the most prominent of whom were Horney and Sullivan. These writers recognized the inadequacies of the instinct theory and argued that the critical determinative influences for personality formation were to be discovered in the child's experiences with significant others. But Guntrip feels that they threw out the baby with the bath water, abandoning not only the instinct theory, but along with it the very concept of a deeplying unconscious. They settled for a superficial, retrogressive view of human motivation.

The third phase, the synthesis of the two earlier ones, is foreshadowed by Freud's elucidation of the structural theory of personality in the 1920's. In positing ego and superego as precipitates of object-ties, Freud came to sense the primacy of the interpersonal in the formation of psychic structure. But he was too wedded to instinct doctrine to move toward a thoroughly object-oriented approach. Instead he superimposed the new structural theory upon the now outmoded drive theory. It remained, then, for Klein and finally Fairbairn to make the clinical and theoretical discoveries which have eventuated in a genuinely psychodynamic point of view. Klein penetrated beneath the Oedipal era to the underworld of pre-Oedipal anxieties and fantasies; in doing so she encountered and recognized the central importance of internalized objects for the growth of personality. The infant's transactions with its objects, under the influence of an extraordinary innate aggressiveness, give rise to a complex endopsychic world of primitive terrors and (introjective-projective) defenses against them. The infant (to simplify) imputes its own sadism to its objects and then

feels persecuted by them; somewhat later, in the "depressive position," guilt and restitutional tendencies toward objects dominate the psychic life.

Guntrip goes on to argue that Fairbairn completed the reorientation of psychoanalytic theory begun by Mrs. Klein. Since about half of this fairly long book is devoted to an exegesis and defense (often shrill) of Fairbairn's very interesting ideas, one cannot attempt here more than a brief and inadequate summary. Guntrip believes that Klein, like Freud before her, could not altogether shed the commitment to an instinct theory. But Fairbairn does so; though he retains the libido concept, he stresses that libido is object-seeking. Thus the pleasure principle is subordinated to the reality principle, for "behavior must be oriented towards outer reality, and thus determined by a reality-principle from the first." Fairbairn departs from Klein in other significant respects: he places less weight on aggression as the motive-force in early object relations, emphasizing instead the positive side of object-seeking; and he is far more environmentalist than she, believing that the child's real experience with objects—the actual quality of the mothering it obtains—are more important than the unfolding of drives from within. Finally, Fairbairn believes that Klein did not develop a theory of ego structure to parallel her theory of internal objects; he proposes an elaborate formulation of the ego which is perhaps the most original contribution of a highly original thinker. To sum up: from Freud to Fairbairn, as Guntrip reads the history of psychoanalysis, we move from a theory of impersonal, pseudo-biological processes to a theory of the person, from the primacy of the Oedipus complex to the primacy of early dependency, from a stress on superego guilt to a recognition of primitive anxiety, and from an instinct orientation to a theory of object relations.

**T**HIS is a contentious, often irritating book; yet on the whole it is an admirable piece of work, admirable for the vigor and honesty with which it wrestles with terribly difficult issues. Its greatest



value for the American reader may well be its success in "normalizing" the English school. Our response to this school has been colored by dismay at its sometimes *outré* language and its sometimes flamboyant case material; we have tended therefore to dismiss Klein and Fairbairn as offbeat and far out. Guntrip's book makes plain that the contributions of the English school are very much in the mainstream of psychoanalytic thought, that they are, in fact, attempts to come to grips with the clinical and theoretical dilemmas that have bedeviled psychoanalytic theory in general for the last several decades. These dilemmas are in large part a consequence of the extension of psychoanalytic practice. As long as psychoanalytic therapy was restricted to the standard psychoneuroses and the milder personality disturbances, one could in good conscience view the Oedipus complex as the decisive experience of the human career. But changes in the scope of analytic practice complicated matters gravely. The use of play therapy with very young children; the expansion of psychoanalytic attention to the psychoses and the more severe character disorders; the lengthening and (presumably) deepening of analytic treatment even for the mundane neuroses—these developments brought us face to face with a pre-Oedipal world far more extensive and potent than had previously been imagined.

At least two key questions emerged: How can we construe the interaction and relative impact of Oedipal and pre-Oedipal experiences in the formation of psychic structure? And how do we revise our conception of psychic structure to take account of our newly-won knowledge of the pre-Oedipal realm? Conventional psychoanalytic writers have of course treated these questions, but it is fair to say that they have treated them uneasily and with excessive caution, unwilling to venture more than *ad hoc* revisions of psychoanalytic theory. The writers of the English school have grasped these nettles boldly. They assert the priority of the pre-Oedipal period, and have proposed a radical reconstruction of the structural theory which takes account of that priority. Their efforts may at

long last be having some effect; for example, we have recently seen some attempts at fresh thinking on early superego functions, attempts which acknowledge, however tacitly or grudgingly, a Kleinian influence. I suspect that in the long run we will absorb only some part of the "internal objects" viewpoint, for taken whole it sacrifices

or dilutes far too much of our hard-won understanding of Oedipal dynamics and much else. But again, that is somewhat beside the point. The writings of the English school are, along with Erik Erikson's work, almost the only new ideas we have coming out of psychoanalysis today. We ought to bestir ourselves and give them heed.

## Confounded Interactions, Balanced Lattices, Youden Squares and Plain F's

B. J. Winer

*Statistical Principles in Experimental Design.* New York: McGraw-Hill, 1962. Pp. v + 672. \$12.50.

Reviewed by ARNOLD BINDER

*The author, B. J. Winer, started out to be a journalist, but somewhere along the line succumbed to the delights of psychology and mathematics. Since 1960 he has been a Professor of Psychology and Statistics at Purdue University. The reviewer, Arnold Binder, normally bases his activity at Indiana University but during 1962-63 was to be found on the UCLA campus where he was a visiting professor. He did his graduate work at Stanford University where he met and was influenced by Quinn McNemar. He continues to teach courses in statistics and experimental design while at home as well as while visiting and will be the author of the chapter on statistics for the next volume of the Annual Review of Psychology.*

DURING the past summer, my colleagues and I at the System Development Corporation completed a reaction time experiment that involved a split-plot design with concomitant observation. Data analysis time brought with it a mild depression since previous attempts to find computational formulas

for related designs had not been successful. Before starting to work out these formulas, however, and without much expectation of success, I glanced through the newly arrived Winer book. There in Chapter 11 was a section of twelve pages devoted to the analysis of covariance with split-plot designs, complete with computational formulas and examples. That experience provided me with the brightest of hopes when, a few months later, I was asked to review the book.

The book is probably best described as a Cochran and Cox for psychologists. It is organized around specific designs rather than mathematical models, stresses the use rather than the logical bases of designs, and is developed more along the lines of traditional agricultural rather than traditional psychological statistics. The treatment is restricted to the analysis of variance except for a few isolated comments and an appendix containing nonparametric tests "closely related to the analysis of variance." One who has not had some solid introductory work in statistical inference will find the going difficult since knowl-



edge of such concepts as expected value, conditional probability, and normal equations is assumed.

The coverage of the book is extraordinary. A number of the designs, though useful in psychological research, are not often found outside of treatments in agricultural and industrial statistics. These include designs involving confounded interactions, balanced lattice designs, Youden squares, and hierarchal designs. Moreover the standard psychological experiments—simple randomized, randomized block, factorial, and split-plot—are covered most thoroughly. Such other incidental topics as orthogonal polynomials, the studentized range statistic, quasi F ratios, and the various methods of investigating the differences between means following an over-all F test (Newman-Keuls, Duncan, Tukey, Scheffé), are discussed and tables provided where relevant.

While reading various topics in Winer's book I had the typical sporadic feeling that some critical element, important extension, or logical relationship was omitted. But, contrary to this reaction on previous occasions, in almost every case I found the supposedly missing material in my subsequent reading. Not in the location I would have designated, to be sure, and not written quite in the way I would have done it, I daresay, but covered it was.

OF COURSE with general statistics in a state of flux and with the rumblings of revolt heard from the darkest statistical corners of the *Bulletin* and the *Review*, one might expect some opposition to a classical treatment like that of Winer. Some may believe that his discussions of statistical inference should have been based on the subjective probabilities of hypotheses rather than on the Neyman-Pearson decision procedure, others may object to his maintaining the distinction between *a priori* and *a posteriori* tests, and still others may argue that his emphasis on hypothesis testing rather than estimation implies an attempt to continue scientists as production workers. But the particular biases of this reviewer parallel those of the author on these points so one will have to look elsewhere for scintillating dissent.

One who is interested in the mathematical bases of the analysis of variance will not find the book too useful, although Winer does go beyond the stage of pure cookbookery in presenting the mathematical models for all designs, the tables of expected values of mean squares, and a bit of the logic binding these two ends. But that too much is not to be expected even in this limited way is demonstrated by the relatively disorganized treatment of F as a ratio of two independent chi-squares. He sneaks in a reference to the F distribution in the context of  $t^2$ , comments on F incidentally when dealing with the ratio of two variances, and then jousts about with the concept through the remainder of the book in a chaotic and sometimes irrelevant manner.

His frequent references to the work on robustness (insensitivity to the violation of assumptions) in the analysis of variance is a welcome antidote to the one-sided presentations of the non-parametric devotees. Other satisfying (at least to this reviewer) aspects of the book are: (a) a reasonably high level of redundancy of presentation—for example, the chapter on factorial analysis is followed by a chapter giving factorial computational methods and examples, with sections paralleling those of the preceding chapter, (b) examples of the various designs taken directly from the psychological literature, (c) an earnest attempt to show the relationship of the book's notational system to other generally used systems, and (d) a careful discussion of the issues involved in preliminary tests and pooling practices (material particularly close to the heart of this reviewer).

THERE are one or two points on the negative side which perhaps it would not be amiss to mention. First, there are certain discussions of important items which are not intelligible without a degree of effort not reasonable to expect from the reader (including, in some cases, perusal of source materials). For example, his discussions of the partitioning of interaction, of the special problems associated with factorial experiments having repeated measures, and of certain alternative procedures for making *a posteriori* tests stretch the limits

of comprehensibility. (However, in Winer's defense I would like to point out that comparable books in psychological statistics have avoided the difficulty only by omitting this particular material.) Second, Winer occasionally uses certain concepts (and, at times, extensively) paragraphs, pages, or even chapters before he defines them. And last, the assumptions behind the models are frequently stated in a haphazard manner so that the reader does not get a clear enough picture of the basic structure. As one example, probably only the most astute of readers will notice that there are normality assumptions for the effects of model II.

I wonder if Winer's attempt to include the three mathematical models (fixed, random, and mixed) as bases in the discussion of every applicable design was warranted. It made many aspects of the discussion cumbersome without much of a compensating gain. Students should be made aware of the existence of the various models, to be sure, but a good case can be made for restricting the discussion to model I except for a few limited extensions. Whether the levels were selected randomly or not is not really the critical issue since any set of levels may be regarded as fixed, no matter how obtained. More important are axiom considerations (such as additivity) and the consequences in terms of power and related theory. There is a great deal to be gained in terms of the structural simplicity of the interrelationships among various aspects of the analysis of variance by restricting most discussions to fixed models.

In concluding, let me say that my initial hopes in starting the review were fulfilled. While there are a few technical and organizational points at which one could carp, these are trivial in the overall contribution. As a textbook for graduate courses in psychological statistics, the book is outstanding, as a reference book for the practicing experimenter, it is superb.



*Investigator, look to your rhetoric. It is the show case of your investigations.*

—J. McV. HUNT







Once again CP's editor has somehow managed to bring under at least precarious control his urge to write off in all directions to everybody about everything. This accomplished, the present space can be turned over to two of the journal's more thorough readers who have something unusually factual to say about books and reviews.

—F. H. S.

## QUALITY CHARACTERISTICS OF BOOKS ON PSYCHOLOGY

By

NORMAN TALLENT and HENRY  
OPPENHEIM<sup>1</sup>

IF, as Boring has stated, "a science is its books" we can then hope to learn something about the current state of psychology from a study of book reviews. Annin (*CP*, Oct. 1958, 3, 299.) who was the first to shed quantitative light on the quality characteristics of psychological books, judged a number of reviews on a four point scale and concluded that "81.4 per cent of these reviews are thus favorable or mostly favorable." While it is also concerned with overall favorableness of reviews, the present study tries to specify some parameters of quality in psychology books and to do so in sufficient detail to be of value both to authors planning to write books and to reviewers searching for some possible criteria by which to evaluate them. In addition, these data should also contain some implications for the field in general.

## METHOD

The 115 reviews appearing in *Contemporary Psychology* from January to June 1961 constituted the sample for this study. They were simultaneously read by the two investigators, one reading aloud to the other. Each time the two agreed that the reviewer had made an evaluative statement, that fact was recorded on a card. To facilitate generalization beyond a particular context, and to avoid cumbersome direct quotes without losing the intent of the statement, the evaluative comment was generally paraphrased by joint agreement. The nature of the statements suggested certain categories and subcategories. These were established and the statements were then jointly sorted into them. To assess the stability of this classification scheme two simple tests of reliability were used. Two months after sorting, 200 statements were removed at random from the categories into which they had been sorted. Upon independent resorting, the level of agreement between the two investigators (interjudge reliability) was 80 per cent for the major categories and 71 per cent for the subcategories. With joint resorting (test-retest reliability) the

level of agreement with the original procedure was 95 per cent for the major categories and 87 per cent for the subcategories.

## RESULTS AND DISCUSSIONS

The sample yielded 1800 evaluative statements, a mean of 15.7 per review. The range was from zero (pure description) to 52. Of the total number of evaluations, 54.9 per cent were positive, 45.1 per cent negative. The significance of this difference was not computed since independence of reviewer judgments could not be assumed. Similarly, other relationships presented should best be regarded qualitatively or as trends.

These results are in general agreement with Annin but our findings are not as highly favorable. The main difference in the methods is that Annin's results were based on global evaluations whereas ours were based on the components of the reviews as reflected in our categories. We feel that probably another reason for the difference in degree of favorableness in the two studies is the "this is a good book phenomenon," so-called when a reviewer concludes with an unexpected upswing of tone after having expressed decisive negative evaluations throughout the review.

Six major categories and 14 subcategories were established. The percentages for the major categories are given in Table 1. These were suggested by the comments as parameters of quality in psychology books. For their additional and possibly heuristic value we will also present the subcategories into which we found it convenient to classify the statements.

TABLE 1  
EVALUATIONS OF BOOKS ON PSYCHOLOGY\*

Category	Favorable	Unfavorable	Total
I. Purposefulness .....	6.4	4.0	10.4
II. Selection of Content .....	27.6	17.3	44.9
III. Quality of Research Design.....	1.4	3.2	4.6
IV. Quality of Reasoning .....	7.2	9.2	16.4
V. Quality of Presentation .....	11.4	9.0	20.4
VI. Referencing and Documentation .....	.9	2.4	3.3
Total.....	54.9	45.1	100.0

\* Figures are per cent of total comments.

<sup>1</sup> From the Veterans Administration Hospital, Northampton, Mass.



I. *Purposefulness*. About one in ten of all the comments dealt with purposefulness. The comments seemed to fall further into three component subcategories: (a) worthwhileness of the author's goals (unfavorable comments predominated), (b) degree to which author meets his stated goal (unfavorable comments predominated) and (c) suitability of the book for special groups (overwhelming preponderance of favorable comments). All these contributed to the overall favorable estimate of purposefulness as presented in the table.

II. *Selection of Content*. Nearly half of the comments pertained to the selection of content. Two subcategories here were identified: (a) the quality of content (largely favorable) and (b) the comprehensiveness of content (unfavorable). Specific quality areas singled out for favorable comment were contribution to science, accuracy, appropriateness and orienting value. Inadequate coverage figured prominently as an unfavorable point.

III. *Quality of Research Design*. Less than five per cent of the total comments dealt with Quality of Research Design, and all these stem from a small number of books reporting original experimental material. Surprisingly, most of the comments were negative. Specific areas (subcategories) mentioned unfavorably were: (a) adequacy of techniques; (b) appropriateness of statistics; (c) correctness of statistical usage and (d) carefulness. Only one area, (e) that the design represented a methodological advance, received a preponderance of favorable comments.

IV. *Quality of Reasoning*. About one-sixth of the comments were concerned with the authors' quality of reasoning. Four subcategories seemed to deal with this category: (a) justification of conclusions and assertions (largely unfavorable); (b) quality of integration (mildly unfavorable); (c) clarity of reasoning (mildly unfavorable) and (d) compatibility with accepted points of view (favorable and unfavorable comments about equal).

V. *Quality of Presentation*. About one-fifth of the comments pertained to the Quality of Presentation. Two subcategories were identified: (a) style (favorable and unfavorable comments

about equal) and (b) use of appropriate aids to presentation (favorable). Concrete remarks were made about clarity of writing, organization, economy of presentation, comprehensibility, and appropriateness of tables.

VI. *Referencing and Documentation*. Only 3.3 per cent of the comments fell here. No subcategories seemed necessary. Negative comments prevailed, with lack of completeness, appropriateness and scope of referencing and documentation being especially mentioned. It should be pointed out that a few authors were chided for overcompleteness.

#### IMPLICATIONS

The six major categories established here represent an attempt to specify parameters of quality in psychology books. While a general regard for these dimensions by authors and reviewers should be of value, the implications of the unfavorable comments would seem to be of greater relative importance inasmuch as a modicum of adequacy in these dimensions may reasonably be expected as a matter of course. Many reviewers questioned both the worthwhileness of some books and the extent to which the stated goals of authors were met. One is tempted to ask of some books, "Was this trip necessary?"

It is also noteworthy in a science so thoroughly based on methodology that there should be a majority of unfavorable comments regarding adequacy of design and use of statistics. Although the findings may in part mirror the many unsolved methodological problems in our field, they further suggest that many psychologists who write books are not sufficiently trained in established techniques. The many negative comments made about justification of conclusions and assertions, and the insufficiency of referencing and documentation suggest inadequate responsibility and familiarity with their subject matter by an uncomfortably large number of authors.

These findings probably reflect in part the actual state of the field as well as the collective idiosyncracies and biases of the reviewers.

#### SUMMARY

Reviewers' comments appearing in six successive months of *Contemporary Psychology* were abstracted and classified. They were regarded as suggesting quality characteristics of books in psychology. Six major parameters were suggested by the data, viz.: (a) Purposefulness; (b) Selection of Content; (c) Quality of Research Design; (d) Quality of Reasoning; (e) Quality of Presentation; and (f) Referencing and Documentation. For additional specification component subcategories were also presented.

Questions were raised particularly about the worthwhileness of books as well as the reasoning, quality of research design, and documentation of material presented in them. The heuristic value of these findings for authors, reviewers, and editors was stressed.

## Houses Found, People Lost?

Kurt W. Back

*Slums, Projects, and People: Social Psychological Problems of Relocation in Puerto Rico*. Durham, N. C.: Duke University Press, 1962. Pp. 123. \$5.00.

Reviewed by MARY EVANS COLLINS

Kurt Back, the author, was educated at the Universities of Vienna and Geneva, at NYU, at UCLA, and finally at MIT where he received his PhD in group psychology in 1949. He is now Professor of Sociology in the Department of Sociology and Associate Professor of Psychiatry in the Department of Psychiatry at Duke University. The reviewer, Mary Evans Collins, is an Associate Research Scientist at NYU's Research Center for Human Relations. Her early training, of which she reports herself proud, happened at the University of Wisconsin with Brogdon, Grant, and Harlow, but somewhere along there her interests changed to social psychology



### STATISTICAL CONCEPTS: A Program For Self-Instruction

By CELESTE McCULLOUGH and LOCHE VAN ATTA, both of Oberlin College. Available in September.

Tested with hundreds of students at 12 colleges throughout the country. Intended for use as a supplement to instruction in those social science courses where it is important for students to gain an understanding of the elements of statistics. The emphasis is on the development of an understanding of statistical concepts with a minimum of statistical computation.

### VIGILANCE: A Symposium

By DONALD N. BUCKNER and JAMES J. McGRATH, both of Human Factors Research, Incorporated. McGraw-Hill Series in Psychology. 269 pages, \$8.95.

From an ONR-sponsored symposium on Human Performance on "Vigilance Tasks." The eight participants include the major theorists in the field and represent a cross-section of the various approaches to the study of human vigilance. Each chapter represents a formal paper delivered at the symposium and the discussion and critique of the paper by the eight participants. Three general types of papers are included: those concerned with presenting new research findings, those presenting theoretical views and those dealing with methodological issues.

### PSYCHOLOGY: Understanding Human Behavior, Second Edition

By AARON Q. SARTAIN, ALVIN J. NORTH, JACK P. STRANGE, and HAROLD M. CHAPMAN, all at Southern Methodist University. 498 pages, \$6.95.

Designed primarily for the one-semester introductory psychology course. The material is presented in an interesting basically non-technical manner, with two chief aims: to present psychology as a science and to discuss psychological problems of general interest to college students and the general reader. Both the principles of psychology and their applications to human behavior are covered. All chapters revised for a more compact treatment of the subject.

### DIGITAL COMPUTERS IN RESEARCH: An Introduction for Behavioral and Social Scientists

By BERT F. GREEN, Carnegie Institute of Technology. Lincoln Laboratory Publications. 352 pages, \$10.75.

This volume acquaints behavioral and social scientists with the use and operation of computers. The book combines an introduction to programming with an account of the applications of computers to research problems. A conscious attempt has been made to keep mathematics at an elementary level, and only basic algebra, trigonometry, and statistics are prerequisite on the part of the reader.

### PSYCHOEVALUATION: ADAPTATION — Distribution — Adjustment

By MILTON E. HAHN, University of California, Los Angeles. 275 pages, \$7.95.

Founded on the knowledge and skills which underlie counseling and psychotherapy; assessment, diagnosis, evaluation and communication. Discussion is limited to the normal individual within the age range of thirty to senility, and covers the personality growth and development of these individuals through their most active working years, and through the process of planning the last thirty or forty years of their lives. The most important feature is the emphasis on communication by several means in order that the individual better understand himself and his environment. For professional psychologists and graduate students in psychology.

### ELEMENTS OF EDUCATIONAL RESEARCH

By HILDRETH H. McASHAN, Brevard County Board of Public Instruction, Titusville, Florida. Off Press.

All the tools a beginning college student needs to initiate and carry out scientific research. Background in mathematics, research, or other special skills is not required. Each step of the process of scientific experimental investigation is lucidly organized within the sequential development of an actual research problem. A glossary of definitions concludes each chapter.

### SYSTEMS AND THEORIES IN PSYCHOLOGY

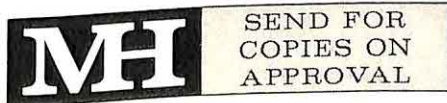
By MELVIN H. MARX, University of Missouri; and WILLIAM A. HILLIX, Navy Electronics Laboratory, San Diego. McGraw-Hill Series in Psychology. 512 pages, \$8.95.

Provides the advanced undergraduate and the beginning graduate student in psychology with basic information about systematic and theoretical problems in psychology. The approach is scientific rather than subjective or clinical. The authors provide the basic tenets of various classical and contemporary viewpoints in psychology and a philosophical framework.

### READINGS IN PSYCHOLOGY: Understanding Human Behavior

Edited by JAMES DYAL, Texas Christian University. 456 pages, \$3.95.

Designed to supplement the above, this collection of readings will help students increase their knowledge and interest in understanding human behavior from the psychological point of view. The articles selected illustrate how principles derived from scientific and clinical observations can be applied to the solution of everyday problems. Each chapter contains readings on the psychological aspects of the process under discussion; the important biological correlates of the psychological process, and socio-cultural determinants.



McGRAW-HILL BOOK COMPANY  
330 W. 42nd Street New York 36, New York



and after World War II she went to work on research in intergroup relations and with Morton Deutsch, produced *Interracial Housing* (1951). She has taught at Sarah Lawrence College, has done research of a publishable kind on student attitudes and of an unpublishable kind on her own fraternal twins. She received her PhD in 1961 from NYU and is now doing research on attitudes of white Americans toward American Negroes.

THIS little book is a disappointment. The promise of the title and the introduction is not fulfilled. Back and his team were steeped in the problems of slums, relocation and housing. They know the Puerto Rican landscape well. They had what appears to be easy access to records, to the people, and to the experiences of the relocation workers. The promise was one of insight into the decisions and problems of people changing their way of life. No insight comes.

The introduction talks about the hunches of the housing officials and preliminary observations of the researchers: that a move from the slums to a housing project represents a radical change of a way of living: an opportunity for a better life tied to change from owner occupancy to rental status and, with that, a commitment to new kinds of responsibilities symbolized by regular payment of high rent, acceptance of regulations against the keeping of domestic animals and against businesses run from home, potential loss of income from occasional work provided by wealthier neighbors, and the giving up of established associations with sympathetic neighbors and local creditors. There are, however, no data on the potential conflicts involved: how the people perceive the opportunities for self-betterment and what they would give up, and how they resolve these problems.

An underlying theme first stated in the introduction and restated as the conclusion of the study on the last page, is that relocation has two important social functions: to protect some of the depressed sections of the population and to provide some persons with a base from which they can seek a bet-

ter life for themselves. It seems to be the author's thought that these two functions apply to two different groups, but the population was not broken down to distinguish persons who were being protected and those who were using the projects as stepping stones to a better life. The approach to the hypothesis is that people who move into housing projects, as compared to the slum dwellers, have lower incomes and there are more broken families among them. They are also somewhat younger and have more children. Therefore, it is reasoned, they are economically more vulnerable, but they have more stake in the future. Since this appears (to the author) to be contradictory there then must be two different types of people who move into projects: one type that uses government support and the other that uses the improvement in housing as a path to social improvement. But there are no data to show that the younger ones see housing as a path to advancement and that the others are primarily concerned with government support.

There are, of course, lots of data in the book. They come from two extremes of the sociological-psychological continuum. At one end are the percentages, means and medians of demographic and related variables; at the other we have inferences from unvalidated projective measures purported to measure a personality construct, changeability. Somewhere in between, presumably, are people, with hopes, fears, worries, satisfactions and reasons for their actions, but the reader never meets them. This is all the sadder because the sample of over 400 cases was meaningfully stratified and the subsamples so well chosen.

It is only by very close scrutiny of the tables that the reader can get a tantalizing half-picture of the people who inhabit these slums and projects. For example, Mr. Project Tenant has a median age of 39, his median yearly income is \$776, he has a mean grade education of 3.42 years, his household has a median number of 5.4 persons and 3.0 children. Sixty-five percent of the households still have less than one bed per person or couple. Fifty-four percent of the heads of households have

regular employment, and 58% pay between \$4 and \$10 per month rent which represents a median of 20.5% of yearly income.

This is a sobering picture. It emphasizes the magnitude of the housing, or more appropriately, the living problems of truly underprivileged people. The rich opportunities provided by this study have not been exploited to illuminate these problems.

## A Newton Needed

D. V. Siva Sankar (Conference Editor)

*Some Biological Aspects of Schizophrenic Behavior.* (New York Academy of Sciences, vol. 96, Art. 1) New York: New York Academy of Sciences, 1962. Pp. 1-490.

Reviewed by PAUL BLACHLY

The author, D. V. Siva Sankar, was born in India and attended schools in that country, receiving his PhD in chemistry from the University of Madras in 1951. In this country he has done post graduate work at MIT and at Johns Hopkins, has taught at both institutions and also at Adelphi College. Presently he is senior research scientist in charge of the Biochemical Research Laboratories in the Children's Unit of Creedmoor State Hospital at New York. The reviewer, Paul H. Blachly, is a physician who holds a masters degree in neurophysiology and a diploma of the American Board of Psychiatry in Neurology. He now teaches on the staff of the University of Oregon Medical School where he pursues his research interest in the influence of drugs on behavior and operates a "drug clinic" for the outpatient management of the chronically psychotic. He reports himself now addicted both to the State of Oregon and to the academic life, the latter because it allows him to "browse, turn over mossy stones, and make Rube Goldberg devices."

THIS book records some aspects of the behavior of those intrepid explorers having the faith, and foolishness



to examine the contents of black boxes. The contributors are a group of persons lost in a cranial jungle, overwhelmed but entranced by the size, variety, and complexity of the flora. Even a small area of the jungle can remain cleared only by constantly beating off new and unexpected problems. As with any group of explorers, there are many approaches to dealing with the strange environment. Some systematically take one structure and investigate thoroughly its habitat, growth, function, and chemistry. Others attempt to investigate all the interrelationships in the jungle, but must do this at the expense of superficiality. Despite their difficulties, they are unified in the faith that there is a way out of the jungle, more effective methods of utilizing the resources found there, and especially that their labors will not have been wasted. None really feels he is close to the answer, but Henry Brill in closing the proceedings seems to echo the hopes of all for the coming of the savior: "It is my considered opinion that when the day comes that some theoretician will actually make such a synthetic leap and really draw together the findings from so many different directions into some common denominator, this may do for psychiatry what Newton's synthesis of data from mechanics, astronomy, mathematics, and physics did for physics and astronomy."

The reports in this symposium provide a representative sample of the scope of research activities on psychopharmacological aspects of behavior. The extensive bibliographies provide an excellent point of departure for anyone entering this field. Fortunately the contents are not limited by the concept "schizophrenia" in the title; most of the research is much more fundamental than that descriptive entity permits.

**T**HE SYMPOSIUM consists of three areas. Part one contains fourteen papers reporting studies of experimental psychoses and neuro-humours. These include reports on mescaline, time distortion, sleep deprivation, drug induced psychoses, and serotonin metabolism. D. W. Woolley who did so much originally to stimulate interest in the latter area reports recent findings.

The sixteen papers of part two vary markedly in quality and deal with the psychopharmacology of drugs. Subjects include drug metabolism (chlorpromazine, nicotinic acid, catecholamines, bulbo-capnine, morphine, imipramine, mono-amine oxidase inhibitors, etc.), electro-physiologic effects of drugs, and the effect of drugs on animal behavior. The paper by Leonard Cook and Roger T. Kelleher is especially useful as an example of the sophisticated techniques required for behavioral research of drug effects. Topics range from those that are of prime interest to psychologists, such as John Fuller's paper on the effect of drugs on psychological development, to those that will interest mainly pharmacologists, such as the discussion by Rex Pinson *et al* of the chemical design of drugs based on metabolic studies.

The majority of the sixteen papers in Part Three left this reviewer dissatisfied. They deal mostly with the search for specific biochemical and electro-physiologic abnormalities in schizophrenia, and as with most such previous studies are inconclusive. Although the obvious clinical utility of chemotherapy of psychoses makes this reviewer believe there must be some basic biochemical fault in at least certain types of schizophrenia, nothing in the papers mentioned here helped to elucidate this fault convincingly. The review by Leon Roisin of histopathological and histochemical studies of schizophrenia provides a useful bibliography of an area that to date has provided nothing of value for the schizophrenic patient. One longs for more debunking research such as that reported by Weil-Malherbe on adrenalin and non-adrenalin in serum of schizophrenics.

This underpublicized volume of definite reference value provides the excitement of sharing the jungle with the explorers, the frustration of knowing that one will never know enough to understand fully all of the disciplines represented, and the dissatisfaction of unanswered questions. If it goads a few persons into attempts to be Brill's latter-day Newton, it will have done enough.



*Second printing . . .*

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# Discrepancies and Exceptionality

Samuel A. Kirk

*Educating Exceptional Children.* Boston: Houghton Mifflin, 1962 Pp. vii + 413. \$6.00.

Reviewed by SAMUEL C. ASHCROFT

The author, Samuel Kirk, was already an eminent figure in the field of special education before he became the recent recipient of a \$75,000 award from the Kennedy Foundation for his contribution to the field of mental retardation. Since 1947 he has been Director of the Institute for Research on Exceptional Children at the University of Illinois. The reviewer, Samuel C. Ashcroft, obtained his doctorate at the University of Illinois and has been associated with the education of exceptional children since 1946. He was formerly Director of Educational Research at the American Printing House for the Blind and since 1957 has been at George Peabody College where he serves as coordinator of preparation for teachers of the visually handicapped. The Peabody program, with the cooperation of the Southern Regional Education Board and the American Foundation for the Blind, serves sixteen states of the Southern Region. He is senior author of the book *Programmed Instruction in Braille*, to be published in 1963.

KIRK's early professional background was that of psychologist, perhaps fitting the punsters' definition of the psychologist as one who "pulls habits out of rats." This background has undoubtedly been significant in Kirk's important contributions to special education. In the present text, Kirk has tried to pull a unifying concept (rabbit) out of the diverse groups (hat) of exceptional children who require special education. This is a neat but difficult trick. Kirk has brought it off imper-

fectly, but his performance in the process is so good that few will mind.

In his preface Kirk says, "In attempting to provide an integrated and unified book . . . I relied on concepts from child development. I have been especially influenced by the comprehensive records of growth patterns of children reported . . . by Dr. Willard Olsen . . . A concept of split growth, or discrepancies in growth, seemed particularly applicable to exceptional children. I have found this approach clarifies and unifies the concept of exceptionality, and I have tried to supply an integrating element which will give meaning to both the characteristics of the children and the resulting suitable modifications of educational practice. . . This application of a concept of discrepancies I consider to be the main contribution of the book."

*Educating Exceptional Children* has 415 pages and 14 chapters. The chapters, each with discussion questions and lists of selected readings are—Discrepancies in Growth and Development, The Gifted, The Retarded, The Hearing Handicapped, The Visually Handicapped, The Cerebral Palsied (and those with associated disorders) and Orthopedic and Special Health Problems, The Speech Handicapped, Behavior Deviations in Children, and finally, Administrative Services and the Preparation of Teachers. This represents comprehensive coverage. Each chapter is packed with information presented primarily in terms of, or supported by, research. Twenty-eight figures, ten tables, and indexes of names and subjects supplement the chapters in a neat and attractive textbook pack-

age tastefully produced by Houghton-Mifflin for a moderate price.

THE PRESENT REVIEWER used the book as a basic text in an introductory course for 35 students at the senior and graduate level. This provided an excellent proving ground; the students were asked to evaluate the text in terms of its usefulness to them. The following positive and negative comments were extracted from evaluation cards with the frequencies of mention noted.

## Positive comments

Summaries (9)  
Unification (7)  
Understandable vocabulary (7)  
Organization (6)  
Attractive, readable format (6)  
References and bibliography (6)  
Graphs, charts and tables (6)  
Adequate coverage (6)  
Recent research noted (5)  
(qualified in several cases)  
Indexing (3)  
Breadth of presentation (3)  
Introductory chapter (3)

## Negative comments

Integration of research (5)  
Lacks clarity in presenting statistics (5)  
Too brief in spots (4)  
Lacks illustrations (pictures) (3)  
Charts and graphs (2)  
Needs glossary (2)  
Lacks education emphasis (2)  
Repetitive (2)  
Gifted chapter (2)  
Speech chapter (2)  
Behavior deviations chapter (2)

In general, the students evaluated the text as very good. They used as bases of comparison a multiple author text and a book of readings assigned for supplementary reading. Seven specialists in different areas of exceptionality who participated with the reviewer in presenting the course were also asked for evaluations of the coverage of their areas. All the reports were generally positive.

It is natural to compare the "single-



author" text of this type with edited texts. Kirk's preface says, "The field of exceptional children is so extensive that it is difficult for one person . . . to write a comprehensive text; and I attempt this task with some temerity. The tendency has been to prepare such a text through multiple authorship, . . . Having prepared and edited *The Education of Exceptional Children* (the Forty-ninth Yearbook of the National Society for the Study of Education), I am also aware of the difficulties involved in working with a number of authors, who have different styles and different philosophies, and who are inclined to present overlapping content."

Kirk's coverage is surprisingly comprehensive for a single-author text. He has included the essential points in some detail and provided extensive research documentation. He has not avoided repetition as much as one might expect of the single-author approach.

AMONG the chief weaknesses of the text are the following. (1) The unifying concept of growth discrepancy is not comparably implemented in all the areas of exceptionality. Examples are the areas of speech and behavior deviations, but the theme also does not ring out with hoped for clarity in such areas as the gifted nor the visually handicapped. (2) Though copious research is included it has in many cases not been fully integrated and unified with the main theme with which it is associated. (3) Case illustrations have been used in some chapters but not in others.

Kirk's book is in a sense both a product and a shaper of its time. The body of knowledge about exceptional children and their special education is growing rapidly. The effects of investment from federal and private sources in research and training are being felt. It is a time of ferment, of reappraisal, a kind of adolescent period in the developmental history of a movement. The contrast between Kirk's earlier edited book and his present one reveal the developmental change. On every hand there is a current striving to find unifying concepts. The identification of

exceptional children in terms of legal, medical, economic or psychometric criteria alone and educational planning in terms of such factors has frequently been disappointing. Current trends reveal efforts to identify children and provide education in terms of characteristics with more unique significance for special education. The burgeoning body of research-based knowledge encourages the trend. The unifying growth discrepancy concept seems to be one of the current responses to the challenge of finding more suitable bases. *Educating Exceptional Children* is an excellent contribution to the field and the movement, but the rabbit is still elusive.

## How Separate Is Youth?

Ernest A. Smith

*American Youth Culture: Group Life in Teenage Society.* New York: Free Press of Glencoe, 1962. Pp. 264. \$5.00.

Reviewed by RONALD LIPPITT

*The author, Ernest Allyn Smith, is a sociologist with a PhD from New York University, with extensive teaching experience, and with a five year history of involvement with New York City's Juvenile Delinquency Evaluation Project. Ronald Lippitt, the reviewer, became very well known to social scientists when Lewin, Lippitt and White did their classic work together. Since those days Lippitt has increased his fame both singly and in collaboration with other sets of colleagues. Since 1946 he has been associated with the Research Center for Group Dynamics, and moved with it from MIT to the University of Michigan. He is now Professor of Psychology and Sociology at Michigan and Project Director in the Research Center. At present he and Stephen Withey are co-directing a program of studies on children, youth and family life. Among his books are Dynamics of Planned Change (with Watson and Westly, 1958, CP, Aug. 1958, 3, 228)*

and Autocracy and Democracy, with Ralph White, 1960 (CP, Oct. 1961, 6, 366).

IN RECENT years there has been a rapid growth of systematic research on delinquency and on the high school culture. This is probably a response to the research funds made available because of society's increasing concern and guilt about the poor job we are doing of educating and incorporating our young into the rapidly changing complexities of the economic, political, and social world. There have been few attempts, however, to map out and clarify the dimensions of the phenomena referred to loosely as "youth culture." This volume is an attempt to tackle that much needed task.

The present approach is one of surveying and integrating the sociological literature from 1900 to the time in 1957 when this manuscript was completed as a doctoral thesis. Some of the recent work with a more social psychological emphasis has been included, but there are serious omissions, and 75% of the 307 references are to descriptive sociological work or analytic essays before 1950.

The author points out clearly that our society is organized to exclude youth from significant role-learning opportunities in the political, economic, and sex-relations aspects of living. But then, in order to construct and defend his thesis of massive alienation and separation between adults and youth, he interprets the facts in terms of a process involving both hostile withdrawal by youth and collusive cohesion among them. This is at variance with some of the recent sociopsychological work, including a national sample interview study, which indicates that a large proportion of young people are disappointed with the help they get from adults but are not either rejecting or fighting them. Young people name a variety of significant adults, as well as peers, as persons whose ideas they listen to in making personal decisions.

In his focus on the informal peer structures of the clique, the gang, the crowd, and the date, the author chooses



to omit consideration of the school and the church because they are adult dominated socialization institutions. The work of Coleman, Reiss and others suggests to this reviewer that a major aspect of many youth subcultures is the type of orientation to and interaction with adult culture. Status within the peer culture is often determined by the type of liaison with, or successful combat with, the adult socialization agents. Peer relations in the school, or church, or family (among sibs) are just as truly types of youth subculture as are those settings more removed from contact with adult socialization agents. One of the underlying problems is that the author tends to interpret the notion of youth culture as implying a single loyalty conformity pressure situation. This seems to the reviewer to be at odds with the multiple loyalty picture from social psychological research on teenagers—multiple loyalties within the peer society as well as to significant adults.

In focusing on the serious discontinuities of socialization into adult sex roles and economic roles the author's generalizations tend to be post World War II rather than the 1960's, in his remarks such as "Today young people can easily find well-paid employment. . . ." (p. 45), and the statement that marriage represents a clearcut boundary of the youth culture. The youth employment situation has changed greatly since the focal period of this review, and many high school age marriages today remain in the context of parental economic dependency and peer culture associations.

The author interprets clique formation and loyalty as primarily a middle class phenomenon and anti-social gang formation as a lower class process. From current research we know that the gang is a rather special big city aspect of youth culture, with many other patterns of group formation and activity occurring among both middle and lower class youth in middle sized cities, small communities, and rural areas.

Major attention is given to the problems and processes of sex role socialization and sex relations within the teenage segment of society. During the

extended description of sexual behavior patterns it is hard to discern whether youth culture or changing mores of the total American society is the object of analysis.

The author has performed a real service in attempting to survey and integrate the very disparate fragments of data and qualitative description relevant to a conceptual analysis of the regularities in the behavior and de-

velopment of the youth segment of our population. The argument for a rather homogeneous and counter-adult youth culture seems weak. The clarification and pointing at important and poorly studied phenomena of group behavior of youth is impressive. The lack of use of complementary psychological data to deepen the analysis of socialization process within groups of youth is regrettable.

## Aging Gracefully or in Anger

Suzanne Reichard, Florine Livson and Paul G. Petersen. (Report on the study directed by the late Else Frenkel-Brunswik).

*Aging and Personality: A Study of Eighty-seven Older Men.* New York: Wiley, 1962. Pp. vii + 237. \$7.95.

Reviewed by BERNICE L. NEUGARTEN

*The study reported in this book was planned by the late Dr. Else Frenkel-Brunswik (of The Authoritarian Personality fame) while she and the authors were associated with the Institute of Industrial Relations at the University of California, Berkeley, and was directed by her until her death in 1958. At that point, Dr. Suzanne Reichard, who had worked on the study from the beginning, took responsibility for its completion, and before her own death in 1961, wrote several of the present chapters. Dr. Reichard was a clinical psychologist who, at the time of her death, was Associate Research Psychologist at the Institute of Human Development on the Berkeley campus of the University of California. The other two authors are also clinical psychologists. Mrs. Livson is presently Research Psychologist at the Institute of Industrial Relations as well as Clinical Associate at the California Medical Clinic for Psychotherapy in San Francisco. Paul Petersen has most recently been an instructor at San Francisco State College, then a USPHS fellow at Langley-Porter Neuropsychiatric Institute and is presently Clinical Psychologist with*

*Solano County Mental Health Service, in Fairfield California. The reviewer, Bernice L. Neugarten, is connected with the University of Chicago's Committee on Human Development. The connection has lasted from undergraduate days, through graduate training, to the present Associate Professorship. Her special interests are adulthood and aging. She directs a special graduate program in gerontology, has worked with Havighurst and Henry on the Kansas City Studies of Adult Life (soon to be reported in book form), has served as associate editor of the Journal of Gerontology and is now a co-editor of Vita Humana, an international journal of human development.*

THIS BOOK reports one of the few major studies yet undertaken of personality patterns in middle-aged and aging persons. As part of a larger program of research on the problem of aging in an industrial society, the study is focused on the role that personality plays in adjustment to aging.

The study is based on a sample of 87 men aged 55 to 84, all of them skilled and semiskilled workers, about half of



them retired, but all in relatively good health. The men were volunteers whose willingness to participate can be estimated by the fact that they came to the University of California campus at weekly intervals for a total of 7 to 12 hours of interviewing. Each man was also given a 2-hour battery of tests of mental functioning.

The significant part of the study is that relating to personality organization and is reported in Part III of the book. Utilizing a psychoanalytic framework, the investigators rated each *S* on some 115 personality variables, then on his over-all adjustment to aging. The latter rating was based upon *S*'s attitudes toward his current life situation, his attitudes toward growing old, and changes in his adjustment in recent years. A major criterion was *S*'s freedom from anxiety or depression.

The 40 cases rated high and the 30 cases rated low on adjustment to aging were then grouped; and using the ratings of personality, an inverse cluster analysis of the cases within each of the two groups was carried out. That is, each of the "high" adjusted was correlated with every other "high" on the 115 personality ratings; each "low" with every other "low." This analysis produced five clusters of persons, or five personality types.

Among the high or well adjusted are the "mature" men who, relatively free of neurotic conflict, are able to accept themselves realistically and to grow old without regret for the past; the "rocking-chair" men, who welcome freedom from responsibility and the opportunity provided by old age to indulge their passive needs; and the "armored" men who maintain a well-functioning system of defenses against anxiety by keeping active. Among the poorly adjusted are the "angry" men who are bitter over failures to achieve their life goals and who blame others for their disappointments; and the "self haters" who turn their resentment inward, blaming themselves for their misfortunes, and who are depressed rather than angry.

These five types have a certain "face" validity for the student of personality, who will be quick to recognize among this group of aging men the well-integrated, the passive, the compulsive, the

extropunitive, and the intropunitive. In highly oversimplified terms, these are types he has met before. It is important, however, even reassuring, that—to the extent any such study can be free of circularity—these types have merged from a painstaking statistical analysis, and not from an *a priori* typology.

THE STUDY has its numerous shortcomings. The problem of reliability of ratings, for instance, is treated in rather cavalier fashion. In the face of correlations between two raters that ranged from .27 to .75 for each of 10 *S*'s, all the cases continued to be rated nevertheless by only one judge (instead of using, say, pooled ratings of two judges). To take another example, nowhere is attention given to economy in the number of personality dimensions. One hundred fifteen is an unwieldy, if not a staggering number, especially when treated statistically as independent. Although an obverse cluster analysis does not demand it, it would have been enlightening to have a description of the intercorrelations between the variables as well as between the individuals, especially for the investigator who might be tempted to replicate the study.

Still more important, perhaps, is the fact that the five types account for only 47 of the 87 cases. (In addition to the 17 cases omitted at the outset because they were neither high nor low on adjustment, 23 others were not included in any cluster.) Thus the 5 types represent only a portion of the variation in personality patterns that may be presumed to exist among aging individuals; a fact that arises not alone from the limitation of the present sample.

THE WORTH of the study is not to be measured, however, by its shortcomings. On the contrary, the study makes two significant contributions. The first is to the field of gerontology, where the demonstration is important that there are a number of different patterns of successful and of unsuccessful aging; that the way a man grows old depends to a large degree on his personality; and that for some men, there are psychological gains in old age that compensate for

the losses. These particular findings may come as no surprise to psychologists who are accustomed to variation in personality patterns, and who should be prepared to find such variation among the aged just as among the young. Yet to gerontologists it is of some importance to have evidence that the passive (the "rocking-chair" men) as well as the active (the "armored") may be well adjusted in old age.

The second contribution is to the field of personality, where the whole area of age change in adult personality is still relatively barren of both theoretical formulations and empirical findings. The present study is a rarity in bringing to bear upon a relatively large sample of normal aging individuals (nonrepresentative though the sample may be) a set of dynamic constructs of personality, a focus upon patterns rather than isolated traits, and a treatment that is quantitative.

This book is likely to stimulate other investigations of age changes in the adult years, and to aid in formulating the developmental, the real view of personality that will encompass not only childhood and adolescence, but the whole life span.

## A Skimpy Uncovering

Berthold Stokvis

*Psychotherapie für den Praktischen Arzt.* Basel, Switzerland: S. Karger, 1961. Pp. ix + 142. \$3.50.

Reviewed by P. M. BENTLER

The author, Berthold Stokvis, is a physician, a psychiatrist and a psychoanalyst in the Netherlands and is presently Chief of the Psychosomatic Center at the Leiden State University. He has written widely in Dutch, German and English on hypnosis, suggestion, psychotherapy and psychosomatics. The reviewer, Peter M. Bentler, reports that he has not yet accumulated much biography. An outsider might well judge, however, that he will have a good and lively one. He is far enough along to



have obtained an MS from the University of Pittsburgh where he did a thesis on cognitive dissonance. Presently he is a graduate student in clinical psychology at Stanford University and hopes to receive his degree in 1964. He has already published a number of articles and is now involved in an attempt to make an empirical assessment of a technique he has devised for testing the statistical significance and stability of a test profile.

**S**TOKVIS' goal in writing this book was to present the general practitioner with an orientation to currently useful therapeutic methods. A general introduction is provided, and discussions of a wide variety of approaches are placed in short sections, each of which is followed by a selected bibliography. None of the discussions is long enough to provide a would-be practitioner with sufficient information on how to act during an interview, but just enough is mentioned to give the reader a feeling that, well, perhaps there is something to this type of therapy after all.

Individual psychotherapy is basically divided into "uncovering" and "not-uncovering" types. Uncovering means about the same thing to Stokvis as it does, for example, to Colby. It describes methods of therapy in which the therapist's goal is to help the patient gain conscious insight into his emotions, thoughts, and conflicts, including childhood origins of illness-producing influences. "Not-uncovering" therapy is Stokvis' term for therapeutic methods in which symptom removal is the primary aim, and in which the originating conditions leading to the patient's problems are not of primary concern. Stokvis points out that he would rather speak of "not-uncovering" than "covering" therapy because of the existence of methods which are clearly not uncovering yet are not covering either. Stokvis means by "covering" much the same thing as Colby does, though he places more stress on covering the symptom than on defense-strengthening. The use of the term "not-uncovering" makes intuitive sense to this reviewer, who thinks of learning therapies such as reciprocal inhibition in which the therapeutic goal is the unlearning of certain responses.

According to this theory, nothing is covered, and the techniques place no stress on uncovering conflicts or childhood experiences either (Stokvis makes only an incidental reference to learning theory). The following listing of current European therapies will serve at the same time to point out the author's classificatory scheme:

1. Individual "not-uncovering" techniques discussed by Stokvis include autosuggestion, suggestion, hypnosis, and practice techniques. Autosuggestion is illustrated by Coué's methods. Suggestion may be indirect, as in the case of placebos, or direct, as practiced by Kretschmer, Adler, or even Alexander and French in their short psychotherapy. Practice techniques are carried out in private, but under physician's supervision. These include mainly Schultz's autogenic training and Jacobson's progressive relaxation.

2. Methods which are partially uncovering include hypnocatharsis, hypnoanalysis, non-directive counseling, and Kretschmer's dual use of standard depth techniques with progressive, "active hypnosis," involving muscular relaxation, subjective changes, meditation, and direct suggestion.

3. Uncovering techniques include catharsis, narcoanalysis, psychoanalysis, Adlerian therapy, Jungian therapy, logotherapy, and Caruso's "personalistic psychoanalysis." The latter is an existential analysis based upon the spiraling upward, progressive, unfolding model of individuation and personalization.

4. Reeducative techniques follow Hugenholtz and Kronfeld. These appear to be a variety of direct techniques aimed at fostering self-sufficiency and responsibility on the part of the patient.

5. Group psychotherapeutic techniques include suggestive therapy, which may involve merely a psychiatrist's lecture to a group of patients; Klapman's general reeducative, ego-strengthening therapy; analytic group therapy; activity-oriented group therapy involving games or work; and self-realization therapy including painting, psychodrama, or socio-drama.

**W**HILE the Leiden practices are not discussed systematically, it appears that

in individual therapy and in group therapy each of the above techniques is used occasionally. Standard psychological tests are frequently administered to assess the intelligence and personality structure of the patient.

Stokvis' own general viewpoint is that psychotherapy is preferable to drug therapy, uncovering therapy is preferable to "not-uncovering" techniques if time is available, and group therapy is an important adjunct to individual therapy. All therapeutic methods, particularly the individual, "not-uncovering" techniques, are considered to work primarily through suggestions which are transmitted to the patient's preconscious and unconscious. If the suggestions are acceptable, their content may become conscious and lead to action.

Of particular interest to those studying therapeutic outcomes are the Leiden data on the effects of psychotherapy. Stokvis feels that therapeutic successes are, unfortunately, not satisfactorily great ("nicht erfreulich gross"). The Leiden practice is to evaluate outcome on a five category scale of social adaptation: very well, well, sufficient, questionable, and unsuccessful. Results of covering therapy of neuroses are distributed 11, 21, 17, 27, and 24 percent over the categories, and uncovering therapy yielded a 3, 20, 17, 37, and 23 percent distribution. Comparatively more successful was psychotherapy on 113 psychosomatic patients who were first treated somatically. Followup data indicate that 60 percent of these patients received ratings in the first two categories.

The book, while small, has a good deal of documentation, and is to be recommended for further references to Stokvis' own interesting publications.



*A scientifically oriented psychology is bound to proceed abstractly; that is, it removes itself just sufficiently far from its object not to lose sight of it altogether. That is why the findings of laboratory psychology are, for all practical purposes, often so remarkably unenlightening and devoid of interest.*

—JUNG





# To Plow or to Plow Under

John F. McGowan and Lyle D. Schmidt

*Counseling: Readings in Theory and Practice.* New York: Holt, Rinehart and Winston, 1962. Pp. v + 623. \$7.95.

Herman J. Peters, Bruce Shertzer, James B. Heck, Richard R. Stevic and Ralph E. Van Atta

*Counseling: Selected Readings.* Columbus, Ohio: Merrill, 1962. Pp. v + 463.

Reviewed by FREDERICK G. BROWN

John F. McGowan and Lyle D. Schmidt, authors of the first book, both took degrees at the University of Missouri, McGowan an EdD and Schmidt a PhD. McGowan has returned to the University of Missouri where he is now Professor of Education and Assistant Director of its Testing and Counseling Service. Schmidt is now at Ohio State University as Assistant Professor of Psychology in the College of Education. Herman J. Peters and Bruce Shertzer, the senior members of the team of editors of the second volume, are both Professors of Education. Peters at Ohio State University, Shertzer at Purdue University. The other three collaborators, Heck, Stevic and Van Atta, were at the time the volume was published all doctoral students at Ohio State University and each enacted at the same time at least one other significant role, in the area of counseling. Frederick Brown, the reviewer, started his addiction to psychology with Archer at Wisconsin, then went on to Minnesota for his PhD degree where he worked with Layton and was strongly influenced by Donald Paterson. After a period at the University of Missouri, teaching counseling and guidance as well as a variety of undergraduate courses, he moved to Iowa State University where he is now Assistant Professor of Psychology and Senior Counselor in the Student Counseling Service. His current fascination deals with the

application of psychological tests to the selection and understanding of students.

WHAT is the current status of counseling psychology? Anyone who has attended APA or APGA meetings, read counseling journals or the Division 17 reports realizes the concern over this issue. One neglected criterion measure, that certainly should lead to some index of status, is an evaluation of the literature in counseling. Using two recent books of readings as samples of that literature (admittedly a shaky procedure) would lead to widely divergent conclusions. Reading McGowan and Schmidt's book one would conclude that here is a fertile field—one that is full of ideas, is concerned with basic issues, is somewhat short on good research and theories, but active and alive. Picking up the Peters et al book, the reader would conclude that the field is fallow and lacking in vigor, and should either be fertilized or plowed under.

McGowan and Schmidt have collected 67 articles dealing with the counseling process *per se* and have organized them into a book designed to serve as a basic text for courses in counseling methods or in practicum courses. It is not a beginning text. It is for the student who already has had some exposure to the field. Actually, the editors underestimate their audience. Some of the readings could well

be used in an introductory counseling course and many deserve to be reread by all counselors.

The distinguishing feature of the book is the editors' incorporation of their own philosophy and experience through introductory and integrating remarks. Thus the reader is, in essence, presented with a number of short chapters each followed by appropriate readings. These original contributions are a good deal more useful than the usual introductory generalities and summaries. Apparent throughout is a guiding philosophy: (a) that the client-counselor relationship is the significant aspect of the counseling process and (b) that each counselor must develop techniques that are consistent with his own personality.

The point of view can truly be said to be that of counseling psychology (87% of the authors are members of APA). The articles are generally of high quality and represent the writings of the major contributors to the area. About half originally appeared in APA and APGA journals and another third were drawn from *The Journal of Counseling Psychology*.

Also notable is the inclusion of APA and APGA committee reports on definitions, ethics, and training standards. Adding to the usefulness of the book are the bibliographies and lists of selected readings at the end of the chapters. In spite of the dust jacket claims, however, not all of the articles were written within the last five years.

IN the second volume treated here, Peters and his co-editors present a compilation of 41 articles on school counseling designed to encourage counselors, counselor trainers, school psychologists and laymen "to do more conceptualizing, experimenting, and researching in the area of school counseling." The articles included are purported to discuss new ideas and methods and to foster "creative understanding."

Although promised much we are offered little. Only in the sections on general principles and processes of counseling will the reader be stimulated. The sections on ethics and on specific



problems of high school counselors may offer a few nuggets but basic psychological principles, though promised, are not covered. The empirical studies are primarily surveys. Most crucial, certain basic areas of the school counselor's function—testing, test interpretation, occupational and educational information, and local research—are not covered.

The range of adequacy and sophistication of articles is phenomenal. We are offered everything from articles written for the layman—certainly not the caliber of readings that should be used to train specialists—to articles on psychotherapy written by psychiatrists—again questionable for the school counselor. One gets the feeling that a school counselor trained with this book would resemble the G-man (Guidance man) in John Hersey's *The Child Buyer*.

Disappointing is the lack of any consistent viewpoint and the absence of any attempt to integrate the material. The editors restrict their own writing to short summaries of the articles. What results is a collection of miscellaneous articles, all at least vaguely related to school counseling, bound within a hard cover. If these are the best and most creative articles in the field, then school counseling is, indeed, in sad shape.

In evaluating a book of readings four characteristics are important. (1) What is the purpose and scope of the book? (2) Given this purpose, is the sampling of available articles representative, consistent, and adequate? (3) What is the quality of the articles selected? (4) Does the editor contribute something of himself through integration and interpretation or is he merely a compiler and summarizer?

Peters set a notable goal but did not implement it through adequate sampling or quality. Furthermore, he did not put himself into the book. McGowan and Schmidt, on the other hand, set a more limited goal and through judicious sampling, quality control, and comment have produced an excellent book. It should occupy a place equivalent to that held for the past ten years by Brayfield's *Readings in Modern Methods of Counseling*.

## Industrial Psychology as Science

Edwin A Fleishman

*Studies in Personnel and Industrial Psychology*. Homewood, Ill.: The Dorsey Press, 1961. Pp. xi + 633. \$7.00.

Reviewed by RICHARD H. HENNEMAN

*Edwin Fleishman, editor of the present volume, received his doctorate from Ohio State University and now, both after and in addition to various research relationships with various psychological operations in the military establishment, is at Yale University, where he is associated with the Departments of Psychology and of Industrial Administration. The reviewer, Richard Henneman, is in a compatible pattern of roles—teacher, researcher, scholar and gentleman—in the Psychology Department at the University of Virginia. He did his doctoral work with Woodworth at Columbia and ever since, most often with one kind of facilitation or another by the Air Force, has worked on one facet or another of the general problem of information processing.*

PROFESSOR FLEISHMAN has provided teachers of industrial psychology with an admirable book of readings. More important, he has provided students with a first-hand introduction to the analysis and investigation of the principal personnel problems confronting business management today. Most of the articles were published within the last ten years. The selections afford excellent illustrations of problems, concepts and research methods in areas ranging from personnel selection to equipment design and accident prevention. The conclusions offered the reader are derived from sound methods of investigation by competent investigators. Fleishman's book is not always easy reading. Slogans, pep talks on management techniques and cookbook rules for "handling people" are conspicuous by their absence. The selections emphasize

the industrial psychologist's role as researcher, rather than as administrator or consultant. A book like Professor Fleishman's should go far in helping to combat prevalent views as to the shallowness and superficiality of this area of concern. The serious instructor and the serious student alike will welcome the addition of this book of unusual substance to the literature of this area.

The table of contents (which is so often a source of disappointment in industrial psychology textbooks) evoked an immediate and favorable response from this reviewer. Fleishman has met with conspicuous success in attaining his stated objective of providing balanced coverage of the field of personnel and industrial psychology. His book is free from four common defects, one or more of which tend to characterize textbooks in this area of psychology. The articles are not limited to the "traditional" topics of selection, training, work efficiency and accidents, though a section is devoted to each of these. Nor is the social psychological side overemphasized. Morale, communication and leadership take up but three sections of the book, comprising only 230 pages of the total of 633 pages. The editor did not yield to the temptation to digress into topics of greater concern to personnel management than to psychology. The woman employee, labor-management relations, and the "industrial environment," for example, are not treated in the 66 articles. Finally, Professor Fleishman did not attempt to include a section on "consumer psychology," perhaps believing that the various problems of employee-management relations constitute quite a sufficient subject matter for



a one-semester course, without tacking on the hassle between distributors and consumers.

The nine sections of the book include two topics which have received inadequate treatment in most textbooks. These are performance appraisal and engineering psychology. Fleishman's book is one of the few to do justice to the importance of securing performance criteria. Two of the six selections in this section contain excellent analyses of the criterion problem. In his selections on engineering psychology, the editor again reveals his sophistication in industrial psychology by avoiding the overworked and misleading term, "human engineering," and separating this section from those dealing with work efficiency and accidents. While research on equipment design has obvious relations to these more traditional topics, it is certainly not to be identified with the broad area of industrial efficiency.

With respect to the selection of specific articles, no two psychologists would agree precisely on the articles to include in any book of readings. However, the majority of readers will agree that Fleishman has chosen the right research topics to be reported and the right articles to report them.

NO ONE has ever written (or edited) the perfect book. In the experience of the reviewer, who has used Fleishman's *Studies* for two years, the most serious questions relate to "readability" and student acceptance. These shortcomings are perhaps inevitable in a book of readings in which the articles have been selected for their significance and soundness rather than on the bases of their respective "Flesch counts." But there is the consequent absence of a unifying style of writing and organization of content which springs from multiple authorship, and the technical style of journal publications does not make for easy reading. Unfortunately, the editor has left too much of the burden of organizing and clarifying to the instructor in the course. If Professor Fleishman has erred in the preparation of the book, it has been in limiting too narrowly his own introductions to the successive sec-

tions. He writes clearly and emphatically. Had he chosen to lengthen his introductions, he would probably have greatly clarified the reading for the student and simplified the task of the lecturer. One might profoundly wish that there had been more Fleishman and fewer articles in the book! This is the greatest deficiency of the book, intended according to the Preface, as a primary text as well as a book of readings. In the experience of the reviewer, this book

will be more successful as a supplement to a more basic text.

Generally speaking, Professor Fleishman has been notably successful in achieving his stated aims of emphasizing the scientific aspects of industrial psychology and of presenting a balanced coverage of the field. The editor has made a very valuable contribution to those who prefer to present industrial psychology to the student as an *applied science* rather than as an *applied art*!

## Mental Health and the Cosmos

Kenneth Soddy (Ed.)

1. *Identity*. 2. *Mental Health and Value Systems*. Chicago: Quadrangle Books, 1962. Pp. vi + 271. \$6.75.

Reviewed by KJELL VON KROGH

*The editor here, Kenneth Soddy, is a British physician connected with the Department of Psychological Medicine at the University College Hospital in London and is also the Scientific Director of the World Federation of Mental Health. The reviewer, Kjell von Krogh, is a Visiting Professor in the Department of Psychology at the University of Colorado. A Norwegian, he was educated at the University of Oslo and took leave from a teaching position at that institution to visit at Boulder. He has served as a clinical psychologist in a Norwegian mental institution, as a member of the Board of the Norwegian Psychological Association and Chairman of that Association's Division of Clinical Psychology, as a member of the Consultative Board for Appointments of Positions in Clinical Psychology of the Norwegian Ministry of Health and as a member of the International Council of Group Psychotherapy.*

THE present book is the outcome of a series of meetings sponsored by the Scientific Committee of the World Federation for Mental Health, of which Kenneth Soddy, the editor of this book, is the Scientific Director. The objectives of the discussions were to attempt to clarify, and, if possible, to seek con-

sensus on frequently used mental health concepts, to review important new concepts, and finally to outline some leads into mental health research.

There are two major sections to the book. Part one, "Identity," was the outcome of the work of the Scientific Committee, composed then of H. C. Rümke (Nederland), Otto Klineberg (U.S.A.), Daniel Lagache (France), David M. Levy (U.S.A.), William Line (Canada), Margaret Mead (U.S.A.), E. F. O'Doherty (Ireland), Paul Sivadon (France) and Kenneth Soddy (England). Part two, "Mental Health and Value Systems," resulted from the deliberations of two small conferences, and from the Committee's own work during the years 1957, 1958 and 1959.

The editor must have faced two Sisyphean tasks. He had to conduct an intelligible chorus from such different sources as anthropologists, physicians, philosophers, psychiatrists, psychologists and theologians, and also to integrate the differing idioms of this multi-professional team. The reader, as a consequence, is now and again faced with the task of finding his way in a jungle of concepts and different styles of thinking. This is understandable when one considers that the discussion, growing out of the concepts of "Identity" and "Value Systems," leaves the reader



somewhat breathless after having visited sixteen major conceptual areas, including some hundred sub-concepts.

The first part, "Identity," and the last chapter dealing with mental health research, are by far the best organized and most coherent parts of the discussion. The paragraphs on "Identity Development" and "Empathy" are particularly astute, as is the recognition of a developing "somatic sensitivity" as "one of the roots of empathy" (p. 7). The novel concept of "Roots" appears to be a fruitful term for "the study of the conditions necessary for an individual to grow up, or settle down, in his identity" (p. 18). In a world with rapidly increasing social and individual mobility there is naturally a growing concern as to why "some families will create a constant home environment wherever they are living," while in others, "the sense of home may be quite disturbed by constant moving." The position of family life, as the producer of identity and value, is examined against the vicissitudes of a world moving towards vital cultural changes. A distinction is made between the "extended-family," with its emphasis on formal organization, and the "nuclear family" with its more tightly knit parent-child relationships. Adaptability is examined in this connection, with the suggestion that children growing up in the "extended-family" will tend towards maintenance of the traditional setting, and consequently will be vulnerable to stress when exposed to change. The "nuclear-family" child, however, "will tend to find satisfaction in the actual process of changing and, particularly, in the evolution of interpersonal relationships."

The Committee sees family type as related to the crucial notion of "Identity Strength" which they describe as a dynamic tendency which serves integration. This "general-manager" of the mind, with its unmistakable features of the Freudian ego, is profitably brought to bear on such phenomena as "Temporary Breaks in Identity," "Promotion of Identity," "Multiple Identities," "Basic Plot" and "Style," "Change of Identity," and finally, "Identity Breakdown."

The second part of the book, dealing with mental health and value systems, is said to have grown out of an in-

creasing realization that mental health is becoming a value in itself similar to the modern concept of bodily health. Embracing 190 of the 240 pages of discussion, this part of the book clearly reveals some of the dangers faced by this type of cooperative enterprise. One constant risk, of course, is patchiness in content and style. With so many contributors, it would be close to a miracle had the present work not at least skirted the edges of this fate. The many and partly conflicting viewpoints expressed also awaken the reader's curiosity as to the unrevealed identity of the various contributors.

Objections of this nature, however, are easily outnumbered by the merits of this part of the book. In many respects the discussion represents a unique contribution to problems of mental health and "the ways in which the concepts of mental health that are acceptable to people brought up within the various great religious and ideological systems agree or differ" (p. 61).

From the wealth of material presented in part two, we can extract only a restricted sample for consideration. One main theme evolves from the definition of mental health as a "great deal more than the mere absence of mental illness" (p. 73). This immediately raises a multitude of questions concerning *what more?* Is mental health synonymous with happiness? Is it the same as moral contentment? Is mental health compatible with error and wrong doing? What is the relation between mental health and social adaptation? Can a man be very wicked but still mentally healthy? A review of religious writings quoting Buddhist, Hindu, Islamic, Confucian, Jewish and Christian sources, demonstrates a consistent tendency to equate "goodness" with purity of the mind as well as "absence of delusion." When considering the virtues leading to "goodness," however, the discussants suggest the likelihood that perfect "goodness" can be achieved "at the cost of such strain that (he) might with justification be regarded as mentally unhealthy" (p. 82).

Other topics profitably discussed include the relation between belief in God and mental health, criteria for mental health assessment, and, relatedly, the risk of promulgating in other societies

the mental health and psychiatric concepts developed in Western Europe.

The discussants have had a hard time in facing the problem of developing universally valid criteria for mental health. In the last section of the book several aspects of such criteria are considered and recommended for further study.

Of special interest is the proposed term of "positivism," which in principle says that ideally we should be brought up positively to be what we are, and not something we are not, e.g., a boy should be taught to be a boy, and not something which is not a girl. The assumption of an inverse relationship between cultural levels of aspiration and acceptance and non-interference with deviant behavior, is also a provocative idea deserving systematic inquiry. However, we miss here an attempt to integrate and summarize the multitude of interesting viewpoints. Also, it seems that the discussants express an unduly pessimistic view concerning personality assessment as compared to what is assumed to be the more advanced state of biological measurement. At this point, a comparative discussion of the nature of mental health concepts would have been of considerable interest. The discussion could also have profited from the inclusion of some of the recent developments and refinements in the field of personality measurement, e.g., the concept of "construct validity," the developing actuarial approach to personality diagnostics, and the growing realization of the reciprocity between psychometric and impressionistic procedures.

In conclusion, objections of the kind raised above could as well be regarded as a tribute to the contributors for having brought the discussion to a head so that technical considerations of a more specific nature have to take over. The work also profits from the awareness on part of the discussants that present day mental health work should have a world-wide address. In an era when radical cultural changes take place within a time-span briefer than that of a single human life, this contribution to the professional persuasion towards sanity should be welcomed as one more aid to man's coping with the precarious impact from a hazy future.



# Much Thinking, Little Doing on Methods

W. M. O'Neil

*An Introduction to Method in Psychology. 2nd Revised Edition.* Australia: Melbourne University Press; New York: Cambridge University Press, 1962. Pp. x + 178. \$4.50.

Reviewed by ALVIN G. GOLDSTEIN

W. M. O'Neil, author of the present book, did all of his undergraduate and graduate work at the University of Sydney and, since 1945, has held the McCaughey Chair of Psychology in his alma mater. He has paid a number of visits to this country, the most recent in 1960 as Visiting Tallman Professor of the Philosophy of Science at Bowdoin College. The reviewer, Alvin G. Goldstein, did his graduate work at Clark University with Heinz Werner and Seymour Wapner and then moved for two years to the Army Medical Research Laboratory at Ft. Knox, Kentucky. Since 1956 he has been on the psychology staff at the University of Missouri holding assorted appointments in research and teaching until recently when he became a regular Associate Professor. His main research interests concern the development of perception in children but he teaches with enthusiasm an undergraduate course in experimental methods.

IN ORDER for a book successfully to present its material to a reader, it should have been written with a specific goal in mind. In the best texts this goal will become evident to the reader in a short while, but it is most helpful if it is stated in the Preface. The author of this book, in both the Preface and the body, is somewhat vague about his purpose. Such vagueness is one of several problems a reviewer faces with this little book. Is it a textbook on method in psy-

chology or is it a general discussion of some of the psychological methods? If it clearly belonged in either category, the job of reviewing it would be relatively unambiguous. But what does one do if this distinction is not clear? The present reviewer decided to call it a textbook. Since it was written for Australian students, this poses another problem. Should it be evaluated with regard to the American undergraduates who may use it? It seems obvious that there was only one answer to this question—an affirmative one—but then there is the danger that it will be an unfair evaluation.

A textbook—especially an introductory text on method—is often a sophisticated version of a how-to-do-it book. O'Neil's book differs from several recently published introductory texts on methods in that it is not a "how-to . . ." book. Compared to American texts, it is better written, more concerned with theoretical issues, deals more with general scientific method rather than specifically psychological methods, treats at greater length such matters as formal logic and general systems. In this reviewer's opinion, a student who completes the book would still need a course of instruction in the details of experimentation.

It is not a "how-to . . ." book mainly because there is no description of, for example, the psychophysical methods, no instruction on how to handle the subject in psychological experiments, etc. In some chapters where O'Neil covers

areas common to other texts, such as hypothesis, regularities, and case study and survey methods, he is original and clear; in other sections he is not only unclear but erects what may be both unnecessary and almost insurmountable barriers for most (American) undergraduates.

For example, only the most dedicated beginner in psychology—and O'Neil is writing for this student—could make his way through the first chapter in which a detailed mathematical description of Spearman's *g*- and *s*-factor theory of intelligence is given as an example of scientific investigation in psychology. For this same purpose Holway and Boring's classic (but erroneous) research on the moon illusion is also (uncritically) presented, but here, because of the relative simplicity of the moon illusion research, the author's goal is much more nearly attained. The Spearman example is rather difficult to comprehend, especially without some knowledge of statistics (and this knowledge on the part of the student apparently is *not* assumed, because on p. 71 the formula for the arithmetic mean is given). Whereas both the Spearman and Holway-Boring examples play an important didactic role—they are used as models—in clarifying new concepts in subsequent chapters, there is a good chance that the student will be in trouble before he gets many pages into the volume. With a less cluttered illustrative example of scientific investigation there would be a greater probability that the reader would have a solid understanding of the elements in the example and therefore be in a much better position to grasp more complex concepts.

O'NEIL attempts to introduce the student to formal logic. Thus all of chapter 8 (15 pages), "Propositions and Inference," and sections of other chapters are devoted to forms of propositions, relations between propositions, distributed and undistributed terms, etc. The student is told that in order to draw conclusions or make inferences, the relation of implication must be understood and to do this a ". . . brief excursion into logic" is needed. This reviewer does not want to argue with



O'Neil about the advisability or the need for using formal logic in an introductory course, but he does wonder about the value of an abbreviated version of logic. If making inferences from data demands a formal knowledge of logic, then logic should get more attention than this book gives to it. However, a better solution for a textbook may be to allow the philosopher of science to do this teaching while the psychologist teaches methods of obtaining data in the infinite variety of experimental, field and clinical situations.

There are sections of this little book which could be used profitably as extra reading, but it is unlikely that it will be useful by itself as a text for an American methods course in psychology. Undeniably, straight thinking about data is a very important aspect of scientific research but correct doing is equally necessary. These aspects are not quite balanced in this text.

Although this is a second edition, the only major revision was rearrangement of the chapters, a change that had relatively little effect on the continuity of the book.



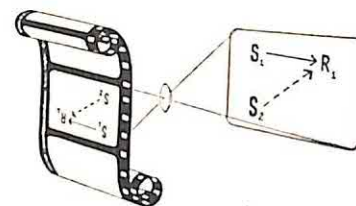
Intellectual "work" is misnamed; it is a pleasure, a dissipation, and is its own highest reward. The poorest paid architect, engineer, general author, sculptor, painter, lecturer, advocate, legislator, actor, preacher, singer is constructively in heaven when he is at work; and as for the musician with fiddle-bow in his hand who sits in the midst of a great orchestra with the ebbing and flowing tides of divine sound washing over him—why, certainly, he is at work, if you wish to call it that, but lord, it's a sarcasm just the same. The law of work does seem utterly unfair—but there it is, and nothing can change it: the higher the pay in enjoyment the worker gets out of it, the higher shall be his pay in cash, also. And it's also the very law of those transparent swindles, transmissible nobility and kingship.

—MARK TWAIN



# INSTRUCTIONAL MEDIA

Edited by A. A. Lumsdaine



## What ETV's Been Doing (Here) Til Lately

Henry R. Cassirer

*Television Teaching Today.* Paris: UNESCO, 1960. Pp. 267. \$3.00

Reviewed by NATHAN MACCOBY

Cassirer, the author, is a senior member of the Secretariat of UNESCO headquarters in Paris, where he is head of broadcasting in the Division of Mass Communication. Between duties in helping to plan and coordinate conferences of expert advisors, such as the recent one on the use of educational media for reduction of illiteracy (Paris, March 1962), and implementing resulting recommendations, he finds time for study and analysis of educational media, such as the one reported in this book. The reviewer, Nathan Maccoby, is Professor of Communication at Stanford University, where he has been located since he left the psychology department chairmanship at Boston University in 1959. A Michigan (1950) PhD, he is one of the numerous psychologists who have achieved distinction from the springboard of a Reed College BA and/or a University of Washington MA (he took both). Later he was one of the G. I. elite (along with Irving Janis, Francis Keppel), and others who helped produce *The American Soldier* and related World War II studies. Maccoby has published in fields ranging from attitude-change theory to employee productivity and morale, and from opinion-survey techniques to theoretical-experimental studies of instructional demonstration films. (Several of the latter, co-authored with Fred D.

Sheffield, appear in the recent NRC-sponsored volume, *Student Response in Programmed Instruction*.)

HENRY Cassirer, on the basis of a six month visit to the United States and visits to and reports from six other countries, has compiled a most useful and instructive manual reporting the state of the art in television teaching. Unfortunately, the third word of the title is no longer applicable. The book was published in 1960 on the basis of work completed in 1959, and the field of educational and instructional television has been growing at a fairly rapid pace since that time. Potential readers also are hereby warned that this volume is not a research report, except perhaps in an historical sense, and contains little theory or theorizing. What it does do is to report in a clear, accurate and concise manner most of the major ventures in instructional (usually closed-circuit transmission in the school or classroom) and educational (over regular TV stations) television, usually on non-profit community or university owned and operated stations. However, some important educational programs such as *Continental Classroom* and *Sunrise Semester* have been carried by commercial stations, usually at ungodly hours but, nevertheless, carried.



Since the United States has been, by a considerable margin, the most extensive user of such broadcasting, it is appropriate that more than three quarters of the book is devoted to the U.S. Canada, France, Italy, Japan, the USSR and the United Kingdom comprise the six other countries studied. In addition, there is some discussion on the potential future uses of television for teaching purposes and of some of the major problems encountered to date.

In his report on the United States, Cassirer organizes his presentation by first reporting on the use of television in the schools. Some of the early programming ventures such as Hagerstown, Philadelphia and Demarva (Delaware, Maryland and Virginia) are discussed in some detail. Since these ventures there have, of course, been a great many others. In many communities such as Pittsburgh, San Francisco, Denver and Boston, the educational television channels broadcast regular in-school classroom programs in which a great many schools participate and which constitute a part of their regular curriculum.

In summing up his account of elementary schools, Cassirer expresses the opinion that television has more potential use in elementary education than in secondary. He offers several reasons for this view—e.g., that teachers of elementary grades are less often subject matter specialists and therefore benefit more from subject matter expertly presented on TV than is the case for the high school teacher. Related to this is the scheduling problem. The elementary teacher usually has her class all day and can be flexible in programming content; high school students typically have different teachers for different subjects, and scheduling is perforce more rigid.

THE author mentions, but in this reviewer's experience does not give sufficient attention to, the problem of teacher resistance to the introduction of TV into the classroom. Televised instruction planned by national or world experts can all too readily be used as criteria against which the live



H. CASSIRER (RIGHT) WITH L. R. FERNIG, UNESCO SECRETARIAT.

teacher must compete. In the present state of this art (even since 1959) as Cassirer rightly points out, TV is used less often to replace teachers than to assist them. He sums up this part of his discussion by saying "Television is not merely a substitute for traditional ways of teaching. It is a medium with its own psychological and emotional appeal able to transcend barriers of time and place of disciplines and personalities."

Now comes higher education's turn. Outside of medical and dental schools, Cassirer says that the United States is the only country using television for instructional programs in colleges and universities. He points out that instructional television can be employed either to substitute for teachers when they are in short supply or to enrich instruction, to present content that cannot be presented in the ordinary classroom. For example, Cassirer reports and illustrates with photographs what dental students can see in a live demonstration aided, versus unaided, by closed circuit television.

With respect to American colleges and universities, Cassirer cites data showing projected increases in enrollment and decreases in the qualifications of instructors. Television, he and many others feel, can help. Certainly programs in televised instruction such as those carried out at Pennsylvania State University and at many other American institutions of higher learning support the view that televised instruction can be used instead of live instruction for considerable segments of the cur-

riculum. Even such technical problems as the lack of possibilities for interaction between student and instructor have been partially overcome, and there have been demonstrations in which the instructor was provided with better 'knowledge of results' of his own instruction during the time of lecturing than is normally possible in the ordinary lecture hall.

Closed-circuit TV has also been used to make it possible for students of one university to tune in on lectures and discussions going on in another one, as well as for the training of professional practitioners such as medical doctors. This reviewer feels that Cassirer is perhaps a bit overoptimistic about the costs of such ventures, but nevertheless sound in his appraisal of TV's potential usefulness here.

IF the discussion of television teaching in the United States is reasonably comprehensive—except for the serious omission of relevant educational and psychological research—the summary reports of the other six countries are tantalizingly sparse. This is especially the case for the account of the use of television in the USSR. The author has unfortunately, in this instance, evidently had to base his account on a report submitted by the USSR to UNESCO in answer to a questionnaire. The result is a total of four pages, and even these contain very little information.

There is one concern that this reviewer feels about the future of instructional television that Cassirer does not adequately point out. Undoubtedly this concern is at most a speculative one, but it is this: if we succeed in extending really widely the use of instruction through audio-visual mechanisms of which TV is an example, may not the day come when attitudes and points of view as well as neutral information and skills are centrally determined and uniformly taught? Certainly even for advanced countries like the United States, such a threat, could develop, even though it does not yet constitute a clear and present danger.





# ON THE OTHER HAND



*In this Department CP invites discussion of reviews and of books reviewed. Here is the place for that kind of intellectual dissent that promotes progress in understanding. Let your criticism be ad verbum, not ad hominem. Seldom does a criticism merit more than half the space of the text criticized—never more than equal space and then only when the letter is interesting and well written. CP edits letters when it thinks they should be. Single-spaced letters will be returned for double-spacing.*

## PRESCRIPTION, STUFFINESS FUSSINESS AND FACT

In "CP Speaks" in the March 1963 issue of *CP*, you say that "writers and editors should concern themselves with questions of proper English usage" and otherwise indicate, through references to such things as "schoolmarm rules" and "linguistic proprieties," a belief that there exists an explicit code which the writer should follow.

I share your concern with the problem of communicating, and I agree that Living Standard English (LSE) can be communicated better in some syntax-discourse forms than in others.

What I object to specifically in your editorial is the rules—all of the rules, such as they are—which you cite as the kind which will help. These rules come from the prescriptive grammars of the past, which provide poor theoretical fits at best for LSE.

I daresay you have not established empirically or seen someone else's evidence that a *CP* reader will get the message more clearly or will prefer, in his capacity as a professional recipient, messages which are packaged according to your rules to those packaged in forms

which violate your rules. What evidence exists that:

(1) Contemporary readers use *data* in the plural more often than in the singular?

(2) Split infinitives, with some exceptions, will not communicate at least as effectively as unsplit ones?

(3) An alternative construction, such as the genitive "test of achievement in the psychology of learning in humans" will communicate better than an adjectival noun construction, such as "human-learning psychology achievement-test"? In this connection, 19 years have passed since Bentley's well-known "A noun modifies a noun modifies a noun modifies a noun." In that editorial note, war was declared upon adjectival noun constructions. While the respected journal in which it appeared has ever since rather rigidly required that long strings of noun adjuncts be avoided if humanly possible and caused the writer to hyphenate furiously those instances which are inevitable, I never have seen an empirical test which justifies all of this concern. It is not self-evident to me that even Bentley's prize example "Education psychology habit practice instinct and fatigue studies" will not get the job done as efficiently as some alternative construction.

(4) The reader expects adjectives to modify nouns? As Francis notes in his *The Structure of American English*, just about anything can modify a noun in Standard English.

(5) Two usages of a word-form and one of a synonym in a sentence which employs a concept three times communicates better than three usages of the same word-form? You didn't, in fairness, contend this, but rather that the "reader will wonder, perhaps with empathetic embarrassment, about the size of your vocabulary." Perhaps we deliberately encourage a good deal of confusion in the name of style when we insist on using several names for every concept in a paper. One may wonder whether we are not downgrading meaning in the service of

style. I surely would hate to wade through an introductory physics book wherein the editor had persuaded the writer not to say *force, mass, acceleration, time, energy, weight, distance, volume, resistance, light* and *impedance* over and over again. Perhaps it is difficult to employ the same concept three times in one sentence. Where the subject matter dictates such a course, however, I wonder whether an empirical test might not show word-form repetition to be the more efficient alternative, however much the exercise of this option might degrade the writing as prize literature.

I really am in sympathy with the editor's problem as you convey it. I myself often write so poorly that almost any change would be an improvement. What I object to in your editorial, coming as it does from one trained in the sciences rather than the humanities, is an apparent nonallegiance to the notion that style guides, to be useful, must be explicit and have an empirical basis. As an example of the wrong kind, there is little basis, if modern grammarians are to be believed, for saying without qualification that thou shalt not dangle participles. Whether one shall or shan't depends on whether semantic ambiguity arises, a matter which cannot be fully settled except by empirical study stemming from an adequate taxonomy of participial constructions.

I think both writers and editors need to have explicit rules to guide them, but they should be rules from a grammar which better fits LSE than the traditional prescriptive grammars, as verified by tests which establish that readers get the meat better when one syntax-discourse form of packaging is used than another.

I for one would be surprised if the conventions which you advocate often matter very much one way or the other. But I would be happy to join you in withholding judgment until you or I or someone else actually goes to the well to find out.

JOSEPH F. FOLLETTIE  
El Paso, Texas

## MARKS MISSES MARK?

I feel called upon to comment on Marks' review of *Management and the Computer of the Future*, by Martin Greenberger (Editor) *Contemporary Psychology*, April 1963, 8, 144. As a person who reviewed this book for the journal of *Educational and Psychological Measure-*



ment, I feel that Marks' review was most inadequate. The review gave no indication of the type of book this was—namely a series of eight lectures given in honor of the M.I.T. centennial year. No statement was made as to the caliber of the authors and discussants who are, in fact, well known leaders in their fields. Even the content of the book was slurred over, and the reader of the review has no adequate information on the range of material covered.

In my opinion, this is a worthwhile book and one which has implications to psychology and especially to those psychologists using computers in their research. It deserves a more substantive review.

HAROLD BORKO  
System Development Corporation

#### AUTHOR'S IMPLICATION OR REVIEWER'S INFERENCE

Mitchell Glickstein's review (*CP*, Feb. 1963, 8, 46 ff.) of *Roots of Behavior* states "The data in the first section are from studies of *presumed* genetic influences on behavior . . . Although he (Hirsch) is careful to distinguish individual differences from genetic variability, much of his argument implies that they are identical. Such an implication seems both hazardous and unjustified" (italics and reference for pronoun added).

That implication would certainly be most unjustified *were it in my text*. It would indicate that in practice I fail to appreciate one of the elementary concepts of genetics, the distinction between the phenotype and the genotype. For Glickstein to infer the presence of that implication is worse than hazardous: it represents either careless reading or captious criticism in view of such explicit statements in my text as

"Whether or not . . . genotypic differences are involved in any particular behavior remains an empirical question to be investigated separately for every behavior" (1962, p. 5).

"The argument in this chapter is not intended to imply that the variability observed in behavioral experiments is to be attributed solely to the underlying genotypic differences among all individuals. The problem of accounting for the observed variations is one that will require painstaking analysis, a kind of analysis from which the

counterfactual assumptions just reviewed have been diverting attention" (1962, pp. 11-12).

I definitely tried to save the reader, who (1) was careless, (2) had an anti-heredity bias, or (3) had a pro-heredity bias, from misconstruing the argument.

Some eminently competent scientists reference that chapter and neither infer the same implication nor consider the "genetic influences on behavior" to be merely "presumed" (e.g., McClearn, 1962; Médioni, 1963). Two others (Dobzhansky & Spassky, 1962), in fact, have recently reported corroboration and extension in another species of the work on geotaxis whose "value to behavioral science" Glickstein "might question." I cannot help wondering about the relation between the concept of "behavioral science" to which Glickstein subscribes, the glib derogation of ideas and research that fall beyond one's limited purview, and the reasons that prompted Donald Griffin's observation: "Biologists are becoming aware that behavior is much too important to be left to the psychologists" (*CP*, 1959, 4, 225).

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JERRY HIRSCH  
University of California, Berkeley



*A dream is a strange thing. Pictures appear with terrifying clarity, the minutest details engraved like pieces of jewelry, and yet, we leap unawares through huge abysses of time and space. Dreams seem to be controlled by wish rather than reason, the heart rather than the head—and yet, what clever, tricky convolutions my reason sometimes makes while I'm asleep! Things quite beyond comprehension happen to reason in dreams!*

*The Dream of a Ridiculous Man*  
—DOSTOEVSKY (1877)



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One of the troubles with beginning writers is that they are almost certain to blaze out of control. Often it is the teacher's job to squirt a little water here and there. For the object is to heat and to illuminate the house, not to burn it down. And the only way to achieve that object is to keep the fire where it belongs—in the fireplace. The act of distinguishing between the logs of the fireplace and the walls of the house is called criticism, and I am tempted to claim that any fires that have been put out as a result of the act of criticism should never have been started.

—JOHN CIARDI





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*Contemporary Psychology: A Journal of Reviews* is published monthly. The subscription per year is \$10.00, foreign \$10.50, single number \$1.00.

Published by the American Psychological Association at 2210 W. Dallas, Houston, Texas and 1333 Sixteenth St., N.W., Washington 6, D. C.

Second-class postage paid at Houston, Texas and at additional mailing offices. Accepted for mailing at special postage rate stated in section 132.122, Postal Manual.

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# Contemporary Psychology

A JOURNAL OF REVIEWS

VOLUME VIII

August, 1963

NUMBER 8

## Entrepreneurial Behavior

David C. McClelland

*The Achieving Society*. Princeton, N. J.: D. Van Nostrand, 1961. Pp. vii + 512. \$7.95.

David McClelland, the author, did his graduate work at Yale, then taught at Connecticut College for Women, at Bryn Mawr College and at Wesleyan University before moving to the Department of Social Relations at Harvard, where he is now Professor and Chairman of the Staff of the Center for Research in Personality. The present book grew out of a program of research, begun in the early fifties, dealing with the achievement motive. The first reviewer, James N. Morgan, is the son of the late and illustrious psychologist John J. B. Morgan, but this Morgan is an economist. He maintains intellectual contact, however, with psychologists and other behavioral scientists. In 1955-56 he was a fellow at the Center for Advanced Study in the Behavioral Sciences. Presently he is at the Survey Research Center, University of Michigan, a Program Director and Professor of Economics. His list of publications includes the 1955 book *Consumer Economics*. The second reviewer, Bernard Mausner, is a psychologist who here contributes his sixth review to CP. A Columbia PhD, he taught at New York University and at the University of Massachusetts, and did research in public health at the University of Pittsburgh before moving to Beaver College, where he now chairs the department,

teaches undergraduates and pursues his research interests in social interaction and in attitude change. In connection with the latter, he is now studying attitudes towards smoking and lung cancer and in that study is using programmed teaching devices to generate messages. He is a co-author of *Job Attitudes: Review of Research and Opinion* (CP, July 1958, 3, 192) and of *The Motivation to Work* (CP, Jan 1961, 5, 9).

Reviewed by JAMES N. MORGAN

THIS is an important book which should be read by economists as well as psychologists. It supports a growing conviction that inter-disciplinary cooperation in the social sciences is most fruitful when the focus is on specific empirical problems. Instead of casual borrowing of concepts or terminology, one gets tests of whether measures derived from one theoretical discipline actually help explain the behavior of interest to another.

Professor McClelland has focused on the problem of economic development, and on the dynamics of change in the working and striving behavior of whole groups of people. Most economic problems and particularly those connected

with economic development deal with the changes in aggregates which result from the behavior of whole groups of people, so he has tackled an economic problem. The principle of Occam's razor has been adhered to firmly by economists. Things which affect some people one way and others in another, or which do not change over time, will not help explain changes in aggregate behavior, and are not interesting to economists. Only variables which can be measured, and shown to have verifiable value in explaining or predicting behavior of groups of people will be admitted into economics.

BUILDING upon his clinical background but going far beyond it, Professor McClelland has gone to great lengths to look for samples, to include a wide variety of situations, to quantify the variables in an unbiased way, and to test hypotheses about the relation of individual human motivation to economic development. His work represents part of a tendency for research in psychology to move to representative samples of individuals or groups, and to quantitative measures subject to statistical tests. Another main stream of interest to economists uses personal interviews with representative samples of individuals to explain short run changes in economic behavior, using both psychological and other variables. (See George Katona, *The Powerful Consumer*, New York: The McGraw-Hill Book Company, 1960; CP, June 1961, 5, 216f., for a summary of progress to date.)

Professor McClelland has also been parsimonious in introducing theory and variables. It is easy to define new variables, and to spell out new theories, but difficult to quantify the former and test the latter. On the other hand, the very restriction of theory and variables

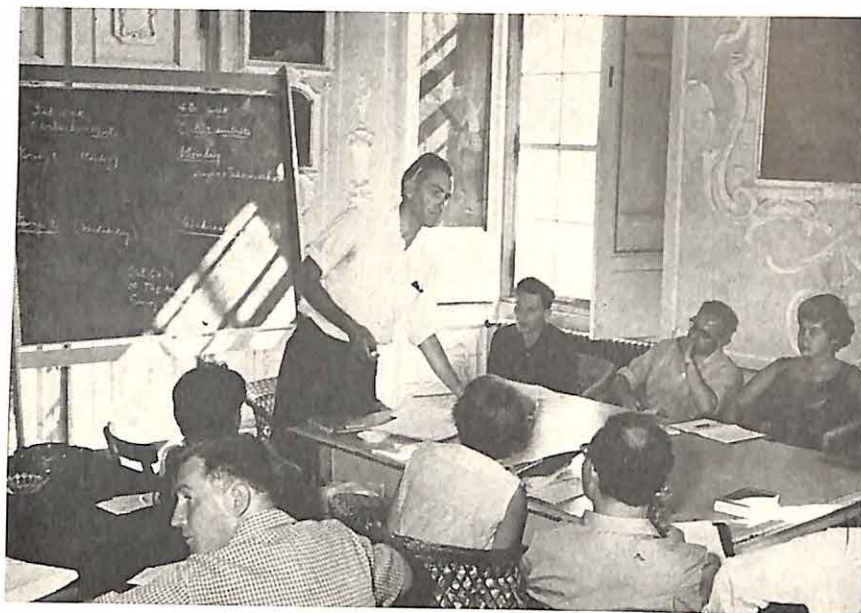


is bound to make the author appear to be claiming too much for his variables. Cautious readers will search for alternative explanations of the findings, spurious correlations, or assumptions which need to be verified. The author does attempt to take account of other forces at work, and to interpret his findings with caution.

The author is obviously aware of the dangers of generalizing from a few cases, and begins by pointing out how many hypotheses about economic development seem to work for some countries or some historical periods, and not for others. He continues to be frank about sets of data which do not support his hypotheses, and generally resists temptations to write off negative results. Poor data or inadequate measurements can lead to failure to prove a theory, but as the author points out, are unlikely to lead to the other type of error—accepting an hypothesis which is really not true. If there is a deficiency in the research strategy, it is the common one of not searching exhaustively for alternative explanations for the same set of findings.

**H**is basic hypothesis is that economic development depends on entrepreneurial behavior—taking calculated risks, planning ahead, working hard, always setting new and higher goals, expanding, trying new things—which in turn depends on a basic personality disposition—the motive to achieve—which in turn depends largely upon patterns of childhood training—early independence training. He points out that even at the individual level, total motivation depends on other factors, particularly the expectancy that hard work will be rewarded, and a high valuation of economic success as compared with other forms of achievement such as the priesthood, the army, or contemplative serenity. And individuals live in different places with different natural resources and potentials. Some of these things can be and are allowed for or controlled out; the others are hopefully randomized out by selecting a variety of countries and periods.

The evidence brought to bear in this book is staggering in its variety. While some pieces are unimpressive, the cu-



DAVID MCCLELLAND AND STUDENTS AT SALZBURG SEMINAR

mulative effect makes a powerful case for the basic hypotheses. There is laboratory evidence on the relation of the achievement motive to selection of moderate risks, working hard at tasks which are defined in achievement terms, placing high reward values on more difficult tasks. Various measures of the motive developed by scoring folk tales or literature or graphic expressions, or children's readers at different periods of history, are related to subsequent economic growth or activity for preliterate tribes, ancient Greece, pre-Inca Peru, Spain in the middle ages, the United States from 1800 to 1950, and the countries of Europe from 1920 to 1950. The data are crude, and the significance levels correspondingly low, but the procedures are careful and sophisticated. The data are frequently adjusted or controlled for other things, but they do not seem to be merely worked over until something turns out significant. There are evidences of advice from competent economists and statisticians throughout the book.

The author makes some attempts to see whether the other two personality measures in common use—affiliation and power—are also related to economic development in some way. The only positive outcome appears to be a complex and changing effect of need-affiliation through its effect on population growth and devotion to children's welfare. Attempts to relate expressed

values or attitudes to economic development also prove of little use, except perhaps for anti-traditionalism and other-directedness.

**S**OME economists who have done empirical work on economic development have begun to ask why there is not more entrepreneurial activity in the underdeveloped countries. They are likely to look first, however, at the structure of financial institutions, instability of the government, or critical bottlenecks or shortages, but may do well to include human motives as well.

In fact, we economists, with our stress on the critical role of the entrepreneur, may well have sold the psychologists too much. It is not just those who build factories who make decisions crucial to economic development. The decisions of millions of poor farmers to use fertilizer or hybrid seed, or to diversify their crops or expand their efforts may ultimately be more crucial to economic development than a few textile mills or a steel mill. The power of McClelland's hypothesis promises to be even greater in explaining the behavior of whole masses of people than in explaining that of a few entrepreneurs. What is needed for proof is a series of studies of people's behavior of types that contribute to economic development—working hard, accepting new ways of producing and consuming,



planning ahead, providing for their children's advancement. If these activities are found to be related to levels of the achievement motive both within and between countries, then new support is provided for McClelland's hypothesis. In one study in this country, a measure of achievement motive based on the strength of differentiation between easy and difficult occupations has already been found to be related to a number of activities conducive to economic development. (See James Morgan, Martin David, Wilbur Cohen, and Harvey Brazer, *Income and Welfare in the United States*, New York: McGraw-Hill Book Co., 1962.)

In the field of economic development where economists have a plethora of unrealistically simple theories, and a growing body of ad hoc empirical studies, *The Achieving Society* introduces an intriguing variable, complete with a theory as to how it operates and enough empirical evidence to justify paying attention to it.

Reviewed by BERNARD MAUSNER

MCCLELLAND'S development of measures of need for achievement and his studies of the determinants of that need are widely known. His current work carries McClelland out of the psychological clinic and the laboratory into the great world. Most of the work is devoted to an examination of evidence bearing on a single, simple, if not simple-minded, hypothesis. This is that economic growth in any society is dependent on the existence in that society of a high level of need for achievement among people playing a significant role in the economy.

The basic design of the studies in which this hypothesis is tested requires a measure of achievement needs and a concomitant measure of economic growth. McClelland's measure of  $n$  Achievement involves an analysis of the symbolic content of verbal material with the use of a procedure analogous to that developed for his earlier analyses of protocols elicited by exposure to TAT pictures. These verbal materials are supplemented by an analysis of graphic designs such as Greek vase

paintings and Andean temple decorations. The latter technique derives its rationale from an association between level of  $n$  Ach and the characteristics of doodles shown by a population of American college students. Continuous and rhythmic forms were found among subjects with low  $n$  Ach; closed and intricate figures were found among subjects with high  $n$  Ach. By analogy, an index of  $n$  Ach is derived from measurable properties of any graphic design.

McClelland has developed a simple index of economic growth or decline for use in conjunction with his measures of needs. This index is derived from such statistics as coal shipments, size of public buildings, area of distribution of the coins of a political unit and, in modern times, electric power production. The index is not derived from change in these statistics since expansion is virtually the norm in human history; it is expansion beyond the expected level that is an indication of growth. To obtain a measure of this expansion, McClelland plots a regression line for the trends over time in his economic measures. He measures growth or decay in terms of the deviations from this regression line.

THERE are a number of major investigations in which  $n$  Ach and economic growth are compared. The most extensive is based on an analysis of the content of books used to teach reading in the fourth grade in 26 countries. Samples were taken of readers used during the years 1925 and 1950. The measure of economic growth was derived from electrical power production. Another study deals with the content of myths and stories from primitive cultures and an estimate of their focus on entrepreneurship as determined by analysis of material in the Human Relations Area Files. Yet another is an historical analysis of Britain and Spain during periods of growth and decline, of Athens, of a primitive Andean society. In all of these studies the same findings emerge.  $N$  Ach predicts future economic growth; it is *not* correlated with the level of economic activity at the point in time for which the analysis of needs is carried out. Thus  $n$  Ach is

at its height in poems and plays written just *before* the great period of Athenian expansion, just before the explosive growth of the British empire, just before the Spanish conquest of the Americas. Levels of  $n$  Ach in fourth grade readers from 1925 predict economic growth in the following two decades; there is no such relationship between the measure of needs in 1925 and the economic measures for the preceding years. The same holds true for the data from 1950. For both Britain and Spain the measures of  $n$  Ach track those of the economic growth over several cycles of expansion and contraction, with a fifty year gap between the two.

SO FAR this inquiry seems to provide little more than illustration for amused lectures on the fallacies of the correlational approach; without evidence concerning the mechanism linking the psychological and the economic measures the statistical relationships have a status equivalent to the famous one between sunspots and twins in Kansas. But McClelland's data include a number of studies which yield evidence about the mechanism of the relationship. He begins with the well known thesis that there is a connection between economic expansion and Protestantism. He first demonstrates that in a contemporary setting in Germany in which Protestants and Catholics live side by side  $n$  Ach is higher among Protestants than among Catholics. But he also notes that the level of training for independence is also higher among Protestants than among Catholics, and that Protestants seem to prefer entrepreneurial, risk-taking careers rather than safe bureaucratic ones. Further data presented from a number of countries indicate that early training for independence, which is a part of the Protestant heritage, is the key to high levels of  $n$  Ach rather than the religious form itself. Further, it seems that men and women with high  $n$  Ach prefer life patterns in which they take risks—but moderate ones—in order to fulfill their needs. Where a society is so organized that people who score high in  $n$  Ach can play a major role in the economy the result is economic growth. Where



the lack of a market morality or the rigidity of society stifle the expression of *n Ach* in the economic realm, even the existence of individuals with a high level of drive will not create growth.

The implications of these findings are explored in a vigorous and lucid attack on the problem of the underdeveloped countries. McClelland suggests that massive public works are wasted effort; he urges a serious concentration on the development of entrepreneurship. Immediately, this can be done by encouraging those individuals who *are* temperamentally suited for moderate risk taking and who are focused on achievement to take part in the economy. Only in that way will a country be able to break out of the psychological bonds which inhibit economic development. On a long run basis McClelland suggests a number of specific devices for furthering independence training in childhood, and for breaking up the ancient value systems of traditionalist societies.

How seriously should one take this massive effort? The measures are certainly more impressive to non-specialists than to specialists. Most economists would consider the simple indices of economic growth somewhat suspect. And any psychologist would be likely to show some discomfort at the notion of evaluating the modal needs of a society from an analysis of its graphic designs based on evidence as tenuous as that presented here. Even though the analyses of literature and other verbal materials have a considerable degree of face validity, questions can be raised concerning representativeness of sample and the meaning of the very notion of modal needs. McClelland's answer is that the very consistency of his findings is overwhelming, despite the fact that these consist of large numbers of low order correlations significantly different only from the zero of random association. He feels that the likelihood is negligible of obtaining even a low order correlation over and over again in analyses of varied material derived from widely separate societies. This reviewer is won by the argument. The way in which the many

pieces of the puzzle fit together is uncanny; the book has the feeling of a most successful detective story. Of course there is a problem; the reviewer is biased in that he was ready to accept the thesis of the book almost before the evidence was presented. So will most of its American readers. McClelland's hymn of praise to entrepreneurship, to the seminal quality of the achievement motives, to the virtues of training for independence is consonant with most of

the values brought to it by the typical American reader. With the one caveat that one's evaluation may be culture-bound, this work should be greeted as a significant contribution to the development of social psychology and to its application to major problems of society. Rumor has it that McClelland is engaged in stimulating entrepreneurship in Tunisia. The results of his excursions into action will be eagerly awaited.

## Colors and Values in Psychotherapy

Charlotte Buhler. Preface by Edward J. Stainbrook. Contributions by Rudolf Ekstein, James S. Simkin. Comments by George R. Bach, Hedda Bolgar, Zoltan Gross, Fay B. and Maurice J. Karpf, Alvin A. Lasko and Larry Mathae.

*Values in Psychotherapy*. New York: Free Press of Glencoe, 1962. Pp. v + 251. \$5.25.

Reviewed by DONALD D. GLAD

*Charlotte Buhler, the author, is well known on both sides of the Atlantic for her publications in developmental and clinical psychology. On the other side of the Atlantic, she received her degree at Munich, taught at Vienna and at Oslo. On this side of the Atlantic, she taught at a number of universities before taking up her present private practice in Los Angeles and joining the staff of the University of Southern California Medical School. The reviewer, Donald Glad, did graduate work in both clinical and social psychology at Stanford University, receiving his degree there in 1946. He moved from Stanford to the State of Colorado where, for twelve years, his primary work occurred at the University of Colorado Medical School but he also involved himself in a varied and active pattern of services to the state and to the profession. Since 1959 he has been Director of the Division of Psychology for the Greater Kansas City Mental Health Foundation. There, among other activities, he directs the*

*group psychotherapy program. He, like Charlotte Buhler, has written in the area of values; his book, published in 1959, is Operational Values in Psychotherapy (CP, May, 1961 6, 161-163).*

THIS vigorous presentation of man, with its masterful scholarship and conceptual power demands assent in the reader who may be swept along with Buhler during her inquiring, tentative search and on to her compellingly valued closure. An inquiry into values leads readily to a systematic result. Having enjoyed the readership experience, I intend to propose some counterpoints, tentatively labelled as the values of *incompleteness, enjoyment, and implicit communication*.

Dr. Buhler provides a patterned organismic movement in her development of human values. From congenital sensitivity and actional tendencies, the infant thrusts into engagement with the world. The phases of development have the sharply cadenced tumultousness



of *L' Histoire Du Soldat*. There is a surge of figure-ground process-structure wherein the basic tendencies toward need satisfaction, self-limiting adaptation, creative expansion and the establishment of an inner order, flow, tumble or dash toward fulfillment in *becoming*.

The author's modestly stated goal is to stimulate investigation by relating human development and valuing to the technical investigation of values in psychotherapy. The goal is pursued with scholarly breadth, sensitive articulation and Promethean form. Noticing my near-reverential response, I thought of a popular value language to soften this "aspiration, how like a God" quality:

This is Prometheus—color him *heroic blue*.

This bold presentation is mirrored in the value dimensions she emphasizes: creative self-expansion and the establishment of an inner order.

There is little place for tender values—the muted reverberations of remembered play, hearth-warmed hands or a tear's caress. The esthetics accountable in her system have strength and formal vigor suggesting "... life's endless toil and endeavor . . ."

Unlike Wordsworth's heaven, Freud's pansexualism *lies about us* in childhood. Sullivanian shades of prison-house adjustment fold around the growing boy. Buhler's contrasting value emphasis focuses upon the child's early battle for autonomy and task-orientation. This conceptualization places self-values in the foreground of personality fulfillment.

THE HUMAN IMPACT of the therapist takes at least equal place with the emotional impact of motivational analysis. She reasons that *not* valuing is impossible, unnatural and unhelpful. She comes to the plausible but unnecessary position that the therapist should show considerable *explicit* openness about values in his treatment management, making constructive explorations of value potential, taking positive value stands on consequential matters, or providing value decisions and education as necessary. Being against sin makes it easy to agree with such intervention. My counterpoint is that value communication is a continuous *implicit* process in psychotherapy. Definition and use of

*implicit* processes is an insistent need in psychotherapy research. However, such recognition and use does not require that values be explicitly communicated. Buhler recognizes this, too, in her concern about the possibility that the therapist's value decisions may be wrong for the patient and explicit communication may reduce the patient's self-expansion. Moralism seems immanent in an explicitly open-value therapy. There is a likelihood of its being an inadequate substitute for more subtle contributions to the patient's development.

Such explicitness is a contradiction of Buhler's own presentation. Her proposition of *trust* is basic to her conception of creative, self-limiting achievement. *Trust* is the expectation of the opportunity to function and to become in a world which provides support and freedom. The *real self* is an identity developed from congenitally patterned tendencies *thrust* into a world which justifies *trust*. Is the priestly Promethean therapist more worthy of trust than other arbiters of value?

My critique of *open-value* leads easily to the absurdity:

These are the therapist's robes, color them invisible.

In either implicit or explicit value therapy may lurk a questionable assumption that Morris' (1956) distinction between conceived and operative values applies to people but not to therapists. Personally, I trust that therapists are more like people than otherwise—a 'fact' of some scientific and therapeutic consequence.

The playful esthetic values which often make meaningless a vivid satisfaction are lost in the Promethean thrust of Buhler's system. Should we not also propose with Chapanis (1961):

This is a scientist, color him fun.

Should my intended lightness in this review have the deftness of a full-palm finger painting, I seriously propose that Dr. Buhler's *Values in Psychotherapy* will become an important part of the equipment of many psychotherapists and social scientists. It is a persuasively broad and long conception of the human process—color it *moving*.

## Socio-Therapeutics

Carter C. Umbarger, James S. Dalsimer, Andrew P. Morrison and Peter R. Breggin. Prepared with the assistance and supervision of David Kantor and Milton Greenblatt

*College Students in a Mental Hospital: An Account of Organized Social Contacts Between College Volunteers and Mental Patients in a Hospital Community.* New York: Grune & Stratton, 1962. Pp. vii + 168.

Reviewed by JOHN M. HADLEY

All four authors of the present book were undergraduates at Harvard when they had the experiences upon which the book was based and when they prepared the manuscript. Morrison and Breggin graduated from Harvard in '58, Umbarger and Dalsimer in 1959. Morrison, Dalsimer and Breggin all entered medical school while Umbarger, whose undergraduate degree was in social relations, has returned to Harvard as a Woodrow Wilson Fellow after a Fulbright period in Denmark. John Hadley, the reviewer, is one of the improbable number of students at the Kansas State College at Fort Hays who came under the influence of George Kelly and who entered psychology. Hadley took his PhD at the State University of Iowa in 1939, and after a period as an aviation psychologist in the Navy, moved to Purdue to develop the graduate program in clinical and counseling psychology. His view has it that the future of the mentally ill is rehabilitation rather than therapy and his present interest is the training of mental health workers at the undergraduate level. His latest book *Clinical and Counseling Psychology* was published by Knopf in 1958.

THIS BOOK, written by four young men who were undergraduate students when they participated in the project, should be required reading for all hospital administrators and other personnel responsible for patient management and treatment. It is an ac-



count of organized social contacts between college student volunteers and mental patients in the wards of a state-run mental institution. The account is a convincing illustration of what can be accomplished in many mental hospitals if the archaic concepts of "mental illness" and custodial treatment can be discarded.

In their introduction, David Kantor and Milton Greenblatt describe the project as the "creation of a totally new and dynamic therapeutic force in the mental health field." We mildly disagree with this statement and, rather, see the project as another illustration of a growing effort to "humanize" the hospital and to raise the patients' general status in the hospital setting. This newer outlook is based on the concept that "patients are people" and on the general assumption that the well-being of mental patients is to some extent influenced by the social context. A derivative from this assumption is the specific hypothesis that mental patients are sensitive to and influenced by the atmosphere of the hospital. The college volunteers demonstrated, as have a number of other projects, that patients can benefit from constructive activities and personal relationships.

This "new and dynamic therapeutic force" can be utilized in any hospital and should be expressed through all personnel coming into contact with patients. If volunteers can be so dramatically effective, then a twenty-four-hour sociotherapeutic milieu should be equally effective. Psychiatric aids and related personnel must be carefully selected and given educational experiences designed to develop constructive attitudes toward patients. Ancillary therapists, nurses, psychologists, and even doctors must look upon patients as human beings with hopes, aspirations, and fears. When all mental health personnel can accept the principle that mental patients are not different kinds of beasts, but are people, then we may begin to make a bit of progress.

The book will probably not be read by the physician of our acquaintance who tells all who come on his ward that they may look around, but must not interfere. We pray that it will be

read by the administrators who support him in his cruel attempts to reduce people to vegetables so that they will be "good patients" and not give him any trouble.

Fortunately, the administrators at the Metropolitan State Hospital did allow the volunteers to come on the wards and interact with patients. More important, they allowed them to come back time and time again, even though, as the account relates, some patients were more disturbed and disturbing after they had been visited. This was the critical measure of support for

the project. Too often, social contacts are limited, or discontinued, when patients appear agitated after visits.

Since the student volunteer program began in 1954, more than 2000 students have worked in the wards of the Metropolitan State Hospital. Seven years after the inception of this program, volunteer programs has been instituted in 57 different colleges and universities throughout the country. Perhaps in time, all hospitals and treatment centers will allow patients to have social contacts—not just with volunteers, but with those paid to help.

## Social Psychology Comes of Age

David Krech, Richard S. Crutchfield and Egerton L. Ballachey

*Individual in Society: A Textbook of Social Psychology.* New York: McGraw-Hill, 1962. Pp. vii + 564.

Reviewed by PHILIP G. ZIMBARDO

All three authors, Krech, Crutchfield and Ballachey, are located at the University of California in Berkeley. Krech and Crutchfield are, of course, well known not only for their separate contributions through research but also for their earlier collaborations on texts in the area of both general and social psychology. Ballachey, now Associate Professor at Berkeley, is a new member of the formerly two-man writing team. The reviewer, Philip G. Zimbardo, is Associate Professor at New York University where he teaches, at one time or another, courses in introductory, in social, in communications and attitude change, and in group dynamics. He did his graduate work at Yale with Bob Cohen, Miller, Beach, and Hovland, served as a postdoctoral trainee at the West Haven Veterans Hospital and keeps up his contacts in New Haven through an ongoing collaboration with Seymour Sarason dealing with a longitudinal study of test anxiety in children.

WITH *Individual in Society* Krech, Crutchfield and Ballachey have set a standard against which all other

current texts unfavorably compare, and which writers of future texts will be forced to consider. The authors, with a specific audience (intelligent undergraduates) in mind, have effectively organized their survey of the major facts, principles and evidence in the field of social psychology, and this in a style both interesting and lucid.

Especially commendable is their use of format as a pedagogical tool—a technique developed in Krech and Crutchfield's *Elements of Psychology*. For example, capsule summaries of important ideas open each discussion and close each chapter and are reinforced by the recapitulation of important principles or facts as they appear within the chapter. Jargon-free glossaries are placed at the end of each chapter rather than relegated to that territory where no undergraduate ever ventures, the end of the book. And, finally, samples of research material upon which the discussion rests are boxed off to command special attention.

The range of evidence so presented and indeed supporting the discussion throughout is impressively wide. The



authors, besides citing over 600 studies of both recent and classical vintage, have drawn upon sources as divergent as Aristotle and Gilbert & Sullivan. In addition, they restrict themselves neither to human bodies nor to studies by American investigators. They facilitate the reader's comprehension and retention of the importance of the studies they cite by omitting highly technical terms, details of procedure and unnecessarily detailed statistics. Rather, they interestingly describe the essential contribution of each study and draw its implications, often with a clarity greater than that of the original reporter of the research. With few exceptions, the accuracy with which research is reported and the range of studies represented is above criticism.

HOWEVER, the real value of this text is that instead of treating social psychology as an area separate from individual psychology, e.g., as the sociology of the individual or the psychology of the group, it rightly considers social psychology as an extension of the basic principles of general psychology. Consequently, the reader's consideration of social attitudes, that topic most characteristic of social psychology, comes *after* he has been presented the picture of man as a thinking, acting, desiring, and unique organism. The chapters on the influence of language, culture and society upon the individual develop the view that social psychology is "the science of the behavior of the individual in society." And, finally, working on the assumption that "man does not live in the cosmos, he lives in a microcosm," the final section deals with group processes, leadership and the roles of the individual in small groups. The architecture of the book, therefore, is not haphazard but grows out of and clearly communicates a definite view of the nature of social psychology.

Of particular substantive value are the excellent chapters that deal with attitude measurement, formation, and change. In dealing with techniques and issues of measurement as well as with the broader conceptions of variables central to the attitude change process, the authors are neither pedantic (as, say, Kimble often was in the first edition

of his introductory text) nor are they condescending (as, for example, Ruch sometimes is in his basic text).

The text, then, is without peer both for use in undergraduate social psychology courses and as a review source for graduate students. However, the perfect text for the ideal course has yet to be written. *Individual in Society* has a number of deficiencies for which the instructor will be forced to compensate with special lectures and additional readings.

THE MOST serious of the book's deficiencies is unfortunately built into its over-all design. As I have noted above, this structure has the virtue of leading the student from principles of general psychology, through applications of these to man in his social environment, and, finally, to experimental social psychology. But, paradoxically, this very virtue is a deficiency, for the student must wait until the end of the book before he is even made aware of problems of methodology in experimental social psychology and he is *never* provided with the tools that will enable him to test the validity of studies presented in earlier, often more sociologically-oriented, sections of the book. One might argue that the student need not make such an evaluation and this indeed would be the case *if* the authors had made the evaluation for him.

This leads us to the second major and related deficiency of the book. The authors, assuming the "definitive basic text" tone, commit two errors. First, they present the material of social psychology as "fact," with the consequence that no suggestion is made of the experimental, problematical nature of the field. Moreover, although the authors claim to espouse a cognitive point of view, they neither integrate their material with regard to theory, nor do they adequately emphasize the importance of theory and the need for research guided by a theoretical approach. This flaw is most obvious in the chapter on conformity and group processes. For example, while Crutchfield's research is presented in detail, the similar, but more theoretically-orientated research of H. B. Gerard is not cited.

Second, and representing what is perhaps a graver error, they present all studies as equally valid and equally valuable. For example, most of Hovland's highly sophisticated and methodologically sound contributions are presented side by side with inferior, artifact-riddled research. The authors neither distinguish good from bad, valid from invalid research, nor provide the student with the means of making such evaluations himself.

A third deficiency of the book is that it presents no example of programmatic research, but rather gives the reader the impression that research in the field of social psychology consists of isolated, single experiments. This is a minor objection since one cannot perhaps expect depth in a comprehensive text. The instructor could compensate for this deficiency by using as a supplementary text a book like Schachter's *Psychology of Affiliation*.

Finally, readers who were dissatisfied with *Essentials of Psychology* because of the scant treatment it gives S-R behaviorism will object to this text on the ground that it ignores even such prominent learning theory approaches to social psychology as Miller and Dollard's *Social Learning and Imitation* and Doob's classical definition and analysis of attitudes.

IN conclusion, *Individual in Society* is an important book from an historical perspective. Comparison with the earlier *Theory and Problems in Social Psychology* shows a change in focus that reflects a general change of direction in the field. Social psychology, as this text demonstrates, has increasingly shifted its attention from the study of social *problems*, like prejudice and industrial conflict, to the isolation of psychological *variables* studied in whatever social contexts best reveal their operation. A related development in the field, the increasing shift of attention from group phenomena *per se* to the individual, is also reflected in the book. In the field we might observe this trend in the progress of Festinger's theorizing from his communication theory of 1950, which attempts to handle problems such as group cohesion and patterns of group



attraction; to his comparison theory of 1954, which deals with discrepancy of attitudes and abilities between an individual and other individuals; to his dissonance theory of 1957, which deals with discrepant sets of cognitions within a single individual that may arise and be reduced under asocial as well as social conditions. In the book we find this trend partially reflected as the "group dynamics" approach which the authors found exciting in 1948 yields precedence to a "group-less" social psychology, one in which implied groups replace actual ones and in which the individual psychological processes consequent upon social learning and interactions are studied.

All things considered *Individual in Society* is a very fine book which gives ample testimony to the creativity and impressive productivity of social psychologists.

## Executives Manage

Roger Bellows, Thomas Q. Gilson  
and George S. Odiorne

*Executive Skills: Their Dynamics and Development.* Englewood Cliffs, N. J.: Prentice-Hall, 1962. Pp. viii + 334. \$10.00.

Reviewed by ROBERT F. PEARSE

*Bellows, Gilson and Odiorne owe their collaboration in this book to the fact that they coincided for a while on the faculty of Rutgers University. Roger Bellows is still there and is Chairman of the Psychology Department. Thomas Gilson is also still there as Chairman of the Management Department. George Odiorne, an economist and specialist in industrial management, is now Director of the University of Michigan's Bureau of Industrial Relations. The reviewer, Robert F. Pearse, did his graduate work at the University of Chicago under Carl Rogers, Lloyd Warner and William Henry and since then has worked as a clinical industrial psychologist with Worthington Associates in Chicago, with*

*Fred Rudge Associates in Connecticut, with the Kaiser-Frazer Corporation, and, most recently, with Stop and Shop, Inc. in Boston where he is now Director of Personnel.*

ALL of us who work in any kind of organization develop personal reactions to the people above us who have executive responsibility. We label some of them "effective" and others "ineffective," depending on our perceptions of the results they achieve.

However, when we try to analyze their skills and performance on the basis of more "objective" standards, we run into a number of problems. To begin with, it is hard to separate the way an individual executive behaves on the job from the interpersonal and organizational context within which he operates. Even mediocre executives can look good if they are supported by a team of able subordinates. Conversely, measured by the criterion of immediate profits, an exceptionally able man may appear to be a failure if he happens to be struggling to rebuild a weak organization.

Partly for these reasons, just what management philosophies and methods really make for executive success is still the subject of endless debate. There are at least three separate schools of thought on this problem—the traditionalists, the corporate planners, and the humanists—each insisting that its particular approach provides the real "key" to successful management.

In *Executive Skills*, Bellows, Gilson, and Odiorne look at the executive's job primarily from the traditionalist or executive-as-manager viewpoint that places major emphasis on the familiar management skills—planning, organizing, controlling, and motivating. At the same time, however, the book outlines a number of techniques currently being advocated by the humanist school, which views management in terms of "democratic leadership."

The authors place considerable emphasis on learning theory in relation to methods of developing efficient skills in reading, human relations, communications, and interviewing. Other chapters similarly deal with the skills required for successful counseling, working with groups, and delegating.

In their detailed discussion of creativity, in which they place particular emphasis on the organization's need for creative thinking among its executives, the authors take due note of recent research in this area. They also devote a fair amount of space to another topic of growing management concern—how to handle resistance to technological change.

On the whole, *Executive Skills* provides a comprehensive account of current thinking about management as the traditionalists view it. Unfortunately, the book is written in a somewhat academic and formal style, which makes it rather heavy going for the lay reader.

It is interesting to note how *Executive Skills* differs from other recent books in the field in terms of what their respective authors believe to be the crucial factors making for success in the executive job. Thus Blake's *Group Dynamics* advocates the use of sensitivity training in leadership skills to build a collaborative team of subordinates. Hardwick and Landuyt, in *Administrative Strategy*, stress the need for executives to become flexible strategists who can use whatever skills are needed to maximize results in different situations. Other authors—Leavitt, for example—see the coming age of computerized management as one in which the successful executive will be the one who can quickly adjust his organization whenever it goes off its planned profit track, much as a space technologist controls an unmanned rocket. To this group, the most important executive skills of the future will be those required to interpret trends signaled by computerized information, and speedily shift company operations accordingly.

These varied approaches underscore the need for more research on the underlying factors crucial to executive success. Not until we know what these factors are can we specify the skills an executive needs to develop in order to succeed in his particular situation.



*The field is so extensive that workers in the different parts of psychology hardly speak the same language.*

—STANLEY COBB





# Men, Machines and Systems

Robert M. Gagné (Ed.) and others. Foreword by Arthur W. Melton

*Psychological Principles in System Development*. New York: Holt, Rinehart and Winston, 1962. Pp. v + 560. \$9.00.

Reviewed by H. WALLACE SINAIKO

Robert Gagné, editor of the present volume, did his graduate work in experimental psychology at Yale and Brown Universities, receiving his doctorate from the latter in 1940. He has taught at a number of places in the east, has served in the Armed Forces, has directed laboratories for the Air Force and is presently Director of Research at the American Institute for Research. The reviewer, H. Wallace Sinaiko, did his graduate work at the University of Minnesota under Longstaff and Paterson and at New York University, where he did his dissertation under C. E. Skinner. His extensive and principally experimental research on man-machine systems has been done at NYU's Engineering Research Division, at the Naval Research Laboratory Systems Division and at the University of Illinois Control Systems Laboratory. Presently he is a member of the Technical Staff of the Institute for Defense Analyses.

PSYCHOLOGISTS have been involved in system development for a long time. Pressures arising from military need have given impetus to what is now called the "systems approach." As World War I was to the flowering of personnel psychology, selection, classification, and other systematic differentiation of human abilities, so did World War II bring academic experimental psychology to bear on problems of designing equipment to match human users' perceptual and motor capabilities. Perhaps the decade-and-a-half following the Second War will become known as that era which produced a further refinement of applied behavioral science in the form of a unified ap-

proach to the design and development of large man-machine systems. During this last period there has appeared a small number of books which describe psychological applications to particular types of system problems. Most of these have concentrated on such things as the sensory psychology of individuals (i.e. audition and vision), on the design of controls and displays, or on motor behavior. However, certain important contributions of behavioral science have gone unmentioned in the systems context. The most obvious of these are: a) the technology of training and, b) the performance of groups. It is these areas on which *Psychological Principles in System Development* puts most of its emphasis.

There are some chapters in the book containing old and familiar material for most readers. Horst's section on classification and personnel selection is one of these. Also, the chapters on training by Biel, Crawford, and by Boguslaw and Porter have familiar and recurrent themes. A reversal of this tendency is the chapter by Wulfeck and Zeitlin which, while containing classic data from sensory psychology, is done in a new context; this section of the book will provide useful answers to questions about man's capabilities and limitations.

There is, in addition, a considerable amount of original material in *PPSD* and, where it appears, it is handled very well. Davis and Behan, in writing about the evaluation of systems in simulated environments, build an effective case for a new method which will be intriguing to many psychologists. The chapter on men and computers, by Edwards, also treats an area that has only begun to

have an impact on our lives: the revolution produced by very high speed machines in the handling of data. The Edwards chapter is particularly good when speculating on principles for combining human beings and computers. Many of these hypotheses will become subject to experimental verification; for example, men should aid computers in such tasks as sensing, extrapolating, and decision-making.

Glaser and Klaus provide a significant contribution with their chapter on proficiency measurement. The emphasis on importance of assessing unique or infrequent, but critical, behavior in systems is an example of something new and different in *PPSD*. So is the proposal that systems be tested under overload conditions. Very little is understood about how future man-machine systems will react to catastrophic events. Glaser and Klaus, although their approach is somewhat indirect, have some potentially useful ideas about filling this gap.

For an answer to a question put to many of us these days, "Just what does a psychologist do in these big systems programs?" Kennedy's chapter is good. Incidentally, Kennedy makes only a passing allusion to an engineering development which is at the heart of most of the systems treated in the book. I refer here to radar, the technology of automatic remote sensing. The development of radar has had a greater influence on applied experimental psychology of the past two decades than has any other single engineering phenomenon. This will be obvious to any reader of *PPSD*, although none of the contributors makes the point explicitly. This overwhelming influence of one type of system component is responsible for a major drawback of the book: there is entirely too much dependence on a single type of military system, particularly the air defense model. There are, however, some refreshing exceptions to the frequent use of radar for illustrative material and one of these is in the chapter by Boguslaw and Porter on team training: the model here is a school training program.

Crawford's tedious treatment of training concepts contains an unintentionally ironic commentary on still another problem faced by workers in this field. Describing in great detail a four-year research program on the training of



Army armoured specialists, Crawford shows how the research resulted in a significant reduction of training time: from eight to six weeks. Then, because of an otherwise unexplained phenomenon known as 'federal statutes,' the Army decided to retain the original eight week training program. (I wonder if psychologists in system development work drink a little bit more than their colleagues in other fields.)

Highlight of the book, because it represents the most original contribution, is the chapter by Kidd on equipment design and human tasks. This is probably the best treatment available on factors affecting human information-processing tasks and ways to incorporate these factors in equipment design. Kidd presents a new, and very useful, vantage point for considering operator characteristics: the notion of error analysis. Also, this chapter points up a new source of difficulty in developing computer-based systems. That is, designers of such systems are likely to ignore or distort human functions when the latter can't be easily programmed to be compatible with the computer.

A MAJOR weakness of *PPSD* is the uniform absence of experimental data: there aren't very many numbers for the quantitative-minded reader. This omission probably represents a fundamental shortcoming of most work in system development. Military security, although never mentioned in the book, is another reason the authors of *PPSD* have so few examples of completed work to report. Thus, Gagné and his collaborators were sometimes forced to create fictitious systems, about which there could be no real data. Hopefully a revised edition of *PPSD*, coming after some of these data have been declassified, will have a more quantitative orientation.

Some material in *PPSD* is not treated in enough depth. Edwards' section on computer programming is superficial and confusing. Miller's chapter does not, as promised by the editor, demonstrate how 'remarkably successful' the technique of task analysis has been; Miller gives no data nor does he illustrate any of the techniques of task analysis. The chapter by Wulff and Berry on job aids is a rambling contribution which could

have been condensed or omitted entirely. In all, about a third of *PPSD* probably won't be required reading when the book is used as a text.

Gagné has assembled a talented and, for the most part, visible group of authors. There are, however, some conspicuously absent people and institutions who should, in a book of this type, have been represented because of their early and continuing contributions. There are no direct inputs from any of the several government laboratories which have figured so prominently in psychological research related to system development. (For example, Naval Research Laboratory, Navy Electronics Laboratory, Navy Training Devices Center and Air Force's Aerospace Medical Laboratory.) Among the significant university centers not included among contributors to *PPSD* are Ohio State's and Illinois' Aviation Psychology Laboratories and Southern California's Electronic Personnel Research Group. Also, Johns Hopkins (Department of Psychology and the Applied Physics Laboratory), and New York University's Engineering Research Division (Project Cadillac of the early 1950's) could well have provided some badly needed experimental data. The most conspicuous omission from *PPSD*, doubly interesting because his research is mentioned many times in several chapters, is the work of Robert L. Chapman. To Chapman and his colleagues, whose experiments on the behavior of large man-machine systems were done in the RAND Corporation's Systems Research Laboratory about a decade ago, can be traced many of the ideas being put to use today in system work, particularly in training and system evaluation. Perhaps it is just as well that Chapman does not occupy an author's niche in this book; the work at RAND deserves an integrated and full-scale treatment of its own.



*For spending the wealth of others will not diminish your reputation, but increase it, only spending your own resources will injure you.*

—MACHIAVELLI



## Brain Blocks in Reading

John Money

*Reading Disability: Progress and Research Needs in Dyslexia.* Baltimore: Johns Hopkins Press, 1962. Pp. v + 222. \$5.00.

Reviewed by HENRY P. SMITH

The editor, John Money, is a New Zealander who received his PhD at Harvard and is now Associate Professor of Medical Psychology and Pediatrics at Johns Hopkins University. The reviewer, Henry P. Smith, received his PhD from the University of Iowa and taught at Syracuse University for a short while before going to the University of Kansas, where he has been since 1947, where he is now Professor of Educational Psychology, and where he spent the years between 1947 and 1959 as Director of the Reading Laboratory. Among his publications are two textbooks, *Psychology in Teaching*, first brought out in 1954 with a second edition appearing in 1962 and, with Emerald Dechant, *Psychology in Teaching Reading* (CP, June 1962, 7, 228).

THIS BOOK focuses upon specific dyslexia—those reading difficulties that are in the realm of psychoneurology rather than those that may be traced to sensory defects, inadequate intellectual development, lack of experiential opportunity, improper pedagogical method, or to inadequate motivation.

Actually, the book is a series of thirteen papers prepared by participants in a conference on dyslexia and related aphasic disorders. The conference was held at the Johns Hopkins Medical Institutions on November 15-17, 1961, under the sponsorship of the departments of Pediatrics, Psychiatry, and Ophthalmology and was supported by a grant from the Association for the Aid of Crippled Children.

Unlike many conference reports, the book reflects a careful planning that has resulted in an intensive coverage of important facets of a rigorously delimited



area. Overlap among papers seems to have been held to a minimum, and the editor has provided a consolidated bibliography and a glossary.

The goal of the conference has been to bring the problems of dyslexia under the combined scrutiny of the fields of neurology, psychiatry, psychology, and ophthalmology. As might be expected, the conferees agreed that dyslexia is a proper interest of psychoneurology rather than the exclusive territory of pedagogical method. Although they were unable to formulate a definitive description of the syndrome of dyslexia, they did conclude that such a syndrome exists. However, they recognized that frequently the distinction between dyslexia and milder or more transient forms of reading disability must be made on the basis of degree and relative permanence rather than on the basis of identifiable symptoms.

Since the emphasis of the conference reports is upon research progress and needs in the identification of neurologically-based disabilities, the remedial teacher or remedial clinic director will discover no concrete methodological suggestions. However, if he can persist through the maze of neurological terminology, he should gain a better understanding of why certain of the scholastic disabilities that he encounters seem to have no obvious cause and respond but little to traditional remedial methods.

Schiffman, one of the participants in the conference, seems to identify, by means of a quote from a recent paper by Clemmens, the primary goal of the conference: "In an age of specialization we have not cultivated the inter-professional communications and exchange of ideas which are necessary for our mutual understanding of these complex problems." The publication of the papers read at this conference plus the postconference review in which the editor attempted to integrate the contributions and provide some bridging from medical science to pedagogy is certainly a step toward inter-professional intercourse on the complex problems of the why of reading disability and the *how* of its treatment. To the reviewer, the conference seems to have been fruitful, and its reports, although difficult to read, well worth the reading effort.

## Weak Theory, Weak Impact

H. J. Eysenck (Ed.)

*Experiments in Personality: Vol. I: Psychogenetics and Psychopharmacology. Vol. II: Psychodynamics and Psychodiagnostics.* London: Routledge and Kegan Paul, 1960. Pp. vii + 262 and vii + 333. 40s each Vol.

Reviewed by HOWELL H. STORMS

*The editor of the present volume, H. J. Eysenck, by now needs no introduction to the readers of CP and that is what he will get. The reviewer, Lowell H. Storms, is presently Supervising Psychologist and Assistant Professor in Residence at the Neuropsychiatric Institute of the UCLA Medical Center. He did his graduate work at the University of Minnesota and in the year 1956-57 was a Fulbright Scholar at Maudsley's Institute of Psychiatry. His publications have dealt with verbal mediation, the validation of psychological tests, the effects of punishment on instrumental behavior, and, more broadly, with general matters of learning and personality.*

THIS pair of volumes was prepared under the authorship of H. J. Eysenck and ten others, all ten of whom are or have been Eysenck's students or members of his staff at Maudsley Hospital. This institutional affiliation is the main unifying fact about the book, although as the title suggests, the presentations therein of animal and human experiments, statistical methodology, etc., do have some relevance to personality. In his introduction, Eysenck sets forth two goals to be served by publication of this book. One is to show how research which would ordinarily be published in widely disparate journals fits into a unified research program. The other is to test "in a more decisive fashion than had been possible before" the theories advanced in *The Dynamics of Anxiety and Hysteria* (reviewed by Lykken, *CP*, Dec 1959, 4, 377 ff.). It seems appropriate to use these goals as guidelines in reviewing the book.

Very little but positive evidence regarding his theories has been presented in previous publications by Eysenck. However, in his present book he has sponsored the publication of findings that include a great deal of negative evidence from his own laboratories. Relationships between Introversion-Extraversion or Hysteria-Dysthymia and such variables as eyeblink conditioning, reminiscence on the pursuit rotor, Archimedes spiral after effect, figural after effect, and the effects of drugs, findings which are central to Eysenck's discussion in *The Dynamics of Anxiety and Hysteria*, largely vanish in the present set of investigations. Nowhere is any reference made to an article by Storms and Sigal (1958) in which it was shown that considerable negative evidence regarding these relationships was available in the same experiments cited by Eysenck in *The Dynamics of Anxiety and Hysteria*. Lack of awareness of the previously available evidence cannot be pleaded, but because this evidence is ignored, comments such as that of Claridge (p. 122, Book II) that his reminiscence results on the pursuit rotor were "somewhat different from that predicted" seem to express an element of surprise.

A number of interesting contortions resulted from attempts to reconcile negative results with Eysenck's theories. Several authors found significant insignificance; e.g., Willet, p. 112, Book I ... "failed to demonstrate any significant difference, although the group means were clearly separated in the predicted direction"; Holland, p. 209, Book II, found that ... "discrimination is fairly clear but not significant statis-



tically," and Eysenck and Eysenck, p. 233, Book I: "Susceptibility to these drugs appeared to be a constant personality feature, and correlations of this susceptibility with Extraversion and Neuroticism were found, although not at a statistically significant level of confidence."

Along the same lines, we find Willet (p. 188, Book II) asserting that a conditioning effect was "rather stronger in the main study" than in a control study, although the mean reported on the previous page for the control study is greater than in the main study. Again, on p. 174, Book II, Willet argues that although the MPI neuroticism scale (N) failed to correlate with salivary conditioning, persons who get low scores on neuroticism must also get low scores on salivary conditioning. Since K and D (other questionnaire scores) "are in a sense the inverse of N, and K and D correlate negatively with salivary conditioning, a positive relationship between salivary conditioning and N might be said to have been demonstrated." The naiveté of Willet's logic is matched by his naiveté concerning the phenomena he has investigated; on p. 173, Book II, he calls "unexpected" the fact that emotional stress leads to reduced salivation, although the ancient Chinese used this fact in a crude lie detection technique. Such shortcomings detract needlessly from the quality of the book and suggest insufficient editorial guidance.

Many readers will also be puzzled by the mechanism used by Claridge to deal with his results. Of his nine "objective behavior tests," he found that only two yielded the expected differences for Hysteria-Dysthymia, and two for Introversion-Extraversion, and no test yielded positive results for both comparisons. In his questionnaire findings, hysterics, who are considered in the theory a criterion group for extraversion, were if anything, more introverted than normals on the E scale, as in a number of previous studies. Claridge concludes that these results provide "fairly substantial confirmation of the theoretical framework" (p. 141, Book II).

It is interesting that none of the circumlocutions illustrated here were employed by the authors of the sections on

psychogenetics, psychodiagnostics, and statistics, all of which are virtually unrelated to Eysenck's theory. It is also interesting that over half of Book I is devoted to intensive research designed to test Eysenck's drug postulate (essentially that depressant drugs should be extravertizing and stimulant drugs introvertizing) and that the research was carried out by using two depressant drugs with no attempt to explain why there was no condition investigating the effects of stimulant drugs.

**R**EGARDING the evaluation and function of theories, Eysenck's epilogue is very interesting and worthwhile reading. He makes a distinction between "weak" and "strong" theories. Psychological theories are characterized by varying degrees of weakness and, Eysenck rightfully points out, weak theories are difficult to disconfirm because there are so many poorly understood factors which can bring about negative results through no fault in the theory. However, Eysenck says that the successful test of an hypothesis "is unlikely to have a cause other than the one specified." What about alternative hypotheses which fit the results equally well? Eysenck's statement is tantamount to saying that if someone hypothesized that schizophrenics have holes in their brains and therefore should be slow learners and subsequently found them poorer than normals on a learning task, any other cause than that hypothesized would be unlikely. Eysenck argues cogently that the greatest value of weak theories is that they direct attention and research toward problems worthy of investigation. It might also be argued that excessive devotion to such a theory could lead to perseveration in unproductive approaches and blindness to important alternative viewpoints. Eysenck has, at least, given us a workable pragmatic criterion for evaluating a theory in terms of the degree to which it "gives rise to worthwhile research." To some extent, this book offers us an opportunity to consider Eysenck's theories in the light of this criterion.

An examination of the reports in these volumes tempts one to the harsh conclusion that there is an inverse relationship between the quality of the con-

tributions and their relevance to Eysenck's theories. Paradoxically, it is the very lack of unity and cohesiveness in the book that makes possible the inclusion of some excellent contributions with practically no relationship to Eysenck's theories. The first half of Book I, written by Peter Broadhurst, consists of a thorough, scholarly presentation of modern methods of biometrical genetics and data from a series of experiments illustrating their application to the investigation of heritability of behavioral dispositions in rats. In Book II, R. W. Payne and D. H. G. Hewlett present an extensive investigation of the similarities and differences among groups of mental patients. This study constitutes one of the finest available contributions to a clearer understanding of schizophrenic behavior. Patrick Slater's section in Book II is largely methodological. He presents an enlightening discussion of the meaning of factor analysis and techniques for analyzing the discriminance among groups, giving rationales for and examples of the appropriate use of these methods.

Holland and also Martin, in chapters more closely related to Eysenck's theories, display a sophisticated interest in their phenomena and present some interesting results largely unrelated to the theory in question. The content of this book suggests that, while some worthwhile research is done in Eysenck's laboratories, it is not likely to be given its impetus by Eysenck's theory *qua* theory, although Eysenck's encouragement of the research is undoubtedly an important factor.

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*Discontent is the mother of progress.*

—ASHLEY MONTAGU





# The Rite of the PhD

David Mechanic

*Students Under Stress: A Study in the Social Psychology of Adaptation.*

New York: Free Press of Glencoe, 1962. Pp. v + 231. \$5.00.

Reviewed by DONALD R. BROWN

The author, David Mechanic, is a sociologist who took his undergraduate work at the City College of New York and his graduate work at Stanford University. He is now at the University of Wisconsin where he is Associate Professor of Sociology and Co-director of the Graduate Training Program in Mental Health. The reviewer, Donald Brown, received his degree in psychology about ten years ago from the University of California at Berkeley. Since then he has been, for the most part, at Bryn Mawr College. In addition to his work there he has done a variety of other things, including service as Research Consultant and Research Associate with the Mellon Foundation at Vassar College, an involvement that led to his preparation of a chapter in *The American College* (CP, Nov. 1962, 7, 393). He has long maintained an interest in personality development and the educational process, with a special emphasis on non-intellective factors in academic achievement. His publications, which are numerous, and his professional activities, which are varied, testify to an active and productive expertness in this broad area of concern.

DAVID MECHANIC, Associate Professor of Sociology at the University of Wisconsin, subtitles his book "A Study in the Social Psychology of Adaptation." He might well have given it the less academic but more descriptive title, "The Doctorate Without Pain is Hardly Worth the Game."

The author spent a year as a post-doctoral fellow living in the best ethnographic tradition among a group of twenty graduate students and an equal

number of faculty members while the students went through the throes of the written preliminary examinations. Thus the study joins an increasing volume of work on the social psychology of higher education [c.f., *The American College* (CP, Nov. 1962, 7, 393f) and *Boys in White* (CP, Nov. 1962, 7, 399f.)]. The present volume concentrates on another institutionalized aspect of the academic culture, which, so far as this reviewer knows, has not before been given formal scrutiny by a social scientist. For those who seek deeper, if non-empirical, insights and who wish at the same time to maintain their sense of humor as well as feeling of compassion for the plight of the graduate student, I recommend George Stewart's novel, *The Doctor's Oral*, as a companion piece.

The study was conducted in a semi-formal manner through weekly individual interviews with nineteen of the students for a total of ten interviews—the last of which followed the announcement of examination results. In addition, sociometric "and other quantitative data were elicited from these students" and a month before the examinations "a complete comprehensive questionnaire covering their examination experiences" was administered to the students. The married students' spouses were interviewed, and each faculty member completed a questionnaire designed to elicit their "perceptions of their interaction with graduate students, as well as their own attitudes toward the examinations." Some faculty members were interviewed and numerous observations were made during informal contacts in the department and at a pre-exam graduate student party. A month

after the exams, a short questionnaire was sent out to the group under study as well as to some other students who were in the same department but not taking the exams. The professional reader will be disappointed to find that none of the questionnaires or interview schedules are presented, nor are there coded breakdowns of the data except in terms of very simple frequency and percentage tables. And these deal only with items the author chooses to refer to in support of the text.

MECHANIC does not merely present a simple empirical investigation of the reactions of graduate students to the stressful conditions that obtain during this trying time of their professional lives. This, of course, he does, and with the methods outlined above; but in addition, he makes a conscientious effort to fit the study into a theoretical context of bio-social stress.

At the beginning of the volume and again in more detail at the end, the author reviews the development of the concept of stress, proceeding through the work of Hans Selye at the bio-medical level on to the treatment of stress by those psychoanalytic and psychosomatic investigators who place particular emphasis on the ego defense system as a method of coping with threat. He concludes, not surprisingly for a former student of E. H. Volkart and an active member of the expanding group of sociologists working in medical and psychiatric settings, that the above approaches have failed to do justice to "social contingencies of stress situations." Therefore he turns to the work of W. I. Thomas for a theoretical handle to mediate the individual reaction to threat, or "crisis" in Thomas' terms, to the social context in which the crisis takes place. This bridge is made by Thomas' notion of "the definition of the situation" which the individual formulates when habitual ways of responding are disrupted by the presence of novel stimuli in the environment. Cognitively oriented psychologists will be familiar with this approach and will appreciate the accompanying dependence on phenomenological data. (Parenthetically, there is not a single primary reference to Thomas in the bibliography.)



Thus the individual's reaction to stress is mediated through a social context as follows: individuals faced by threats resulting from frustrations in goal seeking react by adopting personal versions of socially provided means for accomplishing their ends. These ends are often socially determined and valued and therefore the meaning given to a social stimulus will be determined largely by the social context in which it appears. Stress then becomes a response rather than a stimulus, a stimulus dependent on the interpretation of the individual.

Shades of response defined stimuli! Personally, such a notion doesn't frighten this reviewer at all, providing the social context and individual reaction are used to define the response, stress, in an objective manner. This is what Mechanic attempts to do in his study. He starts by defining the social context and interactions of the student-student and student-faculty world. Then, by use of interview and questionnaire data, he establishes a "situation" in the Thomas sense; in this instance the situation is one characterized by high stakes, high ambiguity, and seemingly impossible demands. This type of situation, all will admit, requires "adaptive coping," at least on the part of healthy personalities. Emphasis is placed on how the group influences the mode of adaptation rather than on purely rational decisions by individual students.

The creative use of interview data in tracing this adaptation process over time makes for an interesting story. However, one wonders if the sociologically oriented thesis can best be tested at such an individually oriented level of data. This reviewer felt that "meaning" in both the Thomas and cognitive traditions did not come through at the social group level, but rather from the phenomenal reports of the students. Therefore, in the absence of a larger representative sample and adequate controls, one feels that more useful insights could have been gleaned by a frankly clinical and more intensive presentation of individual modes of adapting to this crisis of early professional adolescence. The resulting hypotheses could then be tested at the social level by survey and social structural analyses.

## Too Much Simplicity on Adolescents?

Ira J. Gordon

*Human Development: From Birth through Adolescence.* New York: Harper, 1962. Pp. vii + 400.

Dorothy Rogers

*The Psychology of Adolescence.* New York: Appleton-Century-Crofts, 1962, Pp. v + 600.

Reviewed by F. L. WHALEY

*I. J. Gordon, author of the first book, received an EdD from Teachers College, Columbia University and since then has taught at Kansas State College, the University of Maryland and at the University of Florida, where he is now at home as a Professor of Education. Dorothy Rogers, author of the second book reviewed here, took her PhD from Duke University and for a number of years has been at the State University of New York at Oswego, where she is a Professor and Coordinator of Child Development. The reviewer, F. L. Whaley, did his graduate work at the University of Michigan under Willard Olson, working there on a longitudinal study extending from nursery school through adolescence. He also had experience at the University of Michigan Fresh Air Camp and has had contact there with W. C. Morse, Fritz Redl, Ron Lippitt and Norman Polansky. Since 1954 he has taught at the Pennsylvania State University where he is now an Associate Professor, and where he continues his contact with problem children.*

GORDON's *Human Development* is unabashedly of the "self-psychology" school. This is acknowledged in the preface, prefaced in the acknowledgment, and signaled by the 161 pages (out of a total of 389) carrying a reference to the self. Perhaps many of the other topics

to be treated here follow from this.

The author does not explicitly indicate the market at which the work is aimed; several features suggest, however, that the reader can be rather unsophisticated. Methodology is not emphasized, either in a special section, in the citation of studies, or in concern for the adequacy of conclusions. Most of the research is reviewed only in conclusion form; no tables report correlation coefficients, and one of the rare references to confidence levels utilizes the .50 [*sic*] level.

The perennial problem of organizing by age or by topic is confronted again, and the toss went to age. As a consequence, some topics are treated several times, but never in depth. Intelligence, for example, is treated in at least four different sections. This makes it difficult to get a clear picture of this crucial concept, especially in its longitudinal sweep. Although the last chapter states that "we have consistently described intelligence . . . as reflecting learned patterns . . . rather than as a stated quantity or a 'given' native ability, fixed and unchanging," some readers may wish that more of the evidence used by Berlyne and Hunt to explore this interpretation had been presented here. Also, I think that the instructor is overly burdened to prepare the students for, and elucidate upon, the brief description of Guilford's model. A more detailed anal-



ysis of the longitudinal studies of intelligence, with quantitative data (beyond the illustrative case studies) and details of methodology are probably more convincing, and are of more value, than are a few conclusions.

The scope of the work (prenatal through late adolescence) imposes limits on what can be treated. Certain standard topics, such as learning, have been excluded, and wisely so. Many others, however, are given a cursory glance, and so steal space that might have been used for intensive development. Language, concepts, creativity, and beliefs, are among those given a "brush-block."

In topics given intensive treatment, readers may miss some standard references. In the discussion of the family, there is no discussion of patterns of child-rearing in the Baldwin, Radke-Yarrow, or Sears, Maccoby and Levin sense. The recent interest in social (maternal) deprivation is similarly ignored, perhaps with good reason. Missing also are references to level of aspiration and their relationship to the self concept, and to performance. Although sex-role models are discussed frequently, the Sears father-absent studies are curiously absent.

The unique contribution of developmental psychology is the emphasis on the development sweep. The age-graded organization used here requires a great deal of transitional treatment, suggesting future developments at one age and reviewing previous development at the next. Many may feel that more of this could have been done, although there are frequent attempts to emphasize the developmental aspect.

**G.** STANLEY HALL is said to have discovered the adolescent, and his contribution is cited by Rogers as standing "halfway between the fictionalized generalizations of past centuries and the controlled observation and experimentation of the present." It is tempting but probably inaccurate similarly to assess Dorothy Rogers' *The Psychology of Adolescence*.

One of Hall's contributions, the questionnaire, is utilized here with a vengeance. The 34 separate citations of Remmers and Radler (*The American Teenager*, Indianapolis: Bobbs-Merrill,

1957) only illustrates the extreme dependence upon the method. The innumerable references to conclusions based on questionnaires, interviews, and observations provide little solace to aspirations of a science of behavior. Indeed, one sees little movement in that direction. Although 7 lines of the Introduction are devoted to the questionnaire and interview, it is apparently not felt necessary to reinforce this by describing the source of Remmers' and Radler's work, nor to describe in any detail the other studies mentioned. (The Hartshorne and May classic is a rare exception and not all will agree with the treatment.) Instead, Chapter Four, a not unusual example, has about 22 references to secondary sources. In several cases one reads that "a study shows," and is given a reference not to the study itself but to an unspecified page of a secondary source!

An even greater reliance is placed upon self reports, many apparently gathered by the author. These are used effectively. They highlight in a vivid, personal way the adolescent as an individual living in and reacting to a complex universe. There is no pretense that these are anything but illustrations.

Perhaps because so little emphasis was placed upon "the controlled observation and experimentation of the present," the treatment of many topics is more superficial than the restricted age span (pre-adolescent to young adulthood) would permit. While attitudes toward objects are frequently mentioned, there is no intensive treatment of their development. Freud is cited as being second only to Hall in influence in this century, but that is the only reference to Freud. In the chapter on "Psychobiological Sex Roles," the treatment of psychosexual development yields no references, not even to the familiar Freud, Sullivan, or Erickson.

We are told in the Preface that the purpose of this volume is to assist anyone concerned with adolescents, and that an understanding of the material requires no prior psychological training. Consequently, each unit (and many sub-units) contain advice on what to do to, and for, the adolescent. These range from the general advice to take one's own attitudes into account, to such specific suggestions (tentatively presented,

to be sure) as "abolish car licenses for youths under 20, because research reveals an inverse relation between owning cars and getting high grades," with a reference to a Saturday Evening Post article.

This reviewer takes issue with such purposes as these two books espouse, and with the implementing of them. One could say that they are written at an introductory level and therefore should not demand much sophistication in methodology or analysis, and that generalizations are sufficient at this level. On the other hand, recent developments in many areas (e.g., DNA & RNA in genetics) enforce a more tentative view of present theory and generalizations, and obligate us to prepare students to evaluate and learn from new research. This cannot be done unless students are confronted with research and are systematically taught to be concerned with the relationship of the conclusions to the rationale, method, and analysis. Users of psychology, such as teachers at whom these books are aimed, have too long been told that they can, with little or no background, pick up the principles necessary to an understanding of children. Or worse, they are told "how to" deal with children. The complexity of our field and research defies this.

## From Molecules to Madness

Robert Roessler and Norman S. Greenfield (Eds.)

*Physiological Correlates of Psychological Disorder*. Madison, Wis.: University of Wisconsin Press, 1962. Pp. v + 281. \$6.50.

Reviewed by ERNEST S. BARRATT

Robert Roessler, one of the editors of the present volume, is a physician who is now Chairman of the Department of Psychiatry at the University of Wisconsin Medical School, and serves also as Director of the Wisconsin Psychiatric



Institute. His collaborator here, Norman Greenfield, is a psychologist with a degree from a well known institution of higher learning in Berkeley, California, who now, like Roessler, is located at the University of Wisconsin Medical School. The reviewer, Ernest Barratt, received his degree from the University of Texas in 1952, has done postgraduate study in the Electroencephalography Laboratory at Johns Hopkins Medical School and has held a USPHS research fellowship at the Brain Research Institute at UCLA's Medical School. He has taught at the University of Delaware and at Texas Christian University and, since the fall of 1962, has been Associate Research Professor of Experimental-Physiological Psychology in the Department of Neurology and Psychiatry at the University of Texas Medical Branch in Galveston.

THE GOAL of the symposium which was the basis for this book was to present a broad overview of the most recent research involving abnormal 'mind-body' relationships. Toward this end the symposium was a success, going from molecules to madness via the biochemical, neurophysiological, and psychosocial routes. The theme of the symposium is not new, but this book, without becoming either mentalistic or dualistic, amply demonstrates the new look in unraveling the mind-body problem.

The broad range of problems and techniques reflects the "major trends in twentieth century psychiatric research" outlined by Malamud in his introductory remarks; e.g., the authors of the various papers indicate their appreciation: (1) for the principle of multiple causation; (2) for the dynamic changes that take place over time during the course of a particular disease; (3) for an interdisciplinary approach to problems of mental illness; and (4) for a monistic as opposed to a dualistic psychological model.

The range of problem areas includes: discussions of schizophrenia from the viewpoint of (1) biochemical mechanisms, (2) psychophysiological models, and (3) levels of consciousness related to peripheral microcirculation and the reticular activating system; a comparison

of cortical evoked potentials in normal controls vs. psychiatric patients; relationship of hormonal activity to everyday life stresses and anxiety; "cultural conflicts" related to tuberculosis; incidence of somatic disease in persons with adjustment problems vs. a normal control group; relationship of attitudes to psychosomatic illness.

In addition to the methodological discussions related to the research in the above problem areas, there are a number of papers oriented more specifically to experimental design and techniques and to a consideration of appropriate experimental controls. After reading the book, three associations come (freely) to mind—multiple causation, inter-disciplinary approach, and experimental control.

ALL of the writers exhibit an appreciation for experimental methodology and for the distinction between the controlled conditions of the laboratory and the less rigorous control possible in the field study or clinical setting. This is demonstrated very nicely in Handlon's chapter on hormonal activity related to stresses in everyday life situations where he describes studies of a "quasi-naturalistic" nature that are paralleled by well controlled laboratory studies.

Wenger's discussion of uncontrolled variables is especially significant for the beginner in psychophysiological research who often does not control (or at least doesn't report controlling) for menses in female subjects, for seasonal and diurnal variations, for food and alcohol consumption, etc., when taking ANS measurements. Not only because of Wenger's University of Chicago heritage from the days of Thurstone and Holzinger, but also because of his recent results in ANS research, it is natural that he should recommend the use of multivariate experimental analysis (especially factor analysis) in psychophysiological research.

Pollin's discussion of normal controls and Weiner's emphasis on controlling the "totality of the laboratory setting" during psychophysiological research are also valuable methodological considerations. Weiner notes that in contrasting the psychophysiological records of medical students, most of whom had

not volunteered for the experiment, with the records of clinical patients, most of whom had volunteered, the medical students' records were much more labile than the patients'. This observation adds validity to Pollin's that "the use of control volunteers sometimes goes astray and threaten(s) to introduce as much artifact into our findings as they do to bring about greater precision."

This book is recommended for those who want a quick, comprehensive view of current research on psychosomatic problems. The research designs can be generalized to many problems other than the ones reported in the book. There are bibliographies with each chapter for obtaining the details of design and theory which are not contained in the chapters. Possibly it is the lack of excessive detail in the text itself which makes the book easily readable.

## Separation Sickness

James Robertson (Ed.) Foreword by Sir Harry Platt

*Hospitals and Children: A Parent's-Eye View: A Review of Letters from Parents to the Observer and the BBC.* London: Victor Gollancz Ltd. 1962. Pp. 159. 18/-net.

Reviewed by ANN M. GARNER

The editor of this volume, James Robertson, is a psychoanalytically oriented Britisher who, since 1948, has been Senior Project Officer of the Tavistock Child Development Unit. The reviewer, Ann Garner, received an MA from Radcliffe College and a PhD from Stanford, then stayed on at Stanford for a few years to teach there. She is a clinical psychologist, a teacher, a wife, a mother, a researcher, a writer. Presently she is lecturing in psychology at the Graduate School of Social Work at the University of Nebraska.

"WHEN I brought the child home —she'd changed completely. First of all—she appeared to hate her



father—screamed when he came even near her . . . Six months later—she still rocks in bed sometimes for hours on end . . . Her temper is still violent, and she doesn't walk or speak" (p. 60).

"We all moved into a small ward in the children's wing . . . What would have been a terrifying experience for him turned out to be simply a tedious one. And of course there were no repercussions afterwards such as my friends have been through with their children" (p. 41).

These are British parents speaking, describing the effect of hospitalization upon their young children. They speak, as does this book as a whole, of a diversity of experience and attitude, ranging from deep satisfaction over 'living-in' and 'unrestricted visiting' procedures, to profound dissatisfaction with techniques of sudden separation and frightening isolation. They speak in the hope that their remarks may further accelerate the recent improvement in hospital facilities for children.

For over a decade, the Tavistock Child Development Research Unit, and James Robertson in particular, have been studying the effects of hospitalization upon children under the age of 3 or 4 years. Their findings agree with those which emerged from the UN and WHO investigations of parent-separated children, and with older studies of institutionalized children as well. The consequences of early separation from parent figures are now well documented; few would question the possibility of long-time emotional damage to children separated from their parents at an early age.

The present volume, however, while providing additional information concerning the emotionally damaging effects of separation and the ameliorative effects of parental living-in, fits rather a persuasive than an investigative role. The Platt report of 1959 encouraged British hospitals to provide unlimited visiting to all children in hospital, and to develop living-in arrangements for all mothers of preschool children who must be hospitalized. Although a number of British hospitals have acted upon these recommendations, resistance to change has been understandably great, and progress discouragingly slow. Con-

sequently, Robertson in 1961 invited parents to write letters about their children's hospital experiences. This book contains a sample of the 400 letters received, with interpretations by the editor.

No one, least of all the editor, would argue that such data prove anything about child behavior or hospital practice. On the other hand, some interesting consistencies run through the material. For example, the mothers recognize their own need to be with their child, as well as the child's need for the mother. ("Apart from the child, who is the prime consideration, the Mother too is better for being with the child . . ." p. 36.) Parents resist the temptation to criticize individual hospitals or staff members; they recognize that administrative change comes slowly, and that much special education and understanding on the part of the hospital staff are necessary. ("It needs only to believe that there should be no separation, and ways could easily be devised. At the moment I suspect that the ideal is not held, only tolerated under pressure . . ." p. 45.)

In general, parents feel that the more access permitted them to their hospitalized children, the greater their own satisfaction, and the greater also their children's ability to withstand long-time damage. Parents' persistence in trying to gain more access to their children is impressive, even reaching the extreme of removing their sick children from hospital rather than exposing them further to the danger of emotional disturbance. ("The hospital did a fine job of surgery, but the scar left by the sudden separation will take a long time to heal." p. 66.)

Separation anxiety knows no national boundaries, nor do the problems of accelerating social change. Here British parents write convincingly about anxiety, and a British investigator persuades to social change. Both aspects of this book will attract persons concerned with the welfare of young children of any nation.



*Man's greatest error is to believe in his own progress.*

—RENÉ SÉDILLOT

## For Future Freshmen

W. N. McBain and R. C. Johnson

*The Science of Ourselves: Adventures in Experimental Psychology.* New York: Harper & Row, 1962. Pp. ix + 217. \$3.50.

*Reviewed by* STANLEY C. RATNER

W. N. McBain and R. C. Johnson collaborated in writing the present book while they were both Assistant Professors at San Jose State College in California. McBain remains at San Jose but Johnson has moved to the University of Hawaii. The reviewer, Stanley Ratner, received his PhD at Indiana University and, except for a year as a National Science Foundation Fellow at Cambridge University, has been at Michigan State University since 1955. He is Chairman of the APA Committee for Secondary School Psychology and is actively involved in the National Science Foundation program for science teachers and for undergraduate students.

FEW psychologists have acknowledged the fact that secondary school students may be interested in psychology. Fewer still have acknowledged this fact with regard to experimental psychology. W. N. McBain and R. C. Johnson from the Department of Psychology of San Jose State College introduce the secondary school student to the study of the behavior of organisms. They do this by identifying some behavior processes and illustrating these with descriptions of demonstrations and experiments that expose these processes and some variables that affect each.

Due to the absence of a preface and the extreme brevity of the introductory chapter, the specific goals of the book are not clear. However, the authors state in the introductory chapter that they are concerned with telling: "young people who are in school . . . about why people and animals behave the way they do" and "why we (the authors) find the behavior of people and animals so interesting." It seems to the reviewer that these goals are particularly difficult to



meet and they are more or less disregarded in the remaining portions of the book. But the young people in school are the ones to decide about the inspirational qualities of the book and it has merits apart from the goals stated in the introduction.

The subtitle of the book, *Adventures in Experimental Psychology*, suggests its tone and emphasis. By means of untechnical and sometimes uncritical prose some behaviors that have received experimental attention are briefly discussed. Then demonstrations and experiments related to these behaviors are clearly described by means of uncluttered prose and simple line drawings. Many of the demonstrations are presented for replication by the interested student. For example, mapping of the taste receptors is described in detail, as is serial learning. The authors describe the most modest equipment and some relevant control procedures for those demonstrations that can be replicated.

Seven of the eleven chapters in the book contain brief descriptions of behavior processes and demonstrations and experiments. The other chapters deal with topics usually described as scientific method, and with personality and social psychology. An appendix presents six additional demonstrations that include independent variables, instructions to subjects and hints about the results of the demonstrations.

The treatment of the demonstrations in the appendix calls attention to some of the chronic problems within the book. No attention is paid to the possibility or meaning of negative results from these demonstrations and the young student is not encouraged to think beyond the demonstration. In addition other opportunities are missed to set attitudes about research. For example, the concept of control is mentioned as a general characteristic of experimentation but it is not related to the specific experiments and demonstrations.

The last chapter entitled, "If You Want to Read More" contains brief descriptions of 13 books of which Hebb's *A Textbook of Psychology* is the only general book, while the others are inspirational, scientist centered books such as Darwin's *The Voyage of the Beagle*. The authors fail to make use of

or acknowledge the books in general psychology that are already widely used in secondary schools. T. L. Engle's *Psychology*, 3rd Edition is an example. They also fail to relate the content of their book to the content of related areas such as biology that includes ma-

terial from experimental psychology.

The book clearly makes a contribution to the body of material for the secondary school student but it requires additional supporting material and these sources of support are not even suggested within the book.

## Old Bottles, New Wine

Leonard Berkowitz

*Aggression: A Social Psychological Analysis*. New York: McGraw-Hill, 1962. Pp. ix + 361. \$7.95.

Reviewed by ALBERT PEPITONE

*The author, Leonard Berkowitz, is Professor of Psychology at the University of Wisconsin. He did his graduate work at the University of Michigan and on the way from Ann Arbor to Madison he spent a period as research psychologist at the Crew Research Laboratory of the Air Forces Research Center. His bibliography includes many articles on aggression, catharsis and related topics. The reviewer, Albert Pepitone, also a Michigan PhD, worked for a while at Michigan's Institute for Social Research before going to the University of Pennsylvania where he is now Associate Professor. His research and theoretical interests lie generally in the area of cognitive processes underlying interpersonal attraction and hostility. He is now preparing a book in this area. He reports that he and Leonard Berkowitz must look a good deal alike for they are taken for one another at APA meetings. He wishes it to be known that he is fairly sure that he is not Leonard Berkowitz, or vice versa.*

retically overextended, but always a well argued and thoughtful treatment.

As a systematic frame of reference for organizing the vast material, Berkowitz adopts the well known 1939 Frustration-Aggression (FA) theory of the Yale group. More than this, while disclaiming total commitment to the theory and contributing important modifications of his own, he accepts the essential validity of the Yale formulation, and believes it to be "a milestone in the application of methods of experimental psychology to important social problems." It seems appropriate, therefore, to ask how good the FA hypothesis is after a quarter century, and what improvement does Berkowitz make.

From the beginning there has been difficulty in specifying the aggression to which frustration gives rise and to distinguish it from other aggressions. Part of the problem arises from the original definition of aggression as "any sequence of behavior the goal response to which is the injury of the person toward whom it is directed." Of course, it never was insisted that aggression only involves physical injury, but there are problems in applying the tissue damage paradigm to other domains—a "damaged" ego, for example. Physical or not, aggressive behavior does not always have an injurious effect, whether judged from the aggressor's or victim's

A COMPREHENSIVE theoretical and experimental analysis of aggression, hostility, and kindred phenomena has long been needed in social and personality psychology. In a tightly packed and highly readable volume, Berkowitz attempts to do such a job. The result is an often penetrating, sometimes theo-



point of view. Quite often, aggression is not even intended to be injurious; it is employed as a threat with which to influence another's behavior. Indeed, whether feigned or meant to be injurious, aggression can actually have a beneficial effect on the victim. On the other hand, behavior which is apparently noninjurious may be considered aggressive through having seriously damaging effects in the long run or on another level of analysis. Finally, the injuriousness of behavior may be incidental or secondary to its aggressiveness in the sense of persistence, vigorousness, directedness, etc. What is injury, after all, and how do we determine whether one act is more injurious than another? Is a punch in the nose more aggressive than calling one's rival a bastard?

Even more difficult perhaps is the specification of aggressions which are goal responses, aggressions motivated by a specific instigation to aggression. Clearly, some aggression is partly or wholly accidental, or is a side effect of a larger parcel of behavior. More often, motives other than aggression-instigation are involved, or the instigation to aggression is absent altogether.

Berkowitz clarifies the conceptual muddle by making explicit what had not clearly been understood in the original FA formulation: The frustration hypothesis is only concerned with the instigation to aggression and with aggressive goal responses. The frustration hypothesis does not handle instrumental aggression—aggression produced by nonaggressive motivations. Berkowitz also excludes aggression which is accidental and aggression which refers to assertiveness and the like. Disappointingly, however, his analysis does not help much on the problem of what is injury, and this is of crucial importance in deciding when aggression is a goal response, and hence explainable by frustration.

The specification of the goal response, of course, is also pertinent to the issue of catharsis—the reduction in the instigation to aggression following aggressive responses. For example, if injury has actually to be perceived by the victim, not all aggressive responses can be said to reach the goal. Conse-

quently, aggression need not result in reduced instigation. Even if the goal response is made, the reduction in instigation is only temporary, since, theoretically, the frustration is unaffected by the aggression. Such uncertainty regarding the exact occasion of the goal response as well as the time period during which it is effective make the prediction of catharsis difficult for the original FA theory.

**B**ERKOWITZ'S analysis of the alternatives working against the catharsis interpretation of reduced instigation is masterful and probably the best yet published. He rightly insists, for example, that since aggression itself is frustratable, no catharsis hypothesis is necessary to account for the lesser aggressiveness under conditions where its expression is permitted than where it is prevented; the occurrence of aggression may arouse guilt which can curtail subsequent aggression; reduced instigation may be due to a change in the social environment toward greater permissiveness; aggressiveness can be reduced if it happens to remove the frustration or if, as instrumental behavior, it satisfies the motive which produced it. Finally, Berkowitz proposed an interesting cognitive process whereby aggression becomes a stimulator of further aggression by way of reminding the person of the existence or enormity of the frustration.

From a non FA theoretical orientation, of course, a reduction in aggression-instigation may come about by way of diffuse emotional or tensional discharge; the goal response of injurious aggression is not necessary for catharsis. Presumably, to take account of this possibility, Berkowitz—as other theorists have—proposes that *anger* be considered an intervening emotional state with the same motivating properties as the instigation of aggression. In so far as the proposal invites attention to defense mechanisms and psychosomatic aspects of anger, such a proposal may be more than a mere terminological substitution.

Any analysis of aggression must concern itself with the effects of inhibition, particularly on the selection and substitute value of targets other than the

instigator. Theoretically, displacement of aggression depends upon the specification of the goal response. If the goal of aggression is injury *to the instigator*, then no one else can be an effective substitute target. And if injury is part of the specification, then a harmless attack on anyone cannot be of substitute value. Even though displacement may be ineffective, however, it does appear to take place, and Berkowitz adopts Miller's familiar S-R generalization model as his main analytic tool to explain it. He has great respect for the model, but must deal with some very serious inadequacies for explaining target selection under any but the most highly restricted circumstances. For instance, the model does not specify along which of all possible similarity dimensions the individual will displace. Secondly, there is little rational basis for specifying the slopes of the aggressive and inhibitory response tendencies along any given dimension of similarity. Finally, the generalization analog of displacement presupposes that the organism has learned or perceives the source of frustration. Clearly, one must know from which instigator, e.g., self or other person, the similarity declension originates. It would seem that this process of instigator-attribution is more fundamental than deflection once the original target has been established. Berkowitz' emphasis on the self as a source of frustration is a fruitful beginning of such an analysis.

**T**HE QUESTION of what *is* frustration has also been a problem for the FA theory from the outset. As an "interference with the occurrence of a goal response," its conceptual definition requires the identification of a goal response and a condition which prevents the response from occurring. It seems fair to say, however, that over the years this definition has been very broadly interpreted and has led to extremely loose empirical coordinations. Berkowitz, unfortunately, does not reverse this trend. Having one's toes stepped on is a frustration of internal responses toward security and comfort. Being insulted frustrates self-esteem maintenance. Threat to self-esteem, however, is more than a blocking; it represents



degradation and loss of power. Berkowitz handles this type of criticism by assuming there is a continuum of strength of instigation with deprivation on the low end and threat to self-esteem on the high. Of course, whether frustration is a unitary concept with respect to aggression is fundamentally an empirical question. But if frustration covers all aggression-producing conditions by definition, there is no room for empirically based distinctions and no motivation to create them. Berkowitz does propose distinctions based on the individual's *interpretation* of the frustration. Thus, fear rather than anger results to the extent that the frustration is perceived to have noxious consequences which cannot be avoided. If, however, there are two empirically distinguishable situations here, one wonders what theoretical power is gained by calling both frustrations.

Although cognitive factors in frustration are explicitly acknowledged, marking a needed departure from the strictly behavioristic Yale conception, Berkowitz' treatment is more in the nature of a graft onto the FA body. For example, he is critical of Pastore's well known demonstration that arbitrary frustration evokes more aggression than a nonarbitrary one. In the first place, arbitrariness may have affected the inhibition rather than the instigation to aggression. More important, arbitrary frustration may be regarded as an *unexpected* frustration. The greater aggression, then, is based upon the frustration of expectation over and above the nonarbitrary frustration.

Indeed, Berkowitz shows experimentally that interpersonal evaluations are affected by the contrast between expectation and subsequent information. Thus, if an S expects friendly behavior from a partner, his receipt of unfriendly behavior causes him to devalue the partner more than if the unfriendly behavior is expected.

An expectation which does not come off may be interpreted as a frustration just as, when anticipated, an interference may theoretically have weaker effects. However, the frustration-linked analysis of cognitions poses some difficulties. Arbitrary events are not always unexpected. Arbitrary events in the

sense of being unjustified appear to be able to create anger in the individual without palpable frustration. Then too, the demonstration that unexpected friendliness leads to more positive attitudes toward another person shows that the opposite of aggression can occur when expectations are "frustrated."

This is a book which must be read by any psychologist concerned with the theoretical issues briefly touched on above. In addition, Berkowitz has a lot of important things to say about the interpretation of real life "aggressions."

## Impractical Absence of Theory?

Alfred J. Marrow

*Changing Patterns of Prejudice: A New Look at Today's Racial, Religious, and Cultural Tensions.* Philadelphia: Chilton Books, 1962. Pp. ix + 271. \$6.95.

Reviewed by NATHAN KOGAN

*Alfred J. Marrow is an author, lecturer, and industrialist. Once a student of Kurt Lewin, he is now Chairman of the Board of the Harwood Manufacturing Corporation. The reviewer, Nathan Kogan, worked with Gordon Allport and Jerome Bruner at Harvard where he first became interested in social attitudes and problems of ethnocentrism. After obtaining his PhD at Harvard, he spent several additional years there as Research Associate pursuing research on psychological aspects of aging before moving, in 1959, to the staff of the Educational Testing Service where he is presently Research Psychologist in the Personality Research Group and where he is focusing at the moment on the individual and group determinants of risk-taking behavior.*

IN 1955, New York City founded a Commission on Intergroup Relations (COIR) to investigate racial, religious, and ethnic conflicts among its inhabit-

ants, and to take the necessary steps toward resolution of such conflicts. As Chairman of COIR during the period 1955-1960, Dr. Marrow assumed the major burden of carrying out the purposes for which COIR was established. Brought to COIR's attention was an array of problems (e.g., discrimination in eating places, private clubs, housing, and schools) that represent in microcosm those that confront the country at large.

The reviewer must confess to a severe ambivalence in his evaluation of the present volume. In the role of private citizen, he admires the skill and energy that Dr. Marrow brought to the task of reconciling conflicting interests and bringing intergroup harmony to a large metropolis. As an autobiographical account of a social scientist donning the mantle of crusading reporter and social engineer, the book would have to be judged a considerable success. As a contribution to the scientific study of prejudice and discrimination on the other hand, this reviewer, in the role of psychologist, must render a negative judgment. While the author apparently had both the lay public and psychologists in mind as an appropriate audience for his efforts, it is obviously the book's value for the latter audience that is of concern here.

The general style of the volume is a constant irritant. The book gives the impression of being a lengthy free association. Description, interpretation and prescription follow one another in helter-skelter fashion. Chapters begin on a particular theme and drift off in various directions at the author's whim. Redundancy and mutually contradictory statements abound. The expressive language gives the book the quality of a tract. Perhaps, these lapses can be excused for reasons of the author's moral indignation, but this then raises the issue of whether it is not incumbent upon the social scientist *qua* scientist to deal with socially sensitive issues in a spirit of dispassionate inquiry rather than emotional fervor.

Also disturbing is the book's lack of documentation. The work of others is often quoted or paraphrased, yet the author does not provide a list of references. Hence, occasional statements that



this reviewer considered dubious could not be traced to their sources.

THE AUTHOR'S theory of prejudice outlined early in the volume puts considerable emphasis upon personality determinants, particularly authoritarianism and childhood deprivation. How surprising, then, to read on and find that the personality approach to prejudice is largely inappropriate for the kinds of problems Marrow confronted as Chairman of COIR. Instead, we note that the social psychological and sociological aspects assume major importance (e.g., the headwaiter concerned with the restaurant's White patrons, the codes of violence of rival gangs, the role of group support in desecration incidents). Each problem seems to find the author in search of the 'bigot,' who ultimately vanishes in a cloud of *ad hoc* interpretation.

While the book adds nothing that is fundamentally new to our knowledge about the psychology of prejudice and means for its alleviation, it is to the author's credit that he recognizes and publicizes some of the unfortunate by-products of a philosophy that avoids offending anyone and insists that minority group characteristics are irrelevant to the prejudice issue. The book achieves a reasonable balance as a consequence.

Stressing the need for research, Dr. Marrow makes a special plea for Federal research assistance and supports the establishment of a Federal Bureau of Intergroup Relations in HEW. Given the political complexities that surround such a move, one can only express regret that the author did not or could not divert a small portion of his budget to theoretically oriented research, particularly when, by his own admission, more funds were available than could be fruitfully spent. Possibly, Dr. Marrow's decisions in this regard are in keeping with his statement of the principle that "...action itself will lead to more reliable knowledge. So action becomes research, and research action." This is indeed a far cry from Kurt Lewin's dictum: "There is nothing so practical as a good theory."



## The Liberals in Psychoanalysis

Jules H. Masserman (Ed.)

*Science and Psychoanalysis. Vol. V: Psychoanalytic Education.* New York: Grune & Stratton, 1962. Pp. v + 332. \$9.75.

Reviewed by PAUL BERGMAN

*The editor of the present volume, Jules H. Masserman, by now needs little or no introduction to any reader of Contemporary Psychology who pays more than glancing attention to psychoanalytic and psychiatric literature. The reviewer, Paul Bergman, is a research psychologist (clinical) at the National Institute of Mental Health in Bethesda, Md. His psychoanalytic orientation has both Austrian and American roots. He took his first analytic training in Vienna but rounded it off at Topeka, Kansas; he received a PhD degree from the University of Vienna but did formal graduate work at the University of Indiana. Before coming to his present position at NIMH, he served for ten years as senior psychologist at the Menninger Foundation and then worked for three years at Seattle's Pinel Foundation. While he retains his interest in psychoanalytic ideas, his acquaintances observe that he is by no means the "typical analyst," if there is any such thing, and it is obvious to psychologists that his interests range wide. For further evidence of both his willingness and his skill as a CP reviewer, see page 310 of this issue.*

BEFORE 1930 "psychoanalytic education" meant the application of Freudian psychology to the education of children. The proponents of this approach hoped to find a way to end neuroses and other perennial evils of mankind. In subsequent years the meaning of the term has become limited to the training of psychiatrists in psychoanalytic theories and techniques.

Two books about this latter kind of psychoanalytic education are now avail-

able: The survey sponsored by the American Psychoanalytic Association (Lewin and Ross, *Psychoanalytic Education in the United States, CP*, Aug. 1961, 6, 265-266) which ended on a laudatory note, and the here reviewed collection of papers which expresses restrained disapproval. In opposition to the prevailing "conservative" spirit in the Association, the authors of the present collection represent what one might call "liberal" Psychoanalysis.

The "liberal" point of view in psychoanalytic education implies, first, a somewhat critical attitude toward Freud. In the book this attitude is expressed in various degrees, at times intensely and broadly in words like these "...if Freud and others through the years are writers of the absolute truth, then psychoanalysis is not a science" (p. 4, Grinker). More frequently, specific Freudian positions or traditions become the target of attack (Silverberg, Shainess, Rado). At the other end of the continuum Bry and Rifkin see their task as the harmonization of certain minor factual errors of the master with his basic infallibility. To appreciate fully the predicament to which the earnest pursuit of such a task leads, one must read it in the authors' own words: "At first glance certain information derived from outside sources tends to be in conflict with the psychoanalytic tradition derived from Freud's writing... there have been three chief modes of coping with this conflict, none of which has been satisfactory to meet educational standards: to defer the whole matter to a later period, to interpret the collateral information in such a way that the conflict seems to have been removed, or to declare that Freud com-



mitted an error" (pp 6-7). Incidentally, the remedy which the authors suggest is deeper study of the history of ideas.

Next, the liberals characteristically want to relieve psychoanalysis from its present intellectual isolation in relation to the other sciences. They hope to do this by enriching the curriculum of psychoanalytic education with related subject matter and patterns of thinking. The book contains pleas for biology (Hamburg); social sciences (Spiegel); psychosomatics (Wittkower and Aufreiter); communication processes (Jackson); flexible therapeutic methods (Alexander); ward psychiatry (Meyer); identity problems (Chodoff); value problems (Reid, also Eckardt); research (Shakow); alternative theoretical models (Shands). The writers present their cases capably, informatively and even convincingly insofar as one cannot reasonably doubt the relevance of these various concerns to the subject matter of psychoanalysis. None of the writers, however, shows how all this material could be integrated into a professional training without intolerably overloading it. After all, it is a question of educating practitioners, not scientists, in the field of therapy. Should not the design for the education of practitioners be based on an examination of the process of psychotherapy, rather than on a study of the interrelationship of the sciences?

For a few authors, "liberalism" finally means a concern for individual human beings and human life in general. They find such an attitude not sufficiently represented in psychoanalytic education today (Will; possibly also Searles).

IF ONE accepts the way the "liberals" define themselves—as a group holding anti-authoritarian, anti-isolationist and humanist views—he wonders how anybody could possibly criticize their position. Yet there are at least two perspectives, the "conservative" and the "radical," from which the "liberal" position may be viewed critically. A "conservative" might argue like this: "What the 'liberals' have in common is a retreat from the Freudian unconscious. The dissident schools of the past have failed; now the resistance, the re-

turn of the repression, takes the form of an emphasis on other sciences, on psychotherapy, and on humanism. These in themselves are good things, but should not be used to replace psychoanalysis. Psychoanalytic theory has its own tasks and problems with which the 'liberals' frequently show insufficient acquaintance. They particularly neglect the achievements of ego psychology. However, one who rejects Freudian theory has the obligation to set his own observations into the context of an organized theoretical system. This the 'liberals' fail to do. Psychoanalytic education, if it fell into their hands, would soon lose its identity and become dissolved in a vague eclecticism."

As for myself, believing that a "radical" perspective is essential to the spirit of science, I share the "conservatives'" dissatisfaction with the "liberal" eclecticism. However, instead of regretting the drift away from the orderly position of the "conservatives," I regret the "liberals'" half-heartedness in relinquishing the traditional outlook of their field. I do not give as much weight to the disagreements between the two groups as they do themselves. Both groups hold in common basic assumptions and attitudes which I consider mistaken. For example, both groups consider insight the major means of achieving personality change. Both groups, with rare exceptions (Marmor and, of course, Shakow in this volume) think and work in complete dissociation from contemporary theory and research in psychology and psychotherapy. Consequently, their intuition often excitedly rediscovers relationships which research has already confirmed, or worse luck, refuted. A heavy welcome to intuition, but on the frontiers of knowledge, not in well-established territory! Welcome also to the courage to challenge accepted knowledge, but it can hardly be meritorious not even to know that such knowledge exists.

SURELY, those were days of giants when Freud and others with bare hands erected their house of thought. It is the house which the "conservatives" hope to inhabit forever, doing minor repair jobs occasionally. It is the house which the

"liberals" would like to incorporate into a larger, modernized structure without upsetting the historic continuity. The "liberal" attitude fits the requirements of art or politics, but the spirit of science seems to favor the freest re-ordering of the facts. Today there exist various theoretical models which allow for both the preservation and economical reorganization of the rich discoveries of psychoanalysis. But the "liberals," because of their need to synthesize a commitment to science with a loyalty to a particular tradition, have not shown much awareness of such alternatives. They have also been slow to perceive that objective research can and should decide how much respect the traditions merit.

I am not sure how much influence the "liberals" will exert on "psychoanalytic education" in the narrow sense in which the term is used at present. The outcome of their drive for reform does not seem very important. In the meantime, the problem of "psychoanalytic education" in the original sense of the term, how to educate children without subjecting them to crippling dissociations, lies unattended and nearly forgotten. For an attack on that problem, the tools, the methods and the uncompromising attitude of science will be needed. This, though, is not the typical equipment of the "liberals."

## First Freudians

Herman Nunberg and Ernst Federn  
(Eds.) Translated by M. Nunberg

*Minutes of the Vienna Psychoanalytic Society, Vol. 1: 1906-1908.*  
New York: International Universities Press, 1962. Pp. v + 410.  
\$10.00.

Reviewed by PAUL BERGMAN

Herman Nunberg, one editor of the present volume, is a psychoanalyst who was trained in Vienna but who, for 25 years or so, has been writing and practicing from a base in New York City.



*Ernst Federn, the other editor, is the son of Paul Federn who for many years was President of the Vienna Psychoanalytic Society. The reviewer, Paul Bergman, is a research oriented clinical psychologist who is described, with the customary inadequacy, on page 309 of the present issue.*

IN 1902 a small group of followers had begun to gather on Wednesday evenings in Freud's private apartment to learn, develop and propagate the "professor's" psychoanalysis. From 1906 on, Otto Rank assiduously recorded and summarized the discussions. His notes were preserved and now reach publication with a first volume containing the minutes of 1906-1908.

We might call these years the Golden Age of psychoanalysis. Freud had emerged from his earlier isolation, the world had begun to take notice of him, and valuable contributions from the men around him enriched his inspiration. His circle consisted of physicians, writers and educators who represented a cross-section of Vienna's intellectuals. The common enthusiasm was high and nobody yet foresaw that painful secessions would upset the "movement." Alfred Adler, two years later the leader of the first secession, in the present volume still appears as a brilliant, though increasingly independent, contributor to the common thought. Yet even in the discussions of that time one detects the seeds of future discord. For when these men disagreed, their ignorance of criteria by which to resolve differences frequently led them to resort to passionate reiteration, personal attacks, and accusations of "resistance" based on sexual deviancy.

The circle's discussions ranged widely, though centering on two focal areas: 1) sexual pathology and 2) literary arts—in their relation to sexual pathology. And it was disagreement about the role of sex in the human personality which eventually divided the group. Yet, seen from an historical perspective, it now appears to have been fortunate that Freud chose sex as the basis for his system. While either aggression or anxiety would have produced an equally fruitful theory, the choice of sex had a stunning effect upon a late-Victorian world

grown tired of its hypocrisies. That world began to listen. Even today, as one reads the minutes, the sexual interpretation of all things in heaven and earth, in spite of its one-sidedness, retains interest, particularly when applied to clinical case histories.

The interpretations also had some validity, both as diagnostic formulations and as tools to achieve symptomatic improvement. Diagnostically, they frequently pointed to real foci of conflict. Therapeutically, in the cultural milieu of the period, the newness and crassness of the interpretations frequently shocked patients out of their symptoms. But since such effects did not always come about, Freud devised new, slower working techniques which he thought to be capable, in principle, of dissolving the neuroses. What did these techniques, "analysis of the resistances" and "working through," achieve? The discussions do not describe the process of change in patients, nor do they suggest any problems encountered. The unconscious is dealt with in a dim, abstract, and distant manner. For example, one of the discussants (Hitschmann) suggests that "one should consider the problem whether there is a connection between the oral center and sadism" (p. 370). I believe that one who speaks in such terms has never observed a patient re-experiencing the torturing force of oral sadism or any other infantile trend dissociated by trauma. Apparently psychoanalysts at that time (let us not mention today) talked to their patients about dynamic factors, but did not make actual contact with the dissociated emotions, at least not in severe neuroses. They were like geologists who interpret the presence of oil in the earth, and not like drillers who make contact with the gushing stream and relieve the pressure in the depth.

WHATEVER one may think of Freud's therapy, his personality and his thought remain objects of fascination. The present volume shows the "professor" presiding in Olympian benevolence, often protecting speakers from too violent attacks, and distinctly favoring those who contributed creatively to the ideas of the circle. He spoke as he wrote, freely

and energetically, in sustained long trains of thought. At times he would eject aphorisms expressing his characteristic powerful disillusionment, calling love "the normal psychosis" (p. 193) and happiness "the uninhibited gratification of the perversion" (p. 208).

Freud's social evaluation of sexuality retains its previous puzzling ambiguity. At one point he says, for instance, "we endeavor to uncover sexuality; but once sexuality is demonstrated, we demand that the entire repression of sexuality become conscious and that the individual learn to subordinate to cultural requirements . . . a woman who, like the courtesan, is untrustworthy in sexuality, is altogether worthless" (p. 200). Yet, at other points he says that "charming and attractive women acquire and sustain these qualities only in sexual freedom" (p. 310), suggests "the founding of an academy of love where the *ars amandi* would be taught" (p. 311) and wants "a social reform allowing a certain amount of sexual freedom" (p. 273). He never specifies, however, what kind of social reform he advocates.

Whether or not Freud and his followers at times succumbed to self-contradiction, bias and passion, these pages do preserve some great moments in the history of man's spirit. Some of the gains made during those Wednesday evenings will, I trust, never again be lost. The editors deserve thanks for publishing these minutes and for conscientiously clarifying in footnotes a great many allusions to persons and events which otherwise would remain meaningless. Unfortunately they have also added footnotes to expound their own opinions about many of the subjects under discussion, and to stand up for Freud against any doubting or dissenting voice. Future volumes of these minutes would gain considerably if the editors would proceed on the assumption that Freud can speak and readers can think for themselves.



*The moulding of man today is a very untidy business and it's hard to find a simple or elegant pattern in it!*

—TOM H. PEAR





# Judgment Too Long Deferred?

Sidney J. Parnes and Harold F. Harding (Eds.)

*A Source Book for Creative Thinking*. New York: Scribner's 1962. Pp. v + 393. \$6.50 (hardback) \$4.75 (paperback).

Reviewed by JOHN E. COULSON

Sidney J. Parnes, the senior editor here, is Director of Creative Education at the University of Buffalo and serves as a Consultant to the Creative Education Foundation. He has taught courses in creative problem-solving, has written an instructor's manual and a student workbook for courses on creativity. His newest collaborator, Harold F. Harding, is Professor of Speech at the Ohio State University. John E. Coulson, the reviewer, is a Columbia PhD in psychology and is now Associate Head of Educational Research and Development for the System Development Corporation in Santa Monica, California. He helped initiate the SDC Automated Teaching Project in 1958, and has published extensively in the area of instructional methodology. His present research centers on methods of developing problem-solving skills through flexible instructional systems. In a related area, he has recently edited the ONR-SDC symposium volume *Programmed Learning and Computer-Based Instruction* (1962) (CP, May 1963, 8, 216-218.)

IN one chapter of this book Alex Osborn notes that the Principle of Deferred Judgment (a major tenet of Osborn's well-known "brainstorming" technique) calls for "deferment-of-judgment during ideative effort to keep the critical faculty from jamming the creative faculty." Critical judgment, according to this approach, is an important but later process. Dr. Sidney Parnes and Dr. Harold Harding, the editors of *A Source Book for Creative Thinking*, seem to have deferred their judgment

too long in the compilation and editing of the materials. They have shown originality in selecting a wide variety of papers containing valuable information and perceptive insights on creativity, but they have not accepted fully the critical responsibilities that properly belong to the editors of such a volume. The reader has been left with a formidable task of separating the valuable from the redundant and the trivial.

According to Drs. Parnes and Harding, this book is intended to serve as a reference source, a supplementary textbook, and a means for further reading and research on creativity. An associated goal is "to increase interest within colleges and universities in developing this greatest natural resource." These are broad goals, and quite appropriately the editors have assembled a series of papers dealing with the subject of creativity from almost every imaginable viewpoint. Experimental and statistical problems in the definition, measurement and manipulation of creative skills are competently probed by research psychologists and educators such as E. Paul Torrance, Richard Youtz and J. P. Guilford. Carl Rogers and A. H. Maslow offer highly intriguing, if occasionally obscure, discussions of creativity in relation to personality dynamics.

As might be expected, Rogers and Maslow differ considerably in their points of emphasis from sociologist Melvin Tumin, who adopts a sociological framework in describing various social obstacles to creativity. Interestingly, though, Tumin agrees with Rogers in minimizing the importance of social reinforcement for the creative

act. Rogers states that "perhaps the most fundamental condition of creativity is that the source or locus of evaluative judgment is internal. The value of his product is, for the creative person, established not by the praise or criticism of others, but by himself." Tumin echoes this thought in defining creativity as "the esthetic experience, which is to be distinguished from other experiences by the fact that it is self-consumatory in nature. That is to say, the esthetic experience is enjoyed for the actions which define and constitute the experience, whatever it may be, rather than for its instrumental results or social accompaniments in the form of social relations with others."

Since Rogers and Tumin do discuss interpersonal and social influences on creativity in other parts of their papers, it is difficult to decide whether these authors actually mean to deny the vital role of conditioning history and external reinforcement in the acquisition of creative behavior patterns, or whether they simply mean to emphasize that the individual's internalized responses to his own creative acts can acquire secondary reinforcing properties. The first position would seem difficult to defend, since surely the behavior that we label creative is subject to the same basic principles of reinforcement as are all other forms of behavior. But, even if we accept the second interpretation (of internalized secondary reinforcement), this still does not support the unique experiential status ascribed to creativity by Tumin. The reinforcement for a large part of our adult social behavior, including much that we would not call creative, is in the form of internalized responses rather than direct external reinforcement. In any case, it seems much more useful to recognize the ultimate dependence of the creative act on external reinforcement, since such a viewpoint points more directly to the variables of social interaction that must be manipulated if creativity is to be fostered in our society.

IN *A Source Book for Creative Thinking*, strong emphasis is placed on methods of effecting practical improvements in the creative ability of people at all age levels, in the schools and in indus-



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By JAMES A. DYAL, *Texas Christian University*. 457 pages, \$4.95 Cloth, \$3.95 Soft Cover.

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try. Chapters by such practitioners as Alex Osborn and John Arnold call attention to the significant work that can be done today to promote creative thinking, even before we have full understanding of the basic creative processes. One useful technique involves the use of a matrix of ideas or concepts. Such a matrix may provide systematic stimulation for new associations of old elements—a key component in creative activity. Another technique involves the restatement of a problem in terms of its most generic characteristics, thereby making it easier to devise radically new solutions that escape the conventional ruts.

Appendices to the book include a compendium of research studies on creative imagination and a list of selected publications on creative thinking.

*A Source Book for Creative Thinking* includes much that is informative and stimulating. Unfortunately, it also includes much the editors should have deleted. Several of the chapters, including the lead chapter, by editor Harding, present few new ideas but instead are devoted largely to restatements of ideas expressed by other educators and psychologists. Nor does Harding's chapter adequately serve the function of a preview or keynote for subsequent chapters.

Many of the chapters in this book were published without modification from speeches presented at conferences and symposia. This practice makes trouble for the reader, who must sort through the resulting jumble. There are protracted introductory remarks by speakers who are surprised and honored to have the opportunity to address some august body. There are glowing references to subsequent speakers whose work does not even appear in this book. There are complex sequences of questions and answers, reproduced verbatim, involving misunderstandings between the speaker and the questioners and baffling discussions of topics having only the most tenuous relationship to the specific subject of the paper. These unnecessary and confusing sections should have been omitted, or at least modified to form a more integrated part of the papers.

Despite these drawbacks, and because no other book presently available serves

precisely the same reference function, *A Source Book for Creative Thinking* is recommended to anyone interested in the area of creativity and problem solving.

## Philosophical Educational Psychology

Morris L. Bigge and Maurice P. Hunt

*Psychological Foundations of Education: An Introduction to Human Development and Learning.* New York: Harper, 1962. Pp. v + 530. \$7.50.

Reviewed by DAVID P. AUSUBEL

Both authors, Morris Bigge and Maurice P. Hunt, are Professors of Education at Fresno State College, California. Bigge did his graduate work at the University of Kansas, Hunt at Ohio State University. David Ausubel, the reviewer, is now a Professor of Educational Psychology in the Bureau of Educational Research at the University of Illinois. He started his career as a physician and practiced as a psychiatrist until, gradually, problems of individual diagnosis and therapy became less interesting than general problems of psychological development. His present research now centers in the area of verbal learning and there too lies his general theoretical interest. Due to appear in 1963 is a book entitled *Meaningful Verbal Learning* (Grune and Stratton). His earlier books include *The Fern and the Tiki* (1960), *Maori Youth* (1961), *Ego Development and the Personality Disorders* (1952), *Theory and Problems of Adolescent Development* (1954) and *Drug Addiction* (1958).

THIS BOOK has the somewhat unique distinction of being the first textbook in educational psychology written from the standpoint of an educational theorist or philosopher. It seeks to consider, on

the one hand, all important psychological issues that have general significance for educational theory, and, on the other, to provide basic ideas about human learning and development suitable for an introductory course in educational psychology. Nobody, however, could really be expected to achieve both of these mutually incompatible purposes within the covers of the same volume. By choosing to focus on general learning theory and on such highly general theoretical issues as the mind-body problem and absolutism versus relativism, the authors have produced, in the reviewer's opinion, a reasonably satisfactory "psychological foundations of education," but a very inadequate "textbook in educational psychology."

As a psychological foundation of educational theory, this treatise fulfills two important and hitherto unmet needs in educational psychology. First, it pulls together a wide array of significant psychological theory that has general relevance for education. Second, it integrates for the first time the fields of educational psychology and educational philosophy by delineating the historical origins and underlying philosophical assumptions of various psychological theories in education. Educational psychologists thus have an opportunity to become acquainted with those ideas of Rousseau, Froebel, Pestalozzi, Herbart, Dewey, Kilpatrick, and Morrison that have significant psychological implications. They are also familiarized with the underlying philosophical premises of various contemporary psychological systems that have no explicit identification with philosophy.

A noteworthy virtue of this book is that it eschews the currently prevalent "popular magazine" treatment of educational psychology with its oversimplification of complex data and ideas, glossy, irrelevant pictures, pseudo-scientific tables and figures, sentimental case histories, and kindergarten-level writing. The level of discourse here is reasonably high throughout. Bigge and Hunt are never guilty of talking down to the reader.

The most inadequate portion of the book is the section dealing with human development, which is presented at a very superficial and platitudinous level.



The authors include much descriptive material (e.g., physical growth) which has little relevance for educational psychology, and treat other pertinent aspects of social, personality, emotional, and cognitive development as ends in themselves, failing to relate them to the nature and conditions of school learning. Moreover, the more theoretical material in this area (e.g., the nature-nurture problem, human nature) lacks cogency and incisiveness. The discussion of developmental regulation, for example, fails to distinguish between intercultural uniformities, intercultural differences, and intracultural differences; between the respective notions of wholly preformed or predetermined traits and genic predispositions toward developing particular traits and capacities; and between genic and environmental sources of developmental uniformity. Thus, although the authors profess an interactional position regarding the nature-nurture problem, their failure to appreciate the hereditary aspect of this position disposes them to repudiate interactionism and to adopt extreme environmentalistic views with respect to personality development and the modification of intelligence.

The five chapters devoted to learning theory constitute by far the most valuable section of the book. The authors consider the historical origins, philosophical assumptions, and educational implications of five psychological approaches to learning theory, namely, mental discipline, natural enfoldment, apperception, S-R associationism, and cognitive-field theory. Particularly useful is their extremely lucid presentation of Skinner's position and of cognitive field theory. Unfortunately, however, these two positions are neither explicitly compared to each other nor related to important theoretical issues in classroom learning. Moreover, both these latter issues as well as the experimental evidence bearing on school learning receive only cursory coverage, usually from secondary sources. In place of this essential material, the authors devote the entire final section of the volume to problems that are more properly considered in textbooks on teaching methods.

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# The More Intense Phenomenology

Louis Jolyon West (Ed.)

*Hallucinations*. New York: Grune & Stratton, 1962. Pp. iii + 295. \$9.75.

Reviewed by REX M. COLLIER

The editor, Louis Jolyon West, is Professor of Psychiatry and Head of the Department of Psychiatry at the University of Oklahoma Medical Center. An active researcher and prolific writer, he also has played a leading role in introducing a program of behavioral science at the University of Oklahoma. Rex M. Collier, the reviewer, started out as a physiological and experimental psychologist but soon after receiving his PhD in 1934 from Northwestern he began to develop his interest in the clinical area. After some years of teaching at a number of different institutions of higher learning, he joined the Veterans Administration psychology program and, since 1953, has been Chief of Psychology Services at the VA Hospital, Jefferson Barracks, Missouri. He continues teaching, however, holding a part time appointment at Washington University. He also stays involved in research and scholarly activity and is presently working on the development of a comprehensive theory of conscious function, a matter with which he will deal in a forthcoming book.

THIS VOLUME is an edited symposium composed of twenty-six articles from forty-two contributors. That the symposium was a comprehensive cross disciplinary venture is revealed, in part, by the type of educational degrees held by the contributors. Represented are twenty-eight MD degrees, thirteen PhD's and one BA. The twenty-six chapters include a broad range of topics and subject matter, and the following tabulation is offered as a brief summary; theory (two articles), sensory deprivation and sleep loss (eight articles), pharmacological studies (three articles),

neurological and neurophysiological (two studies), somatic and body image issues (two studies), phenomenology (two articles), schizophrenic sources (three articles), occurrence in children (one study), social factors (one study), and hypnotically induced hallucinations (one study).

The book, dedicated to Heinrich Klüver "whose many contributions to research on hallucinations have been an inspiration to all of us," grows out of a behavioral science climate. Even so, it is by no means limited to traditional S-R and input-output orientations. Editor and authors should be commended for making no apology for a frank study of a segment of conscious phenomena. Implicit in the symposium is the principle suggested elsewhere by Jasper and Penfield that consciousness represents the highest levels of integration.

As might be expected, the issue receiving most attention was the relationship of sensory deprivation to hallucinations. Among the types of S. D. used were the following: isolation in light and sound insulated rooms, immersion in a tank of tepid water, blindfolds, translucent goggles, cardboard cuffs to reduce tactile stimulation and instructions to be as immobile as possible for the reduction of kinesthetic stimulation. Results do not give direct correlations between S. D. and frequency of hallucinations. In the light of earlier studies, this result came as a surprise to some researchers. Instruction, expectation, personality pattern, body image, and degree of ego integration were noted as influential variables.

MOST of the studies contribute to the point that hallucination is not neces-

sarily to be placed in the category of the pathological. Rather the concepts of scaling, degree and range are found with some frequency, but probably could have had more encouragement. The conceptual approach to most of the studies appears compatible with Klüver's early concept and also with the more recent work of Robert Holt that hallucinations are one form of sensory imagery often related to the eidetic image. One series of imagery classifications ran as follows: (a) analogy, (b) daydream, (c) fantasy, (d) pseudosomatic delusions, (e) illusion, and (f) hallucination. The general implication of gradation is at least present. Classifications in terms of complexity were used. For example, simplest (Type I) were flashes or flickering or dim lights without shape; more complex (Type II) were centrally located lights with shape but no color; while Type III were complex, highly structured, animated scenes rich in detail. Another complexity continuum is as follows: (1) lines, dots, and diffuse light; (2) geometrical designs, (3) isolated objects; and (4) integrated scenes. Some corroboration seems indicated also for Klüver's hallucinatory imagery form constants described as: (a) grating, lattice, fretwork, filigree, honey-comb, or chessboard; (b) cobweb; (c) tunnel, funnel, alley, cone, or vessel; (d) spiral. In general, the important point is that when degrees, ranges, and gradation are seen, the underlying, integrative conceptualizations are more likely to be in progress.

The Scheibels' discussion, brilliant as their writing usually is, shows again the fantastic complexity (a) of the brain stem reticular formation, (b) of the inter-relations of b s r f and cortical levels, and (c) the impossibility of understanding the higher level brain functions apart from the inter-active relations to lower levels. For those who still seem to assume that a complete wiring diagram of the nervous system would answer all our "S-R problems" about behavior the following quotation is appropriate: "The organization of collateral sensory inputs to the core is such that informational specificity may be lost and output appears related to the integrate of inputs." This conception is compatible with Klüver's point made



many years ago that all theories about form constants or imagery "stressing either 'peripheral' or 'central' factors are too simple." Relevant is the finding of some of the symposium researchers that continued stimulation of the eye with dim translucent light (which does not allow either pattern or form discrimination) is more productive of hallucinatory experience than no light at all. Thus, there is the implication that we deal with an active rather than a passive organism; that sensory materials are creatively reworked rather than simply relayed through a C.N.S. by some form of intervening variable.

THE SCHEIBELS' article occurs early—the second chapter—in the book. Perhaps the editor is playing the same hunch that would guide the reviewer, and that is that the needed integrative conceptualization for hallucinatory experience—and perhaps for all experience—will eventually be a product of a more comprehensive understanding of the role of reticular substance. Kluver, who saw close relationship between intense imagery and synesthesia said twenty years ago, "It seems necessary to assume some basic process operative in different sense modalities to cope with all varieties of synesthetic experiences." This reviewer is convinced that the "basic process" eventually can be identified, but would insist that to solve the problem the following requirements must be met: (1) the metaphysics of psychophysical parallelism and epiphenomenalism with associated approaches to concepts of consciousness must be superseded; (2) the attempts to reduce consciousness to action currents and other traditional types of physiological variables must be seen as inadequate; (3) emphasis on the organism as a total unit must modify and give perspective to a current over-emphasis on nervous system; (4) a more effective and integrative use must be made of the principle of continuity in the animal series than is currently apparent; (5) sustained and creative attention must be given to the need for a basic concept of the living organism; and finally, (6) consciousness, itself, must be seen as

dynamic, intimately involved in action, and a means of integrative regulation of behavior.

The need of an integrating concept for *Hallucinations* is a need shared by all psychology and psychiatry. It would be too much to ask that the editor and

contributors of this book satisfy a general basic need that the rest of us have not been effective in supplying. However, for those interested in the general subject of imagery or in hallucinations as a special aspect of imagery, this book is highly recommended.

## On Theory and Action

Mason Haire (Ed.)

*Organization Theory in Industrial Practice: A Symposium of the Foundation for Research on Human Behavior.* New York: Wiley, 1962. Pp. v + 173. \$5.75.

Reviewed by RAYMOND A. KATZELL

Mason Haire, editor of this volume, took his PhD with Lashley at Harvard and then moved, by way of work in military aviation, to industrial psychology at M.I.T. and on into the broader realms of industrial relations and industrial social psychology. In 1956 he authored *Psychology in Management* and in 1959 edited *Modern Organization Theory*. The reviewer, Raymond Katzell, is an NYU PhD who has been long concerned with organization theory and its applications. As Professor of Management Engineering and Psychology and as Director of the Research Center for Industrial Behavior at NYU from 1957-63, he was intimately involved in the theoretical and research sides of organization. During his earlier six years as Vice-President of Richardson, Belkows, Henry and Company he was involved with some intimacy in the action side of organizational affairs. His theory of organization is soon to be put to test once more, as he assumes the headship of NYU's All-University Department of Psychology.

THE CENTRAL CONCERNS of organization theory are structures—patterns of relationships among the organization's members as individuals and as groups. In addition to attempting to

identify significant kinds of structures and their interrelationships, organization theory addresses itself to questions of ways in which various structural patterns affect performance and goal attainment. Other issues of importance concern the shaping of structures by such inputs as the cultural setting, environmental conditions, technological and managerial methods, and member characteristics.

These kinds of problems, although comparatively new to behavioral scientists and especially to psychologists, are an old story to practical men of affairs. The book under review contains ten papers prepared by such men, all of them members of the managerial staffs of major corporations in the U.S.A. We are told in the preface that it is the intention here to portray "organization-theory-in-action," so that comparisons may be made with the approaches employed by behavioral scientists.

The result is easily as heterogeneous in substance and treatment as admittedly were the ten papers published in 1959 by behavioral scientists under Haire's editorship. Three of the present papers are case reports of the over-all organizational designs of specific companies: Genesco, Maytag, and Pennsylvania Bell Telephone. The last adds an interesting dimension by describing also



the steps through which the structure of a new division was developed. Four other papers are each addressed to some relatively specific aspect of organizational design or operation, including the consultant role of the headquarters staff man, the organization of the planning process, some conditions under which members might give more generously of their talent and energies, and the need for organizational structure to accommodate both the purposes of the enterprise and its contemporary business environment. The remaining three papers are thoughtful attempts to conceptualize one or another broad and fundamental facet of organizational life, in the businessman-scholar tradition of Chester Barnard; one of them analyzes the central importance of designing individual positions and their relationships so that work and responsibilities may be effectively accomplished; a second considers the implications of the proposition that the comprehensive objective of any organization is to maximize the synergistic effect of group effort; the third takes a careful look at authority in organizations, concluding that both personal and institutional or formal authority are needed by the enterprise and its members.

Does all this serve to fulfill the objective of depicting "organization-theory-in-action"? Alas, whereas those papers describing ongoing organizational arrangements in specific companies undoubtedly represent action, they provide little systematic theory. Conversely, those that are theoretically oriented supply little in the way of action, either in the form of operating programs based on the concepts or in the form of empirical research on the notions propounded. Scientific organization theory and its application appear to be as far apart as ever.

To psychologists, then, perhaps the main interest of this book will reside in its utility as a mirror, to help us better to see ourselves and our work. Have our theory and research had an impact on sophisticated practitioners? Are we addressing our efforts to problems that they also perceive? Is there anything in their views of organizational problems that might suggest redirection of our at-

tention? The answers to these questions have implications not only for the practical application of our work, but also for the improved alignment of our theories with the real world.

Regarding the first question, the book does furnish scattered evidence of the penetration of managerial consciousness by the work of behavioral scientists. This evidence includes mention in one form or another of McGregor's "Theory Y," Likert's "linking-pin" concept, and Argyris' concern for incongruity between individual needs and organizational demands. There is also evident awareness of the utility of such practices as participation in decisions and feedback of results. But such impacts appear mainly at the conceptual rather than at the action level, or, if the latter, in a somewhat tangential rather than central role. At a number of points, the work done by behavioral scientists would have been quite apposite, had the writers taken it into account. One example occurs on page 94, where there is speculation about the proportion of time allocated to "management" and "nonmanagement" functions by managers at various levels, without reference to the voluminous research on executives' activities by the Ohio State group. As a matter of fact, the book is depressingly free of allusions to research, past, present, or future.

We may find some reassurance in noting that many of the organizational problems and concerns raised in these essays have their counterparts in the kinds of things behavioral scientists have been working on. These problems include the sources and use of authority and power, facilitating the flow of information, motivating managerial performance, allocation of decision-making responsibility, determining the optimum number of hierarchical levels, and others equally familiar.

THERE are several other persistent issues, explicit or implicit in these essays, to which we behavioral scientists have been paying relatively little attention. One of these has to do with the proper allocation of responsibilities, functions, and tasks among positions and groups. This, of course, comprises the old departmentalization and job

design problems that have been focal in the engineering approach to organizations, but which still suffer from the absence of sound scientific theory. Here is a domain to which psychologists should be able to make important contributions, but we so far seem to be more concerned with the form and manner of organizational behavior than with its task-related content. The papers also make repeated mention of the importance of measurements of results as bases for accountability reporting and control; yet (with a few exceptions) behavioral scientists have not really woven such considerations into their theories or research—indeed, the concept of accountability is all but absent from our work. Perhaps our relative neglect of problems like these, which appear to strike the practitioners as being of major significance to organizational design and operation, helps explain why many of them view our efforts as somehow not quite coming to grips with reality. Either we should demonstrate why such considerations deserve little place in organizational theory and research, or else take steps to get them more completely into the picture.

In short, you need not read this book if what you are seeking is an exposition of scientific organization theory in action. But it might be worth reading if you happen to feel the need to check your views of organizational problems against those of a small sample of informed managers. If you do so, you will receive an extra dividend in the form of Mason Haire's brief introductory chapter, particularly his stimulating discussion of certain recurrent themes and myths found in some theories of organization.



*Originality gives a man an air of frankness, generosity, and magnanimity by enabling him to estimate the value of truth, money, or success in any particular instance quite independently of convention and moral generalization.*

—GEORGE BERNARD SHAW





# ON THE OTHER HAND



*In this Department CP invites discussion of reviews and of books reviewed. Here is the place for that kind of intellectual dissent that promotes progress in understanding. Let your criticism be ad verbum, not ad hominem. Seldom does a criticism merit more than half the space of the text criticized—never more than equal space and then only when the letter is interesting and well written. CP edits letters when it thinks they should be. Single-spaced letters will be returned for double-spacing.*

## AGAINST INTERLINEAR SPECULATION

Ivan D. London's review (*CP*, April 1963, 8, 153-156) of the Russian book by P. O. Makarov on *Methods of Neurodynamic Research* concludes that Makarov's book "... says more on occasion to the discerning reader than is spelled out on the printed page." London's review, I suppose, is such a special occasion because the review spells out very little about what the book actually has to say. The review's information count seems meagre in comparison with the elucidation provided in the same issue of *CP* by Richard Lynn's review and review translation of a different Russian work.

In London's review, speculation between the lines far outweighs analysis of the book's content. Just look at a partial listing of the review's notions: intrusion of political ideology, minimal intrusion of ideology enough to satisfy appearances, Pavlovian dogmatism, the use of Pavlov's name as window dressing, neglect of statistics, renewed interest in statistics, paucity of references to Pavlovian theory, and the smuggling in of use of verbal reports as a disguised weakening of Pavlovian theory. None of these interpretations is associated necessarily with the

book. As a matter of fact, some of them have appeared apart from the book (e.g., London, I. D. "Refractions of a Soviet Psychologist," *CP*, Mar. 1960, 5, 98-99).

The effect of the review, I fear, is to make the substance of Russian scientific work harder to understand than ever. Whether the review's political considerations are right or wrong is a problem for independent investigation so large that it merits a grant of its own. It does not do to introduce them in the frame of activities supported by a National Science Foundation grant apparently to spread knowledge by breaking the language barrier.

Between the lines of the printed page of a scientific book there may well be a no-man's land of the cold war. How unfortunate, it seems to me, to sacrifice the restraints imposed by the printed word to the passions of almost sheer blank space projection! The defense of science against contamination by politics is so strong that the defense extinguishes the reporting of scientific work. This paradoxical effect, so different from the basic purpose of a review, may be as unconscious as it seems obvious. I can only feel sad for what might have been another important contribution to American scientific psychology in the brilliant mold of London's "Research on Sensory Interaction in the Soviet Union" (*Psychol. Bull.*, 1954, 51, 531-568).

S. D. KAPLAN  
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*Contemporary Psychology: A Journal of Reviews* is published monthly. The subscription per year is \$10.00, foreign \$10.50, single number \$1.00.

Published by the American Psychological Association at 2210 W. Dallas, Houston, Texas and 1333 Sixteenth St., N.W., Washington 6, D. C.

Second-class postage paid at Houston, Texas and at additional mailing offices. Accepted for mailing at special postage rate stated in section 132.122, Postal Manual.

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# Contemporary Psychology

A JOURNAL OF REVIEWS

VOLUME VIII

September, 1963

NUMBER 9

## IPL: New Advance in Software

Allen Newell (Ed.)

*Information Processing Language-V Manual.* Englewood Cliffs, N. J.: Prentice-Hall, 1961. Pp. vii + 244.

Reviewed by ROBERT K. LINDSAY

Allen Newell, the editor here, received his undergraduate degree in physics from Stanford and his PhD in industrial administration from the Carnegie Institute of Technology. For 11 years he worked as a research scientist at RAND Corporation before becoming Professor of System and Communication Sciences at Carnegie Tech. Robert K. Lindsay, who here reviews Newell, has had some experience at being reviewed by Newell while the latter was his teacher at Carnegie. Like the author, Lindsay did his undergraduate work in physics and again like the author, received his PhD in administration at Carnegie Tech. Unlike the author, he sandwiched in between these two bouts with higher education an MA in psychology from Columbia University. He is now Assistant Professor of Psychology and Research Scientist in Computation at the University of Texas. He is also consultant to RAND, to System Development Corporation and to General Electric.

PHYSICISTS frequently acknowledge their debt to mathematicians. Mathematics is also indebted to physics, for the enrichment process is decidedly reciprocal. By contrast, experimental psychologists have made few

contributions to mathematics, and they have made only sporadic attempts to use mathematical techniques developed in contexts other than psychology. If we exclude mathematical description of data, such as by curve fitting and statistical analysis, and if we restrict our consideration to the use of mathematical concepts and notation as the bases of theory, we eliminate, with only a few notable exceptions, almost all of the applications of mathematics to psychology.

If one asks why psychologists have not made more use of mathematics, he frequently is told that psychologists are simply not good mathematicians. A less frequently heard reply is that mathematicians do not offer psychologists the techniques they need. Neither reply is the whole answer. Psychology will certainly benefit from more rigorous mathematical theorizing by more enlightened theorists, but special tools are needed for the special problems of psychology.

Physicists have been quite successful in isolating variables which interact very little with one another. Perhaps psychologists have merely not found the variables which will allow a similar factorization of their problems; or perhaps their problems are basically more

complex. If the latter is the case, behavioral processes—the data of psychology—are probably not best described by the mathematical techniques which apply to the data of physics. It may be that mathematical psychology will remain sterile if it restricts its efforts to the application of mathematical concepts developed to describe simple physical systems. A mathematical theory of processes—behaviors—could be the contribution of psychologists to mathematics, a contribution leading to fruitful interaction of the two disciplines.

THE DEVELOPMENT reported in Newell's book is, therefore, rather important. The author spells out the basis of a new technique for rigorous theorizing, and his motivation for this work seems to come largely from consideration of psychological questions, though indeed the applications are broader. The technique involves the use of a digital computer as a vehicle for theorizing and for making deductions from theory. Perhaps the new technique can be made clear by contrasting it with more familiar uses of computers.

We may distinguish three applications of computing. First, and most common, is the use of a computing machine to substitute numbers into formulae (statistical or otherwise) and thereby quickly obtain results which require enormous amounts of calculation. Second is the use of so-called Monte Carlo techniques: given certain stochastic assumptions, random numbers are generated and calculations are performed on each instance, thereby developing statistics for which simple computational formulae cannot be derived algebraically. The third technique is to state assumptions *as processes* which





ALLEN NEWELL

can be specified as computer programs and which need not involve arithmetic calculations at all. Examples include the association process, recognition processes, and hypothesis formation. Once such processes are rigorously stated as programs, the computer may perform them, and the observation of the computer's behavior may then serve as the deductions from the theory. The psychologist has, in effect, built his theoretical organism. By this technique of simulation it is thus possible to deduce rigorously the consequences of large numbers of theoretical postulates which cannot be stated conveniently in standard mathematical form, and hence to which classical procedures of analysis are not applicable. This new technique, developed in large part by Newell and his colleagues, removes many restrictions on methods of theorizing. Many mathematically stated theories might also benefit from this technique, particularly those that have been limited by the complexity of the deductive process, so that only special cases of a general theory could be studied.

Computers are notoriously poor conversationalists. To make their services more widely available, programs are written to translate a more nearly human dialect (the source language) into the numerical code of the machine.

One such translation program is FORTRAN, which allows users to employ a notation very similar to familiar algebra. As was pointed out above, no familiar notation exists for process description, and the job of discovering a good notation is in some sense equivalent to the job of theorizing about processes. Newell has defined a notation which will describe processes. His Information Processing Language V (IPL) is this notation, and an IPL system is a computer program which translates the notation into instructions for a given computer. When the language is employed, the user does not need to know the detailed characteristics of the machine that will do the translation and execute the programs.

By far the most extensive use of computers in science has been in numerical analysis (including data reduction and statistical work) and Monte Carlo simulation. As a consequence, most advances in computer hardware (the machines) and software (the programming techniques) have been in the direction of algebraic compilers and numerical algorithms. However, several contributions have been directed toward the development of a language for specifying symbolic processes. Most of these have employed the concept of list processing as their basic feature.

A computer's memory is typically divided into small segments called words, each of which can record a number. Each word is named by another, unique number, its address. A word's address never changes, though the word's contents may. Addresses are always smaller in magnitude than the maximum number which a word can record, and hence the contents of one word may include one or more addresses of other words. In numerical applications some words record numbers whose magnitudes are important in the calculations. In a list processing computer, the contents of words are divided into two functional parts, a symbol and an associative link. The symbol is most often not treated as a numerical quantity, but simply as a distinguishable entity which can be compared to other instances for identity. The associative link is an address of another word, and

thus it provides for the association between two symbols: given the address of one symbol, the next one can be found. In addition, symbols themselves may denote other lists of symbols, hence a hierarchical organization is possible.

Having committed memory organization to this list structure format, one must then provide special operations for manipulating these new units. Since computers are provided only with basic operations for manipulating single words and the numerical quantities they contain, a list processing language must provide programs for handling lists. Such a collection of programs forms the basis of Newell's IPL, the language under consideration in this review.

IPL incorporates several other interesting features. One example is the use of a list of available words: all computer memory not being used for any other function is tied together in a list. Whenever some additional memory is required, this list is consulted. This results in complete availability of memory capacity without accurate prior knowledge of the uses to which it will be put, thereby solving a ubiquitous programming problem. Also, the available space concept allows definition of recursive processes (processes which use themselves as subprocesses) and this feature proves extremely valuable.

NEWELL's programming language is designed primarily for the manipulation of lists and symbols. However, the facility for storing numerical data and performing arithmetic operations (both fixed point and floating point) is also incorporated. Other convenient features include: (1) a system for handling lists of attribute-value pairs; (2) "generators," which separate the functions of retrieving and processing information so that these operations may be conveniently programmed; and (3) a random number generator.

The IPL language is designed as a research tool, and little effort has been made to make it maximally efficient. The system is 'interpretive,' that is, each source language symbol is translated and the corresponding machine code executed before the next symbol is translated. Thus, if a portion of the



program is executed over and over it must be translated many times. FORTRAN and most other programming languages are 'compilers,' that is, all of the translation is done in one pass and the program is executed at a later time. Newell has sacrificed the greater speed of the latter technique in favor of certain design flexibilities of the former. For example, the source language programs themselves are stored as list hierarchies and are in the computer's memory at the time of program execution. It is possible for portions of a program to modify other portions of the program. This is one means of designing learning processes.

The syntax of most computer languages is quite awkward; that for IPL is extremely so. This is undoubtedly due in large part to our unfamiliarity with thinking in terms of this type of processing. Algebraic compilers, such as FORTRAN, have an advantage in that their symbolism, that of algebra, is highly familiar and thoroughly studied by prospective users. The difficulty in learning IPL is greatly increased by its awkward syntax, and although Newell's *Manual* reads as well as most programming manuals, that is not very well. This book is not for casual reading. The reader should be warned that a comprehension of the language and its uses depends upon careful reading and the working out of numerous examples.

The *Manual* is divided into two sections, "The Elements of IPL Programming" and a "Programmer's Reference Manual." Readers with programming experience will find Section I more confusing than enlightening, and will probably want to ignore it altogether. They will find a complete and precise description of the language in Section II. Readers without any programming experience will also find Section I confusing, but some may benefit from its detailed examples and suggested applications.

IPL systems presently exist for IBM 650, 704, 709, 7090, Control Data 1604, Bendix G-20, Remington Rand 1105, and Philco S-2000 computers. The widespread implementation and use of IPL gives it certain practical advantages

over other languages which are designed for similar uses. With the publication of this *Manual*, formerly available as a RAND Corporation document

(P-1897), IPL is the best documented and most public list processing language and deserves investigation by serious students of cognitive processes.

## Psychological Anthropology

Thomas Gladwin and William C. Sturtevant (Eds.)

*Anthropology and Human Behavior*. Washington, D. C.: The Anthropological Society of America, 1962. Pp. vii + 214.

Reviewed by WILLIAM E. HENRY

*who is Professor of Psychology in the Department of Psychology and in the Committee on Human Development at the University of Chicago. He received his PhD from the Committee on Human Development in 1945, worked as an Instructor in the Army Specialized Training Program, and as Social Science Analyst, U.S. Department of the Interior, Office of Indian Affairs, before getting his degree. He has recently (1960-61) been visiting Ford Foundation Professor at Michigan State University. He is the author of many articles and monographs and, most recently, with Elaine Cumming of Growing Old (1961, CP, Nov. 1962, 7, 416).*

THE EIGHT chapters of this volume represent lectures given before the Anthropological Society of Washington. Seven of them present various views of the interaction of anthropological and psychological issues. While the experience of the authors in the dual fields varies, all do to some degree attempt to relate their comments to data or concepts from both disciplines. Five present new or revised views of conceptual or motivational fields, and suggest some relevances for anthropology and/or for the broad field of the study of human behavior. Three deal with various direct interrelations of psychology and anthropology.

The first chapter, by Anthony F. C. Wallace, was last in the original series and constituted a review and summary. It is printed first here and serves much the same purpose. It does, however,

go beyond those functions, in that it presents Wallace's views of the former and the newly developing complexion of the culture and personality field, increasingly becoming known as "psychological anthropology." Wallace sees a variety of changes occurring in the area, roughly those of movement from earlier instinctual, unconscious psychoanalytic formulations toward more socially related and more cognitively related concerns. His view of this shift, particularly as it includes attention to cognition is amply demonstrated by three papers in the present book. Ulric Neisser's "Cultural and Cognitive Discontinuity," Dell H. Hymes' "The Ethnography of Speaking," and Charles O. Frake's "The Ethnographic Study of Cognitive Systems," all focus upon cognitive process. Neisser's is essentially informative for the anthropologist, as he reviews psychological approaches to the study of higher mental processes. His emphasis is upon concepts of information processing, relying upon Piaget, Bartlett and Schacter. Hymes and Frake deal with semantic issues, Hymes emphasizing some of the shifts from psychodynamic to cognitive frames of reference and Frake with the taxonomy of the speaker's view of the world and its relation to the ethnographer's structural analysis.

Wallace's comments on reformulations of motivational theories are substantiated by two papers. William Caudill's "Anthropology and Psychoanalysis: Some Theoretical Issues" describes modifications in psychoanalytic theory, particularly as they have been



influenced by social theory. Caudill by no means discards all of the classical analytic formulations, however, and comments pointedly from clinical as well as anthropological data on their continuing relevance. He does, nonetheless, find the more recent ego psychological concepts highly functional and finds both Erik Erikson and Freud extremely useful in the analysis of his studies of Japan and Japanese psychiatry. Another reformulation, and one closely related to the cognitive papers above, is that of Daniel E. Berlyne, who in his "New Directions in Motivation Theory" posits essentially a motivating force stemming from cognitive considerations. While Caudill relies centrally upon psychoanalytic concepts and anthropological data, Berlyne draws his support from experimental psychology and neurophysiology. He proposes a kind of curiosity-seeking and resolving motive and describes the concept of arousal levels, suggesting their relation both to physiological states and to the amount and nature of environmental excitation. He relates these to anthropological issues primarily through questions as to the nature of societies demanding various arousal levels and to possible cultural determinants of perceptual curiosity.

**A** REDUCED emphasis upon national character and group character as a kind of monolithic group nature, noted by Wallace, is clearly documented in the one paper by Richard Jessor. Jessor reports "A Social Learning Approach to Culture and Behavior" in which he comments upon specific researches utilizing a combination of Mertonian analysis of social structure and Julian Rotter's social learning theory. Jessor, and Wallace, would see this approach as a desirable one, in that it moves more toward the examination of smaller person-system units.

In part documenting Wallace's views, these papers do emphasize the increasing centrality of cognitive processes and of motivational theories modified in socially relevant directions. Wallace also suggests increasingly sophisticated ways of relating personality to culture, an emphasis notable in Caudill, in Margaret Mead, and on a more confined

scale, in Jessor. The continuation of interest in child development, implied in Caudill and Mead, is also suggested by Neisser.

The resurgence of biological determinants of behavior and a focus upon culture change, suggested by Wallace, are not centrally dealt with in the volume, though Mead comments on both and demonstrates the latter in her reference to her important study of culture change in Manus.

For the psychologist wondering what the field is all about, the papers of Caudill and of Mead, as well, of course, as the overview of Wallace, are most relevant. I have reserved Margaret Mead's paper for the last, since it is in a way both a personal and a cultural summary. It is in the form of a work autobiography, aiming at clarifying Mead's views through a documentation of her work history. Her "Retrospects and Prospects" clearly shows the whole course of her own work, displaying the diverging and overlapping concepts—from psychology, anthropology, psychoanalysis, sociology and political economy. It documents not merely the magnificent scope of the author, but also the methodological and conceptual adventures in the area.

In selecting the invitees for the lectures, the editors (and I presume the selectors) Thomas Gladwin and William C. Sturtevant would seem to have given preference to persons concerned with cognitive areas. Their presentations are interesting and systematic, giving an impression of importance to this area with which Wallace, in his overview, is clearly sympathetic. Caudill and Mead, however, clearly the most experienced among the other participants in this complex field, deal only peripherally with it, except insofar as their attention to the more interactive and socially relevant variables may imply attention to the rational through lesser attention to the instinctual and the unconscious.

The lectures must have made an exciting series, and the solid and often dramatic intellectual explorations characterizing it come through in the volume. The psychologist intrigued by these presentations should perhaps also be encouraged to look into other recent

efforts to state and to criticize the nature of the field. These would logically include the volume *Psychological Anthropology* edited by the anthropologist Francis Hsu (1961, CP, Aug. 1962, 7, 299); that edited by the psychologist Bert Kaplan, *Studying Personality Cross Culturally* (1961, CP, June 1961, 7, 210) and the analysis by Gardner Lindzey, *Projective Techniques and Cross Cultural Research* (1961, CP, Mar. 1963, 8, 102).

## Are Theories Necessary?

Aubrey J. Yates

*Frustration and Conflict*. New York: Wiley, 1962. Pp. v + 236. \$5.00.

Reviewed by REED LAWSON

The author, Aubrey J. Yates, is an Englishman who received all of his training in England, ending with a PhD from the Institute of Psychiatry at the University of London. He has been a Lecturer in the Institute of Psychiatry at Maudsley Hospital and is presently Senior Lecturer in Psychology at the University of Western Australia. His current interests are in the fields of "communication and high-level skilled behavior." The reviewer, Reed Lawson, received his PhD from the University of Missouri, staying on there afterwards for a while but then moving to Ohio State University in 1957 to become an associate professor. He is the author of *Learning and Behavior* (1960) (CP, Aug. 1961, 6, 275-276) and senior author with Melvin Marx of a review in *Genetic Psychological Monographs* (1958) dealing with the experimental and theoretical work on frustration. His current interests include verbal behavior, punishment, and conditioned reinforcement.

**I**N "Frustration and Conflict" we have an excellent example of a scholarly summary of some important



"miniature theories" and their attendant experimental work. This book can be read profitably by anyone who wishes a thorough, clear exposition of the main-streams of American psychological thought and research concerned with the two topics with which it deals.

The basic organization of the book is to present, in turn, each of the major theoretical treatments of these topics that has been presented in the past 25 years or so, along with the experimental research directly bearing on that position. The coverage of experimental data is quite up to date and, within the restriction Yates imposed, very comprehensive.

The restriction apparently imposed was that an experiment must literally have been a direct 'test' of a theory before it could be considered relevant. One gets the impression, for instance, that an experimental study of conflict would not likely be included in this survey if it did not actually cite Miller's or Lewin's writings in its list of references. This is a practical rule of thumb that enables the reviewer to keep the range of material within reasonable bounds—which is no mean task in an area such as 'frustration.' But such a practice, it seems to me, has two undesirable consequences.

First of all, there really are quite a few studies that bear on these topics, even though not specifically related to them via the introduction or discussion sections of the reports. To take just one example, the Miller-Hull analysis of conflict integrally involves certain conceptions of the process of generalization. The work during the past years by Norman Guttman and his associates has been causing a revision in (certainly a clarification of) our concept of the dynamics of generalization. Doesn't this work perhaps lead to a clarification of some of Miller's notions? Isn't it possibly the responsibility of a scholar trying to summarize experimental work on conflict to discuss the possible implications of a closely related body of work?

THE above criticism might be disputed, but it leads into the second difficulty with Yates' strategy of analysis. He presents several theories at face value, and discusses them only in terms

of research which in its inception also took them rather literally; this is an approach that tends to preserve a kind of theoretical *status quo*, the value of which is dubious at our present level of development. It leads, whether Yates intended it to or not, to an admiration of theorizing as a verbal-intellectual accomplishment without regard to the ultimate value of theorizing in science.

One further minor objection may also be raised. Research in which responding is not primarily spatial in character is discussed very little. The basic definition of frustration for which Yates expresses a preference is the old physical barrier one. Conflict is discussed almost entirely in terms of movement toward and away from goals. He hardly mentions free operant work (which is essentially non-spatial responding) bearing on frustration and conflict. This seems strange for a writer who is apparently concerned to some extent with clinical psychology and personality. Verbal behavior must certainly be related to frustration and conflict, but can this behavior be discussed in terms exactly analogous to those appropriate for describing movement in space? The possible difficulties of applying the principles of the latter response system to the former are only rarely and indirectly alluded to.

Most of the above is primarily an objection to the frustration/conflict research field, especially as Yates has specifically defined it. The major objection to the book itself is that the material is reported so unquestioningly in any fundamental sense. Has progress been made toward a useful theory of frustration or conflict? It is hard to tell from this volume alone, and when one realizes that empirical work is continually challenging many of the concepts on which are based the theories described, then the value of this book as other than an essay on one small part of recent psychological history may well be questioned.

This review would be extremely misleading, however, if it did not conclude by emphasizing again that Professor Yates has covered his chosen topic well in a clear manner. In what he set out to do, he succeeded admirably.

## Chemists Too?

Anselm L. Strauss and Lee Rainwater. With Marc J. Swartz, Barbara G. Berger and W. Lloyd Warner. Foreword by Albert L. Elder

*The Professional Scientist: A Study of American Chemists.* Chicago: Aldine, 1962. Pp. xiv + 282. \$6.00.

Reviewed by ANNE ROE

Anselm Strauss, one of the principal authors, is a sociologist who did his graduate work at the University of Chicago and is now Associate Professor of Sociology at the University of California School of Nursing. The other principal author, Lee Rainwater, received his PhD in psychology from the University of Chicago and is Director of the Institute of Sociological and Psychological Studies. Marc J. Swartz is an anthropologist and Barbara Berger an educational psychologist, both at the University of Chicago. W. Lloyd Warner is now University Professor of Personnel and Production Administration at Michigan State. The reviewer, Anne Roe, has had a varied and productive career that has led to her present involvement in the study of careers. She is Professor of Education and Research Associate at the Harvard Graduate School of Education, and is also Director of the Center for Research in Careers, just being established at Harvard. She has a special interest in the studies of scientists and is at present revisiting the scientists she first studied fifteen years ago and wrote about in her book, *The Making of a Scientist* (1953).

THIS book, as the subtitle indicates, relates to chemists, except for some comparisons and brief opening and concluding notes on professions in general. It was sponsored by the American Chemical Society, whose members have for a number of years been engaged in a somewhat uncomfortable appraisal of their status—not as se-



curely professional as they would like—and of the appropriate role of their major society, (the ACS), in advancing their establishment in this role.

The heart of the book is a detailed report of questionnaire responses, and interviews with chemists and chemical engineers both in and out of the society, and with business men and other professionals who deal with them.

Because of the quite different purposes of the two books, and the different sampling methods employed, not many direct comparisons can be made between this study of chemists and Clark's study of American psychologists, although many of their more agonizing concerns are more than faintly reminiscent. Interestingly, the authors are not unaware of this and refer in a number of places to some of our experiences which are similar to the chemists' present concerns . . . "as the post-war history of psychology has demonstrated, tensions develop when academicians are reluctant to legitimize the work of colleagues outside the universities—unless they themselves can retain secure control over the training and work."

The ACS is considerably larger than the APA—93,000 members in 1962—over 90% of their members are men, only a sixth are primarily employed at universities, and about 40% of them have the BA only. Their predominantly industrial role, and the large number without advanced degrees are, of course, important reasons for the fact that neither they nor the public see them unequivocally in the professional role.

There is much in the details that should be of interest particularly to counseling psychologists, and to those interested in the psychology and sociology of professions.



*So long as we can make efficient use of things, we feel no irresistible need to understand them. No doubt it is for this reason that the modern mind can be so wonderfully at ease in a mysterious universe.*

—ROBERT M. HUTCHINS



## Encompassed by Small Groups

A. Paul Hare

*Handbook of Small Group Research.* New York: Free Press of Glencoe, 1962. Pp. v + 512. \$10.00.

Reviewed by BERTRAM H. RAVEN

*A. Paul Hare, the author, is a sociologist who taught at Wellesley and Harvard, held research posts at Yale and Princeton and was Associate Sociologist at the Massachusetts General Hospital before assuming his present position as Associate Professor of Sociology at Haverford College. The reviewer, Bertram H. Raven, now Associate Professor, Department of Psychology, UCLA, received his PhD in Social Psychology at the University of Michigan and conducted research there at the Research Center for Group Dynamics. He has continued a research program investigating social influence processes in the small group. During 1954-55 he was a Research Scholar (Fulbright) at the University of Nijmegen, Netherlands, and during 1962-63 he was Visiting Professor at Hebrew University, Jerusalem.*

PERHAPS the affluence of our society is indicated most clearly in the sheer quantity of research publications streaming forth in all disciplines. Psychologists have long given up attempts to be versant in related fields. Time limitations force them to direct their reading to only one small subsegment of their own. Government committees have met to discuss the problems of maintaining surveillance of research findings. One well-known psychologist avers that he no longer reads any publication unless it has been recommended to him by at least two people whose opinions he respects, and, knowing the stringency of his criteria, it is conceivable that the problem is thus solved for him.

If one's interests lie in a field such as 'small group research,' then his problems are compounded by the fact that various disciplines contribute to the torrential flow—sociology and psychology (with their sub-disciplines such as industrial, social, military, personality, child, and educational), social work, industrial relations, political science, public health. Hare, in his survey of the literature on small groups, found that in the decade before the turn of the century, an article on small groups appeared approximately every two years. By the 1920s, this rate rose to 21 per year, then to 31 per year in 1940-1944, and 152 per year in 1950-1953. That this increase is continuing is evident from this reviewer's count of nearly 200 studies of the small group in 1960. Using as his basis a 1400-item bibliography compiled in collaboration with Fred L. Strodbeck, Hare determined to perform an even greater service in culling through the massive literature, summarizing the basic conclusions, and assembling these in a coherent form for others who are unwilling or unable to go through a similar ordeal. Thus Hare's *Handbook of Small Group Research* follows in the tradition of the monumental *Experimental Social Psychology* by Lois and Gardner Murphy (1931), revised with Theodore M. Newcomb in 1937, rather than later 'handbooks of social psychology' (C. C. Murchison, 1935, and Gardner Lindzey, 1954) in which topics were assigned to a number of individual authors for each to summarize and integrate in his own way.

The Handbook is composed of three major sections. In the first, the



author considers central tendencies in groups: the normative patterns of interaction, role development, and social control. In a second section, Hare examines some factors which cause deviations from the central tendencies: individual personalities, group size, the nature of the group task, and the communications network. Finally, a section is devoted to "group productivity," considered as a dependent variable, affected by all of the factors discussed previously. Such an organization leads to considerable overlap, as Hare recognizes. A single study might be cited with equal appropriateness in any of the three sections. However, the plan of the book is clear enough so that, given some trial and error, one can find a summary of research on a particular problem.

Serious students of the small group will be delighted to have at their fingertips elaborate descriptions of classic studies such as Triplett's investigation of dynamogenic factors in cooperation and competition, the O.S.S. assessment project, and the Western Electric studies of employee effectiveness. Inevitably, the reader will find himself wondering why a relatively insignificant study will receive a number of pages of discussion, while an investigation which the reader considers important will be relegated to a passing reference. In the latter case, he may find that study included in abstract form at the end of the chapter, along with a number of other studies which Hare includes as an "exhibit" of a particular line of research. It is to the credit of the author that, though a sociologist in orientation, he includes a wealth of material contributed by psychologists and other disciplines. Only in some of the most recent material, such as research stimulated by dissonance theory and game theory, will one find noticeable gaps.

There is some indication that Hare's original plan for this volume included a broad organizing theory which would encompass all of the research conducted to date on small groups. The beginnings of such an attempt are indicated in the initial chapters and, occasionally, in later introductory paragraphs. The theory, basically an output and input analysis represented in a two-dimen-

sional plot, analyzes group behavior, role, and personality in terms of the type of relationship, either obtained or desired, among individuals in a group. Personality typologies are suggested on the basis of such analysis: a "submissive" individual has a high input of influence and a low output; a "dominating" person has high output of influence and low input; an individual with low input and low output would be "apathetic." One can then analyze interactions among individuals in terms of their desired and received inputs and outputs. This conceptual scheme, which

was in part influenced by William C. Schutz, does not carry the author very far in this volume. It appears that Hare intends to develop his theory further in a future project.

A very handy reference book, Hare's *Handbook* will be found on the desks of many students of the small group. To individuals not familiar with the field, it will serve as an introduction and guide to further reading. To those specializing in the small group, it will offer some comfort—in this massive literature, one can at least see some semblance of order.

## Life and Depth Psychology

Dieter Wyss

*Die tiefenpsychologischen Schulen von den Anfängen bis zur Gegenwart: Entwicklung, Probleme, Krisen.* Göttingen, Germany: Vandenhoeck & Ruprecht, 1962. Pp. XXXI + 412. Lw DM 32,-

Reviewed by BRUNO KLOPPER

The author, Dieter Wyss, spent most of his school years in South America and Switzerland, but returned to his native Germany in 1949 as an assistant of Victor von Weizsäcker in the Department for General Medicine at the University of Heidelberg. Since then he has had training analysis with Mitscherlich and has worked for some years as an assistant of Professor Zutt in the Psychiatric Clinic in Frankfurt-Main, where he now has a clinical practice. The reviewer, Bruno Klopfer, is a thoroughgoing Jungian psychologist. After two years of Freudian analysis (1927-29) by Dr. Werner Heilbrun, and repeated theoretical encounters with Jung's thinking, including his doctoral dissertation (PhD in 1922 from the University of Munich), he worked with Jung personally for a year or so before coming to the US in 1934. He has taught at Columbia University, CCNY, UCLA and Claremont. In recent years he has been a part time analyst, and has been a member of

both New York and California Jungian groups, and he spent four spring terms in Zurich teaching at the Jung Institute. He is currently a clinical professor of psychology at UCLA but plans to retire in June, 1963, to devote himself full time to Jungian psychotherapy.

IN the first year of its publication *ICP* (June 1956, 1, pp. 170-171) reviewed Ruth Munroe's *Schools of Psychoanalytic Thought*. The reviewer ended his enthusiastic write-up with the statement that "hers may indeed become the definitive text of the next decade." Dieter Wyss challenges this claim mostly by the fact that he grew up in the German language area where most of the post-war schools had their origin. As a disciple of Victor von Weizsäcker, and as a German psychiatrist, he participated in the scientific and philosophical tradition from which the variety and complexity of this development stems.



The twenty-two pages of his "Table of Contents," with their page-by-page, sometimes even paragraph-by-paragraph, description of his thought progress, with all the analysts' names interspersed (reassembled at the end in an "Author's Index"), and with an impressive list of hierarchically ordered numbers and letters—all this sometimes makes one feel that the sequence of letters and numbers lends an unjustifiably authoritative support to the often surprising combination of names and schools, but the text usually supports the internal necessity of the arrangement.

The first half of the book is devoted to depth psychological theories, with a natural science orientation, which are subdivided into Freud, the schools of Freud, and the neo-psychanalytic schools. The latter contains a more careful selection than one usually finds in American textbooks. The more specific contributions are in Parts II and III, which first line up the philosophically oriented theories and then tackle the basic problems of their relationship to the whole. The emphasis shifts in the second part to a greater prevalence of German-language authors versus Anglo-Saxons in the first part.

THE positivistic claims of the Freudian schools, and especially the problem of causality, are dealt with in considerable detail, but Wyss comes (on page 336) to the conclusion that these claims are essentially fictitious. With the same 'objectivity' he discusses the relationship between Freud and Jung. We may use as an example his demonstration of ways in which Freud and Jung used the concept of "symbol." On page 241 he explains: "For Freud the symbol is nothing but an early stage of a mental idea, a sign for something . . . which is identical with image and idea (Bild and Begriff) . . . Jung, in contrast, uses the symbol to amplify." The symbol cannot be defined or its meaning exhausted in a definition. Wyss emphasizes that Jung refers himself to Cassirer's *Philosophy of the Symbolic Forms* and essentially perceives the world in the famous line at the end of Faust: "All earth comprises is symbol alone"

("Alles Vergangliche ist nur ein Gleichnis"). Wyss explains, on page 389: "He (Jung) attempts to document this view of the world psychologically, i.e., he compiles in extraordinary scholarliness the material for the correctness of this view (amplification) . . . Through this material, as well as individual case studies, and also through fairy tales, myth, and philosophies, his (Jung's) concepts find a broader empirical basis than the older teachings and systems."

But, in spite of this admission of the empirical character of Jung's work, Wyss raises the question whether one could claim for Jung the scientific methodology of historical sciences. He comes to the formulation of Victor von Weizsäcker: "Truth is not a rational but an irrational problem of decision." Wyss confronts the common-sense elements in Freudian psychology with the "strange and more complex prerequisites in Jungian thinking in order to explain the greater popular appeal of Freud, especially in the United States (p. 390)."

THE first ninety-eight pages of Wyss's book describe under the title, "From Symptom to Person" (1880-1905), the personal influences on Sigmund Freud's development. The first two pages contain a highly condensed biography in which the essential data of the first thirty-five years of his life are compiled. The following sections are arranged around certain contents like hysteria, dream interpretation, anxiety, and sexuality. Section VII has the title, "Historical Influences." This calls for a comparison with a similar historical analysis which forms the first chapter of the *Festschrift* to Jung's eightieth birthday; it appeared in the same year as Munroe's book (Rascher, Zürich, 1955), with contributions in three languages, English, French, and German. The text preceding the only major review of Jungian literature (CP, March 1959, 4, 72ff.) explains why this *Festschrift* was not reviewed.

The author of this chapter, Dr. Lili-ane Frey, Zürich, chose an almost identical title as Wyss, "The Beginnings of Depth Psychology from Mesmer till Freud" (1780-1900). The seventy-nine

pages of this chapter offer a most intriguing comparison with the seven pages of Wyss's "Historical Influences" (which also shows the very condensed way of Wyss's writing). While Wyss seeks the beginning in Kant's disciple, Herbart, and follows the development over Fechner and L. Bretano to Breuer and Meynert, the two personal influences in Freud's life, Frey seeks the beginning quite a bit earlier in the philosophy and scientific efforts of the romantic, from Leibnitz's concept of the unconscious, Schelling and Carus over the development of Mesmer's animal magnetism, the ideas of sonambulism and hypnosis, the theory of suggestion by A. A. Liebeaults and H. Bernheim, leading to a detailed analysis of Charcot (1825-1893) and Janet (1859-1948), who built the personal links to Freud.

At the end of his historical analysis, Wyss returns to the "discomfort" of Freud's thinking, which leads him to the recognition of the irrational which he "cannot really understand" (page 50), while Frey demonstrates how Jung's concept of individuation sheds a new light on Mesmer's animal magnetism (*fluide universel*).

While it may be perfectly true that Munroe's book is "written for the intelligent layman" and is outstanding for her "sensitivity to the reader's needs," Wyss's book is clearly meant for the professional scholar and therapist. It would keep anyone busy for years, even if he had no difficulty with the German language.



To those young men and women who are hesitating in the choice of a future career, that of the research worker is here recommended. It means a lifetime of devotion to a cause which probably will bring neither fame nor fortune. One who chooses it must find most of its rewards within himself . . . and his reward is in the knowledge that he has carried the standard of science a little further into the unknown.

—DR. JOSEPH A. CUSHMAN







### A JOURNAL OR A MAGAZINE?

ONCE in a while there comes along a phrase or expression that brings with it a meaning the span and scope of which seems all out of proportion to the apparent character of the expression itself. The single, simple phrase may supply the missing element without which the miserably complex cannot be made into that elegant simplicity by which life can be lived and science advanced. One humble expression, capturing just the right and heretofore elusive shade of meaning, affords a new and pleasurable way to construe a significant recurring experience.

For the present writer, the term "enlightened confusion," coming from a now unidentifiable source, has been such a capstone phrase; it comes frequently to tongue when the discussion turns to liberal education, open mindedness, democratic process, or to the making of intelligent decisions about intricate matters inadequately understood. Another phrase, occurring in the speech of Kenneth Spence while he and the writer once strolled across the campus of the University of Washington is "there are process psychologists and there are organism psychologists." This phrase—and it may or may not have been original with Spence—has often been of felt utility in thinking or talking about the varieties of approaches to psychological science or about the characteristic slant of any one psychologist.

One-phrase bits of instant education may be encountered in the morning news or in a drug-store paperback. When a body is down with a puzzle and has reached the proper stage of enlightened confusion concerning it, his system is open to any input that may serve to bring a rounding off of things.

He may find the missing fact or phrase or discrimination almost anywhere. But he is more likely to find what he needs if he sticks to the company of writers and talkers who are in the habit of making unusual and unusually precise discriminations and who have the capacity to catch in words the exact nature of what the discrimination is. And it is probably true, though not necessarily so, that a puzzler is more likely to get his needed discriminations from one who lives in the particular cognitive and semantical neighborhood in which the puzzle is located. Certainly if anyone is intellectually entangled with problems involving *CP*, he does well on every count to expose himself to E. G. Boring. In a marginless self-typed postcard, in the more formal, more elegantly arranged but not necessarily juicier form of a letter, or in any one of the many published paragraphs in and about *CP*, Boring may give him precisely the discrimination he needs—though he may not have known his need until after it has been met—and precisely the phrasing of it that will bring pattern where there is none and that will allow what feels like thought and reason to replace what surely was a semi-cognitive mish-mash of ideas and attitudes.

A RECENT letter from Boring carried the phrase "a magazine, not a journal." For *CP*, that phrase, even apart from the good sense accompanying it, very quickly clarified a number of vaguely worrisome problems and almost immediately banished a swarm of niggling little nits of doubt about many decisions already made—already made on the basis not of any explicit policy or principle but at best in accordance with nervous intuitions. *CP* is and should be more of a mag-

azine than a journal. That's the ticket. All along it has in actuality been more of a magazine than a journal. But now that Boring says it, things become clearer and can be faced explicitly. If, as a matter of policy, *CP* is a magazine, the policy can be carried out now with more consistency—or dismissed with more dispatch—if that's the way feedback falls. Of course, it remains to be seen whether there can be enough definitions of "magazine" and of "journal" to give utility to the distinction between them.

There are a number of dimensions with respect to which magazines seem to differ from journals. A look at some of these dimensions, although they may not be as crisply definable nor as conceptually independent as one might like, may help clarify the existing, and maybe proper, differences between *CP* and the variety of other publications in APA's stable.

THE MAJOR difference between *CP* and other psychological journals seems to be found with respect to a dimension of personalness. Magazines, like *CP*, tend to be personal while journals are not. *CP* is personal in that individuals, identified as such, write in other than a coldly impersonal way about the work of other individuals, also at least sketchily identified. Journals, by contrast, are impersonal. The role of the writer of journal articles is that of the ruthlessly objective scientist who filters hard data through a cortex as impersonal as a computer and who sets down conclusions that have a properly dehumanized inevitability about them. Some journal writers can play the proper role and still inject some human juice into their articles, but departures from established expectancies come easy only to the true deviates or to the very secure and distinguished psychological scientists, and even the latter may find it impossible to escape the impersonal confines of the scientist's role except on ceremonial occasions such as a presidential address or an invited lecture at a symposium.

*CP's* reviewers are encouraged to be personal. They are even encouraged, within limits, to be subjective, to ex-



press their general personal reactions to the book with which they are dealing. And they are steered away from an exclusive concern with objective and impersonal abstracting of the book. *CP* should not compete with *Psychological Abstracts*. It should be a more personal publication. But there are limits and proprieties that must be borne in mind if the personal is to be printable, is to be more than the maunderings of noisily unharnessed idiosyncrasy. The personal reviewer, to write well and properly must write from a known stance. His reader must be able to take into consideration what is the reviewer's stance, and must be able to allow for the reviewer's personalness in formulating his own personal reaction to the book about which the reviewer writes. *CP*, by established procedures, attempts in several ways to keep the fire of personalness while avoiding the chaos of wild idiosyncrasy. It writes a biography of the reviewer. A purpose of this procedure is to tell the reader not only that the reviewer is a person—a whole and intricate person—but also that the reviewer is a particular kind of a person who might be expected to have a particular kind of posture toward psychology in general and a particular, maybe peculiar, slant toward the specific book he writes about. Frequently the reviewer naturally and subtly supplements this biographical material in ways that can help the reader toward the formulation of his own independent assessment of the book being reviewed. The freedom to write personally, then, must be both arranged for and earned. And personal writing must still flow within well-established if sometimes hazily seen restraints. At one extreme there is the restraining influence of libel laws. At a more mundane and less litigious level, there are the general proprieties of literary criticism. According to these properties, as *CP* sees them, the personal reviewer does make judgments, does use adjectives—as any human being must do in responding to his world. But in using adjectives about a book, the reviewer not only writes from an explicitly described stance but he also presents as best he can the evidence

upon which he bases his adjectival juridical judgments. The rules of scientific evidence, the juridical precedents of due process and everyday considerations of simple politeness, functioning in remarkably close congruence all can guide the proper use of adjectives. In effect, the reviewer is saying to the reader: "Look, I'm this kind of individual and I tend to think in this general way. I see this book as this kind of production. It seems to me that the

following documentation constitutes evidence in support of my position. But watch it, friend. There is at least an outside chance that I'm a least bit fallible."

Are there other differences between a magazine and a journal? *CP* says yes. Are there difficulties and even dangers in keeping *CP* a personal publication? Again, yes. But these are matters for future consideration, both by *CP* and its readers.

## Out Via the Door

R. D. Laing

*The Self and Others: Further Studies in Sanity and Madness*. Chicago: Quadrangle Books, 1962. Pp. ix + 186. \$6.00.

Reviewed by SIDNEY M. JOURARD

*The author, Ronald Laing, is identified in the review. Sidney Jourard, the reviewer, took his MA at the University of Toronto and his PhD degree at the University of Buffalo. He taught at Emory University and at the University of Alabama Medical School before assuming his present position as Associate Professor in the Department of Psychology at the University of Florida. In 1958 he authored Personal Adjustment a second edition of which was released this spring, and is presently down with a book dealing with self disclosure, an area he has been exploring for the past few years.*

MANY psychologists are discontented with both psychoanalytic and quasi-behavioristic accounts of interpersonal relationships; they feel that something important is being left out. The omission seems to be man's self. But it is no easy thing to portray the subjective experience of self-being, and it is more difficult to portray the interactions between two selves. Laing has tackled this problem in what I think is a masterpiece of existential and phenomenological analysis, written in superb English prose.

The author of this fascinating little book sees psychosis as the way out of an "untenable position." People move or let themselves be put into such positions when they elude authentic experience and pretend to be who they are not. The way out of an untenable position in an interpersonal network, or nexus, is really via the door, but this step can only be taken by someone who knows who he is, and whose identity-for-himself has been confirmed by others. As Laing puts it, within the phantasy of the nexus, to leave may be an act of ingratitude, of cruelty, of suicide, or of murder.

Ronald D. Laing is a psychoanalyst doing research in interpersonal aspects of psychosis, at the Tavistock Institute and Clinic in London. He writes from a phenomenological-existential viewpoint, but unlike many who have sipped the existentialist wine, he has become neither muddled nor despairing. He writes in pellucid prose about subtle intrapersonal and interpersonal processes.

One contribution which impressed me was his discussion of confirmation of one man by another. He has adopted Buber's term in a way that most psychologists will find provocative and con-



genial. He explores the question of who confirms what in whom, and how. The relation of this line of thinking to reinforcement theory suggests that students of operant conditioning may be able to explore questions of personal identity.

I was most intrigued by Laing's analysis of "existential position as a function of the action of the self." In this chapter, the author explores what is meant by "putting oneself into one's actions." He shows how a man fulfills and becomes himself in the act of authentic self-disclosure. People who do not reveal themselves or whose self-disclosure is not acknowledged by others turn in despair to false modes of self-disclosure. Laing links these ideas in a most intriguing way to paranoia, hysteria, and schizoid ways of life.

THE ENTIRE book revolves around the concept of phantasy, which Laing sees as an unconscious mode of experience, different from imagination, dreams, and reality. Phantasy is a primordial level of experiencing the world, the self, and people in such primitive terms as empty/full, good/bad, etc. Phantasy accompanies or undergirds conscious and rational experience, and provides one's experience of relatedness to others with a dimension of depth. However, one can become submerged in a phantasy nexus with others, and never realize that one has been so involved until one has somehow gotten out. Psychosis is seen as a rather ineffective way out of a situation that can no longer be borne.

There are many clinical illustrations, exquisitely and simply described, of how patients lost their selves, or gave them up in order to exist in relations with parents who simply would not let them be, and who undermined their sense of their own identity in ingenious ways. A discussion of "collusion" clarifies the way in which people can collaborate, in their relationship, so as to insure that each will maintain a make-believe sense of his own identity. Laing cites Genet's play, *The Balcony*, to illustrate how a prostitute colludes with her clients, permitting them to think they are who they hope they are.

Two engaging, and illuminating chapters that illustrate many of the concepts of this book are entitled "Driving

the Other Crazy," and "Masturbation." In the former, double-binds, and other schizogenic modes by which *P* undermines *O*'s sense of his personal identity are explored. In the latter, Laing portrays the masturbatory act as an expression of a desire to have imaginary intercourse, in preference to the risks of involvement with a real other person.

The author has adapted some of Heider's symbols for the description of interpersonal relatedness, and he uses these to good advantage in explicating such complicated transactions as "He thinks his wife thinks he supposes she loves him." Such transactions do exist, and it is a contribution to devise a shorthand way of depicting them.

The chapter on "existential position as a function of the action of others" treats of the ways in which a person is put into false and untenable positions by others' actions. He calls attention to the need people experience to have a place in *somebody's* world, and how people sometimes may be treated as if they did not exist. Paranoid delusions of reference, for example, may arise out of the suspicion that one is of no importance to anyone.

This book had a powerful impact on me, in part because its contents resonate so well with my interests in self-disclosure, authenticity, and psychotherapy. I regard it as required reading for anyone interested in existential approaches to psychopathology.

## Who Killed Vocational Guidance?

Ruth Barry and Beverly Wolf

*An Epitaph for Vocational Guidance: Myths, Actualities, Implications.*  
New York: Teachers College, Columbia University, 1962. Pp. 241. \$4.75.

Reviewed by DAVID V. TIEDEMAN

Both authors, Ruth Barry and Beverly Wolf, hold degrees from Columbia University, both have accumulated extensive counseling experience, both have worked in the graduate training program in counseling at the Board of Higher Education in New York City and Columbia, and both worked together on an earlier book, *Modern Issues in Guidance-Personnel Work* (1957). The reviewer, David Tiedeman, is a product of Harvard's Graduate School of Education and is now a professor in the same institution. He purports himself to be basically a psychometrician who has turned professional in an effort to see more closely and understand more clearly the problems of career and of existence. He sticks to the belief that ways can be developed through an advancing behavioral science to help people attain mastery of their existential problems.

THEY who would provide an epitaph for vocational guidance must be certain that: 1) vocational guidance is dead; or 2) vocational guidance ought to be dead and is slain in the reading of the epitaph. Barry and Wolf admit that vocational guidance exists but intend to slay it as they present their epitaph. This is why I dare ask in paraphrase:

"Who killed vocational guidance?"

"Not Barry and Wolf," say I.

*Epitaph for Vocational Guidance* had innocuous origins (Preface, p. v.)—the authors wished to provide a more satisfying textbook on occupations. In confronting that task, they discovered, as have some before them, that work emanates from the desires of the worker as well as from the expectations and reactions of those with whom he attempts to do business. As such, "occupations" represent the indivisible hopes of the



seller and demands of the buyer. Furthermore, they are a part of an ever-evolving structure of activity, as the worker may or may not be coming to realize.

The construction of occupation as the product of a developing purpose in action transpiring within coalitions of men formed to operate with people, ideas, and things for the advantage of the coalition makes it imperative that the practice of vocational guidance be set in a new linguistic frame. Barry and Wolf are aware of this imperative because they note, ". . . Vocational guidance methodology was and is predicated upon the outmoded assumption that information teaches, that advice and information-giving are functions of the counselor, and that vocational guidance can exist apart from 'personal' guidance . . ." (p. 9). Although the semantic form of the basic assumptions undergirding current vocational guidance is open to question, the allegation in itself is probably true. My difficulty in honoring *Epitaph* as a contribution, however, rests simply with my expectation that a book should represent a step in the development of an idea (which *Epitaph* is not) rather than a shaming of those who have acted in good faith with useful results despite inadequate grounds (which *Epitaph* is). There are more people than these authors seem to realize who are identified with the practice of vocational guidance but who nonetheless recognize errors in old ways. The tragedy of *Epitaph* may be that it creates defenses (and justifies attacks) that will temporarily hinder an already burgeoning modification, broadening, and upgrading of practice in guidance.

The major scholarly difficulty with *Epitaph* is not its tone; it is that the authors realize the need for developmental theory but do not provide a structure in *Epitaph* which initiates a developmental theory. Rather, Barry and Wolf let concepts of practice dominate their structure. Little wonder, then, that the tone becomes essentially destructive (as the title implies) and that occasional statements are erroneous and others represent overgeneralizations regarding the present condition. A modified practice must stem from or create a modified theory.

The principal structure of *Epitaph* is vested in only three parts: myths, actualities, and implications. Each of these three main structures is partitioned into sub-structures.

The myths of vocational guidance (which are considered in the order noted in parentheses) consist of belief in a single theory (1) which in its application is intended to enhance the realism of occupational choice (5) through consideration of test results (2) and information (4) about occupations which have been assigned to classes (3). In the presentation and criticism of these myths, Barry and Wolf unfortunately: 1) fail to distinguish theory from the steps of practice; 2) destroy technique without due regard for its indispensibility even in the desired developmental guidance; and 3) arouse guilt about reference to the imperative of realism in guidance when it is the anchor of existence.

"Actualities" delineate the problems which have arisen, and will probably continue to arise, from the greater aggregation and longer life of people, and the concomitant reduction in the amount of human energy required for production. These problems of education and development will have their repercussions in various habits, customs, and attitudes towards work. The maintenance of confidence during the change of requirements will certainly be a more necessary attitude towards self-in-situation in the future if purposeful action with accommodation is to be possible with a minimum of blame displaced inward or outward.

"Implications" turn out to be suggestions for modifying the practice of guidance to permit "existence" to displace "work" as the central concern in programs of guidance. Attitudes toward self-in-situation, which are the beliefs about existence which a person holds, are formed developmentally. Cognitive functioning will eventually become a central object of guidance theory as a result. The practice of guidance will become diffused throughout the school program. So, naturally, the counselor must change. He must become a professional and be trained as one. A counselor cannot help another accept his existential problem if the counselor re-

mains a technician. Acceptance of the existential problem cannot be conditioned. As *Epitaph* so rightly points out, the practice of technology in "helping" tends towards conditioning.

Have Barry and Wolf "killed vocational guidance" then in providing *Epitaph*? Not in my judgment. The need for developmental theory is noted but a developmental theory is not even initiated. Criticism is offered from the structure of current guidance practice rather than from the structure of (also current) developmental theory. The best the book can do is to arouse shame among those of us who practice. The book may well also arouse guilt, however, and lead to defensive reactions from many practitioners. Finally, the book is not a text on occupational information, as the authors wistfully hope; rather it is a series of essays on myths, actualities, and implications as its title announces.

And what of reform in vocational guidance? The techniques of vocational guidance are presently being modified and anchored in a growing theory of vocational development. The theory of vocational development is being joined with evolving theories of existential development. Finally, Guidance—as the science of purposeful action applied through education—is beginning to take shape. Truly professional training in Guidance could readily become available with the fusion of goal, theory, and adequately supervised practice which is now possible.

A lead exists when there is acceptance among the parties in which leadership is required. I trust Barry and Wolf can see fit to join us, not bury us.

## We're All Psychotic

Charles Berg

*Madkind: The Origin and Development of the Mind.* London: George Allen & Unwin Ltd., 1962. Pp. 277. 36s net.

Reviewed by ALBERT C. CAIN

The author, the late Charles Berg, was born in India, educated in Switzerland





## SLEEP AND WAKEFULNESS

Revised and Enlarged Edition

*By Nathaniel Kleitman*

The 1939 edition of this much valued work of reference has been in continuing demand, although recent research has outdated much of its material. The author's own revision incorporates significant new findings which led to the modification of his views concerning a basic rest-activity cycle; evolutionary changes in sleep, as well as in wakefulness; and separation of the concept of consciousness from that of wakefulness. A superb Bibliography comprises over 4,300 references, covering studies on animals and human beings, normal and pathological, factual and theoretical.

*Comment on the first edition:*

"... a refreshing scientific document in the midst of theory, superstition and suggestion ... comes nearer to being a scientific evaluation of all the facts heretofore accumulated than any other work on the subject thus far available."—*Journal of the American Medical Association*.

576 pages, index \$12.50



THE UNIVERSITY  
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Chicago and London



and, at the time of his recent death, was practicing psychiatry in London. He also served on the staff of the Tavistock Clinic, and worked with the Institute for Scientific Treatment of Delinquency. The reviewer, Albert Cain, is a clinical psychologist who received graduate education at the University of Michigan and remained in Ann Arbor as Assistant Professor in the Departments of Psychology and Psychiatry and as a member of the staff of the University of Michigan Medical Center. Among other activities, he teaches courses in child psychopathology and pursues the study of such processes as mourning and pseudoretardation.

THE PSYCHOANALYST David Eder once said, "We are born mad. We acquire morality, and become stupid and unhappy. Then we die." Such is Berg's central thesis, his banner: that man, madkind, in his inner life and general social behavior is essentially insane, psychotic.

"Madkind" is a collection of connected essays, originally intended for a larger work, but limited to its current form by Dr. Berg's recent death. Its announced task is that of comprehending man's psychic structure, customs, beliefs, social institutions, relationship to the universe, and evolutionary development—"the case history of a psychosis." Viewing our madness as revealed in parties, marriage, initiations, games, education, war, religion, politics, penology, etc., he presents its symptoms, family background, early development, therapy, and guarded prognosis.

Wherein our psychosis? Madkind's life is dominated by the irrational, driven by unconscious fantasies, haunted by rage-filled projections and a savage superego. Desperately seeking relief, we distort the expression of our biological drives, and in our need for psychic comfort substitute delusions for distress. Berg asks what more could be expected from man's ego, a still feeble, immature new evolutionary development. Savages, as almost everyone knows are psychotic; we are little removed from them. Children, as everyone knows (except those who treat child psychotics) are psychotic, and adults are acromegalic children. Thus our psychosis.

Berg's metaphor, while none too fruitful, presents little problem. More troublesome are repetitions, broadside generalization, "nothing but" approaches, contradictions, confusions of illustrations with evidence, tired barbs at religion, bizarre statements alongside acute insights, and applause for patients' appallingly articulate indictments of society.

But most significant is Berg's general approach, reflecting fixation to the least admirable aspects of pre-1930 psychoanalysis. Primitive behavior, dealt with in careless generalizations, is portrayed as at best quaint, more typically as a ghastly collection of menstrual taboos, hexed nailparings, and initiation rite mutilations. Similarly, society is conceived of as an individual, more precisely as a patient (bringing it within the clinician's competence). Society, its institutions analyzed as symptoms of pathological conflict resolutions, is of course, opposed to and suppressive of basic human drives. In brief, none of the last twenty years' rich understanding of the interplay between social structure, child-rearing practices, and personality development appears: virtually the only complexity permitted is hierarchical ordering of unconscious determinants of social phenomena. As was typical of and perhaps necessary for psychoanalysis of the early 1900's, non-clinical data is not to be learned from, but rather "applied to" or "demonstrated upon." Data, concepts, and references from the social sciences are ignored or most peculiarly selected. Recent theoretical, methodological, and empirical advances in psychoanalysis are equally ignored, excepting some unsteady use of Kleinian concepts. And even the early id psychoanalysis never insisted, as Berg does, that we are reality-avoidant, have no significant apparatus for objectivity and reality-testing, engage the environment only grudgingly, are deeply pained when our fantasy absorptions—which fill the only time we feel truly alive—are interrupted, and have barely a "vestige of sanity."

Even taken not as a scientific endeavor or applied analysis but simply as a slash at institutionalized unreason and an anguished plea for more loving, less guilt-inducing child rearing, this

book is deficient. It invites categorization, akin to the more poetic work of Berg's intriguing predecessor Groddeck, as a museum piece. But it disappoints most in being an unfortunate representative of the raw unrevisable virtues of the older, orthodox psychoanalytic understanding of man, so clearly needed now with personality theories increasingly anemic, with middleweight perceptual or cognitive elements being touted as central in the study of man, with what are surely psychology's neo-Dadaists earnestly questioning whether we need motivational concepts, and with the tall achievements of psychoanalytic ego-psychology showing early signs of diminished respect for their biological parents.

## Crossed Signals

Christian Astrup

*Schizophrenia: Conditional Reflex Studies.* Springfield, Ill.: Charles C Thomas, 1962. Pp. vii + 345. \$13.75.

Reviewed by H. E. KING

The author, Christian Astrup, is a psychiatrist now associated with the Gaustad Hospital in Oslo, Norway. He has gained varied clinical experience in Norway, Russia, East Germany, England and the U.S.A., and has published widely on schizophrenia and related phenomena. H. E. King, the reviewer, is a Columbia PhD, who taught at Columbia and at Tulane before becoming Professor of Psychology at the University of Pittsburgh School of Medicine and Chief of the Psychology Service at the Western Psychiatric Institute and Clinic. He is a long time student of physiological psychology and has been most intimately associated with experimental programs in cortical ablation, subcortical recording, electrical excitation and chemical stimulation of the brains of both animal and human subjects. He authored a 1954 book *Psychor*



motor Aspects of Mental Disease and has both contributed to and edited other volumes.

Now and again the reader can be transported, by means of a book, to some distant land and be made to see, for a moment, how it is there. This monograph by a young Norwegian psychiatrist gives rise to just such a feeling as it sets forth the details of a clinical and experimental investigation based squarely upon a conditional (conditioned) reflex approach to an understanding of the psychoses. In the first one hundred pages the author describes the basis for selecting a series of investigative techniques to be applied to schizophrenic patients, following an approach to psychiatric disorder pioneered by Pavlov, late in his life. This opening section of the book, reviewing the theoretical and experimental antecedents for what the author proposes to observe, should hold the fascinated attention of American psychologists as it draws together, in English, the more recent history of Eastern European approaches to the problem. More than a thousand references are cited and related to the investigation at hand. This bibliographic corpus, together with a special index of relevant Russian journals and congress reports, should be a welcome addition to both our scientific and medical libraries.

The plan of the experiment outlined by the book is straightforward. Astrup has applied a battery of special techniques to permit observation of the adequacy of function of "unconditional reflexes" and of the "first and second signal systems" among patients with chronic and acute schizophrenic disorders. His observations on the malfunction of Associations, Motor Conditional Reflexes, Autonomic Reactions and Defensive Reflexes are then related to clinical data, with concurrent notations on hereditary loading and patient response to ataraxic drugs. An interpretation of the findings is less simple. There is a language problem, made critical by the fact that the data are descriptive and supplemented by only simple statistics of contingency. The non-native English style of the writer, while reinforcing the exotic tone of the work, is

often difficult to penetrate. A more crisply edited text would have helped to focus attention on the subject at hand. Any critique must also attend to the vast and unfamiliar body of theory from which the working hypotheses have been drawn. Astrup works well within this theoretical superstructure and its view of "the higher nervous process" described by concepts such as "elective signaling irradiation," "shifting induced inhibition," "pathological excitation of inert subcortical structures" and "stagnant excitation." The reader's exceptions, where taken, often root beyond the investigator to the theory itself, to its vaguenesses or confusions, or to its alternate use of terms such as "nervous function" to mean a disruption of behavior or of physiology. The system selected to classify patient reaction-types is also difficult to follow. The author has adhered sensibly to a principle of maximum homogeneity within clinical subgroups, but has adopted a system for ordering behavior almost certainly unfamiliar to American psychopathologists. There is often a problem in trying to relate the descriptive findings to the two unfamiliar frames of reference.

The book should serve as a concentrated introduction to a unique and complex way of regarding the behavior disorders, and of experimenting with them, that is largely unknown to an American audience. The value of Astrup's findings can only be appreciated or evaluated within the context of their theoretical origin, at present "(little) known or understood in our midst," buried as much of it is in the Russian language (Gantt, the *Foreword*). As a report of an experimental-clinical investigation in psychopathology the book is orderly, difficult and far-reaching. As a point of view about which we might well learn more, whether to emulate or contest, it should serve as a remarkable guide.



*Let the imagination go, guarding it by judgment and principle and holding it in and directing it by experiment.*

—MICHAEL FARADY



## On Freud with Care

Reuben Fine

*Freud: A Critical Re-evaluation of His Theories.* New York: David McKay, 1962. Pp. vii + 307. \$6.95.

Reviewed by FRITZ SCHMIDL

The author, Reuben Fine, has a PhD from the University of Southern California and has been for fifteen years or more a practicing psychoanalyst. Presently he is Supervisor of Psychotherapy at Elmhurst General Hospital and keeps up his active involvement with the training of non-medical analysts in the New York area. The reviewer, Fritz Schmidl, is an affiliate member of both the Seattle and San Francisco Psychoanalytic Societies and is a frequent author for the *International Journal of Psycho-Analysis* and the *Psychoanalytic Quarterly*.

THE AUTHOR "undertakes to examine the whole body of Freud's thought, to clarify what he said, and to review his ideas critically . . ." (P. VII). He expresses the opinion "that the division of psychoanalysis into different schools is an historical error" (P. VIII).

Freud's theories are presented in historical sequence. Part I of the book deals with the "Beginnings of Psychoanalysis," Part II with "Id Psychology," Part III with "Ego Psychology." A brief Part IV titled "Retrospect and Prospect" concludes the book.

The organization of the book shows that Dr. Fine apparently shares Ernest Jones' conviction that "psychoanalysis . . . can be profitably studied only as an historical evolution, never as a perfected body of knowledge . . ." (Ernest Jones, *The Life and Work of Sigmund Freud*, Vol. I, p. XII).

The bulk of the book consists of a summarizing review of Freud's writings; many verbatim quotations from works published during Freud's lifetime as well as from letters published after his death are used skillfully to bring the difficult subject to life. The way in



which the author manages to present an enormous amount of material in but a few more than 250 pages is admirable. The bibliography is not only up to date but particularly useful because comments about some of the titles mentioned will help the reader select for further reading what will be of most interest to him.

As a critic of psychoanalysis Dr. Fine is careful and restrained. He feels that Freud himself would respect criticism as a natural development. The "critical re-evaluation" promised in the title of the book is mainly given in the form of a re-statement of Freud's theories rather than in any polemic way. Dr. Fine shows great objectivity when dealing with the main arguments of the so-called Neo-Freudians. Discussing, for instance, the difference between the biological and the cultural orientations he states that this is "one of emphasis rather than of fundamental position" (p. 89).

THE AUTHOR does not indicate for what kind of audience the book has been written. Although the style is lucid, readers not familiar with psychoanalysis may have considerable difficulty in understanding the material. The presentation of the historical evolution of Freud's ideas offers a kind of insight which could not be achieved in any other way. Yet the question has to be raised as to whether at this point a systematic presentation wouldn't be preferable. A physicist can understand the whole of Newtonian physics without any knowledge of the steps through which Newton arrived at his theory. Why should this be different in the case of Freud and psychoanalysis? It is fascinating to learn how Freud constantly worked on his ideas, changed some and added new ones. But such study is rather demanding of time and energy. Unfortunately many well educated people, including scholars and teachers, consider psychoanalysis and its language something "unintelligible to all but the initiate." (Merle Curti in a presidential address to the American Historical Association, 1955.) Ernest Jones' idea that psychoanalysis should be taught by way of presenting its historical evolution may be correct where the problem is one of teaching future psychoanalysts.

Persons, however, who do not intend to become psychoanalysts may shy away from the difficult task of learning the historical way.

In a number of instances Dr. Fine summarizes psychoanalytic ideas in a way that they are likely to be misunderstood. One page 225, e.g., Freud's theory of anxiety is explained as follows: "It now appeared that the various types of anxiety were all forms of separation anxiety; in infancy, the fear is of the loss of the mother, in the Oedipal stage it is the fear of castration (a separation from the genitals) . . ." The reader not familiar with the psychology of the unconscious is most likely to consider the idea that castration anxiety is a form of separation anxiety, at least far fetched. Dr. Fine quotes correctly from Freud's "Problem of Anxiety." Yet only when the reader would look up the trend of thought which Freud followed (S.E. Vol. XX, pp. 138-139) could the sentence in Dr. Fine's summary assume understandable meaning. In other instances, e.g., where the author mentions Erikson's work (p. 67) he quotes the word "epigenetic theory," but leaves it completely to the reader to inform himself of the main trends in Erikson's work. Maybe these shortcomings could be eliminated in a later edition of Dr. Fine's book. Its conciseness is certainly an asset, but some elaboration would make it more useful.

As the book is now it can be highly recommended as the basis of an academic course in psychoanalysis taught by a competent teacher who can help the student where the presentation in the book is too sketchy to be understood.



*Most discoveries have been made on soil that has already been prepared to some extent, and no matter how revolutionary a scientist may have been, his work is only a more or less direct continuation of the work of his predecessors.*

—R. TATON



## Aging Abroad

E. Woodford-Williams and A. N. Exton-Smith (Eds.)

*Aspects of Geriatric Psychiatry, Haematology, Nutrition and Cancer in Old Age.* Proceedings of the Third Meeting of the European Clinical Section of the International Association of Gerontology, The Hague (Netherlands), September 13th-16th, 1961. Basel and New York: S. Karger, 1962. Pp. IV + 244. \$10.60.

Reviewed by NATHAN W. SHOCK

Both editors are British physicians and are well known in gerontological circles. E. Woodford-Williams, is in charge of the geriatric program in Sunderland, England, and has made many notable contributions to knowledge about age changes in renal function, cardiovascular performance, and body composition. A. N. Exton-Smith, in addition to conducting surveys of the socio-economic and health status of elderly people living in the community, has published *Medical Problems of Old Age* (1955). The reviewer, Nathan W. Shock, has been Chief of the Gerontology Branch of the National Heart Institute and of the Baltimore City Hospitals since 1941. He has published many experimental articles on the physiological and psychological aspects of aging and has served as editor for a number of symposia on aging. He is author of *Trends in Gerontology* (1957) (CP Oct. 1958, 3, 309f.) and *A Classified Bibliography of Gerontology and Geriatrics* (1963).

THIS volume contains the papers read at the third meeting of the European Clinical Section of the International Association of Gerontology. The editors, Dr. E. Woodford-Williams and Dr. A. N. Exton-Smith, have been closely associated with the developing fields of gerontology and geriatrics for the past 15 years. It is a pity that they could not have been more vigorous in bringing their wealth of laboratory and clinical experience to bear on the edit-



ing of many of the individual papers reported. But the life of an editor of a symposium is not an easy one. Some contributors can be very stubborn. Although the volume contains papers on a number of clinical problems of old age, such as hematology, nutrition and cancer, psychologists will be primarily interested in the papers on geriatric psychiatry. These papers offer an overview of current European conceptions of the approach to mental disease in the aged. The traditional apathy toward treatment of mental diseases in the elderly is apparently disappearing. A number of the papers indicate a belief that, although brain damage often occurs in the elderly, depressive states are treatable, even at advanced ages. However, successful treatment requires special facilities. In some instances, reorganization of the staffing in the mental hospital, with participation of nurses, social workers, psychologists and physicians in diagnostic and assessment procedures, contributes to the development of the hospital as a "therapeutic community." The importance of diagnosing and treating physical diseases as an aid in improving mental states in the elderly is stressed. Both sedative and stimulating drugs are useful in the treatment of mental disease in the elderly, but effective long term therapy depends heavily on the social climate to which the aged person is exposed. Hospital treatment is often an essential first step, but long term success depends heavily on effective coordination of community resources with the hospital based program. Although some of the authors present experimental and laboratory data, most of the articles represent clinical impressions. Some of the reports deal with surveys of elderly people living in the community, but most of them are concerned with the aged in institutions and hospitals. Hence, few conclusions can be drawn about the characteristics of the many elderly people who do not require institutional care. With this warning the volume deserves the attention of workers in the field of gerontology.



# PSYCHOLOGIA

AN INTERNATIONAL JOURNAL OF PSYCHOLOGY IN THE ORIENT

*Editor: Koji Sato, Kyoto University*

VOL. 6, NO. 1-2

MARCH-JUNE, 1963

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ORDERS AND CORRESPONDENCE TO:

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DEPARTMENT OF PSYCHOLOGY, KYOTO UNIVERSITY  
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# The Oldest Wonder Drug of All

David J. Pittman and Charles R. Snyder (Eds.)

*Society, Culture, and Drinking Patterns.* New York: Wiley, 1962. Pp. ix + 616. \$9.75.

Reviewed by EDITH S. LISANSKY

Both editors are sociologists with a long history of research involvement with alcohol and alcoholism. Pittman is now Associate Professor of Sociology at Washington University and is presently a principal investigator on a project involving a treatment and referral demonstration. Snyder is now Professor of Sociology at Southern Illinois University. Edith Lisansky, the reviewer, did graduate work at Columbia under Klineberg and later at Yale where she received her PhD degree. She has worked for the Connecticut Commission on Alcoholism and has done extensive research at the Yale Center of Alcohol Studies. Although she is best known for her publications on alcoholism, at the present time she is deeply involved with children, participating in a research project dealing with early reading.

ALTHOUGH anyone who has ever attended a meeting of psychologists know that they are not disinterested in the uses of alcohol, their apathy about research problems in alcohol can be described as monumental. Why do sociologists find the problem so much more enchanting? Of the 40 contributors to this volume, virtually all—except for a few ecologists, anthropologists and a stray journal editor—are sociologists. Psychologists, whatever the reason for their lack of interest in alcohol studies, are conspicuous here by their absence.

This is the fourth work sponsored by the Study of Social Problems; of the three earlier books, one dealt with community organization, another with mental health and a third with sexual behavior. Like all edited collections, this volume contains a certain amount of

rehash—included, one assumes, because the material has its place in the overall scheme and is new to some readers. Compared with the average rehash score for such volumes, however, the present book comes off very well. Two thirds of the chapters, many with new data, were written expressly for the book. Many of the data were empirically gathered, some are theory-based. Kettil Bruun's report is based on small group theory, for example, and Peter Park's experimental study of role deviation derives from Talcott Parsons's theory.

The editors do a first class job of organizing the book into sections; there is one section on drinking behavior, a long section on alcoholism, (one of the ABC's of alcohol studies is the distinction between drinking per se and alcoholism), and a final section on social control of alcohol use. Each section begins with an introductory note giving a summary of chapters to follow and pointing out their relatedness to each other and to other work. These notes serve beautifully the purpose of integrating a very wide variety of material and approaches (not all equally valuable or fruitful, one might add). A minor criticism: Earl Rubington's interesting chapter on the social organization of the Skid Row subculture is placed appropriately under "Class and Status" but an editorial comment about the accepted distinction between alcoholics and the chronic drunkenness offenders of Skid Row would have made clear why the chapter is not in the section on alcoholism.

With the proliferation of research papers in psychology and the limited journal space, this reader has watched,

and with regret, the disappearance of "Background Literature." One of the admirable aspects of the present collection is the concern of many contributors with ideas and hypotheses generated several decades back. For example there is the longstanding concern with the *definition* of alcoholism, a concern echoed and renewed here by Mark Keller, editor of the *Quarterly Journal of Studies on Alcohol*. And two major contributions made years ago by E. M. Jellinek, one a formula for estimating the prevalence of alcoholism in a given community or time and another the "phases" of alcoholism, are here examined, tested and modified in several different chapters. In addition, two earlier works reviewed in the light of relevant new findings are Horton's 1943 cross-cultural study of anxiety and drinking in different societies, a study often cited in support of the view that alcohol reduces fear, and Dollard's description of the drinking mores of the different social classes. With the latter, it turns out that Dollards' descriptive hypotheses holds up very well except for the lowest class levels, confirming again, among other things, that behavioral scientists' experience with and knowledge of the poor is all too limited.

I would not agree with the editors that this is an "interdisciplinary" book. It is primarily a sociological book. It would enrich a discussion of drinking patterns if psychologists could contribute some information about the effects of alcohol on behavior, feeling and moods, or perhaps could add something to a theoretical discussion of etiology or to a consideration of treatment of alcoholism. The book is written almost exclusively by sociologists because they have made alcohol their problem and have produced interesting and important contributions.

BUT there is a kind of cultural interchange between fields of human endeavor, and so it happens that we frequently find the same term used in the overlapping disciplines of psychology and sociology. Take *ambivalence* for instance.

*Ambivalence* was first used in connection with attitudes toward alcohol in a



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## ☐ DIGITAL COMPUTERS IN RESEARCH: An Introduction for Behavioral and Social Scientists

By BERT F. GREEN, Carnegie Institute of Technology. Lincoln Laboratory Publications. 352 pages, \$10.75.

The purpose of this volume is to acquaint behavioral and social scientists with the use and operation of computers. The book combines an introduction to programming with an account of the

applications of computers to research programs. A conscious attempt has been made to keep the mathematics at an elementary level, and only basic algebra, trigonometry, and statistics are prerequisite on the part of the reader.

## ☐ HANDBOOK OF MENTAL DEFICIENCY: Psychological Theory and Research

Edited by NORMAN R. ELLIS, George Peabody College for Teachers. McGraw-Hill Series in Psychology. Available in November, 1963.

The first definitive presentation of all the significant theoretical approaches to the study of mental deficiency. Its purpose is to assess the status of behavioral research and theory in the field. The material is divided into two parts: Part I is devoted to the exposition and evaluation of theories of defectives' behavior. Part II summarizes the literature pertaining to the area of mental deficiency and evaluates the available data, pointing out the relevant aspects as well as the shortcomings.

## ☐ THEORY OF PSYCHOLOGICAL MEASUREMENTS

By EDWIN GHISELLI, McGraw-Hill Series in Psychology. Available in January, 1964.

Provides knowledge, at the level of an elementary course, of the problems, statistical techniques, and theoretical concerns basic to psychological testing and measurement of mental traits. In this text almost every formula developed is presented fully, and as simply as possible, to aid the student in understanding. The treatment of norms and standardization of scores, correlation, reliability, and validity of measurement are presented comprehensively and emphasis is given to mathematical models and their uses. Integrated illustrative problems are included.

## ☐ FIELDS OF APPLIED PSYCHOLOGY

By ANNE ANASTASI, Fordham University. Available in January, 1964.

A comprehensive and integrated picture of the professional activities of psychologists in business, industry, advertising and marketing, education, clinical practice, law, government, and the military. The approach emphasizes methodology, how psychologists operate in the various fields of applied psychology. Each topic is discussed and illustrated by relevant applied research. The text is very up to date.

## ☐ ALCOHOL AND CIVILIZATION

Edited by SALVATORE P. LUCIA, University of California, San Francisco. McGraw-Hill Paperback Series. *Off press.*

Taken from a symposium at the University of California School of Medicine in 1961, this volume presents an objective view of the historical psychophysiological and culture roles of alcohol in society. Major areas covered: the effects of alcohol on the body, alcohol in our society, and the interdisciplinary viewpoints of the symposium speakers. Available only at your local bookstore.

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paper by Abraham Myerson in 1940. Myerson, a psychiatrist, qualified his use of the term and gave the label "social ambivalence" to mixed attitudes of approval and disapproval. It is in this sense that Edwin M. Lemert in his chapter on "Alcohol, Values and Social Control," uses it: valuations of alcoholic beverages become "polarized" because people perceive alcohol as a behavior modifier and recognize that such behavior modification may be "... socially and personally destructive as well as socially integrative." Ozzie G. Simmons, in a chapter on Peruvian drinking behavior, uses *ambivalence* to describe the attitude that drinking itself and even drunkenness are good but, "... it may bring shameful consequences in its wake." It turns out, in an editorial comment, that social scientists also have "ambivalent attitudes" which stem from a tradition of abstinence (the editors and the reviewer obviously move in a different circle of social scientists). And there is, too, Park's concept of "role ambivalence" which characterizes young adult problem drinkers, i.e. incipient alcoholics; this is their "... inability to play decisively the adaptive role, or any other role, for that matter."

It seems then that *ambivalence* refers to the subjective experience of being in a conflictful situation or to mixed favorable-unfavorable attitudes. "Role ambivalence" carries it further: such attitudes result in a paralysis of choice or decision about roles. But there is a clinical definition, too, and this refers to love-hate polarities in affectional relationships. Perhaps we should all agree among ourselves how and when to use the term. As things stand, *ambivalence* is a popular word in this book and it is, therefore, overextended. Marshall B. Clinard, in an engrossing chapter on public drinking houses, uses the term to characterize the discrepancy between the acceptance and popularity of bars and taverns (someone has called them the counseling centers of the slum areas) and the disregard and rejection of its obvious small-group study potential by sociologists. That's *ambivalence* too?

It is difficult to judge just how much interest psychologists might have in a collection of sociologists' thoughts in a

problem area such as drinking patterns and alcoholism. As the editors indicate, perhaps the book could be used as supplementary reading for courses on deviant behavior or in social psychiatry. I suggest that a variety of psychologists interested in human and social problems might find it instructive to skim or even read the book. Are psychologists inhibited in alcohol studies because of methodological difficulties or are they, as Pittman and Snyder suggest, ambivalent?

## Male Homosexuality

Kurt Freund

*Homosexualita u muže (Homosexuality in the Male)*. Prague: State Publishing House, 1962. Pp. 274, 54 tables, 15 graphs. 32,50 Kčs.

Reviewed by J. HOSKOVEC

*The author, Kurt Freund, is a research worker in the experimental psychiatric laboratory at Charles University, Prague, Czechoslovakia. The reviewer, J. Hoskovec, is in charge of the archives of diagnostic materials at the Institute of Psychology at the same university. He has had the opportunity to become acquainted first-hand with the work of Dr. Freund and has been able to discuss with him the problems considered in the present book. The present review is one of a number arranged for and edited by CP's energetic consultant on Slavic books, Josef Brožek.*

DR. KURT FREUND'S monograph represents a significant contribution to the technical literature on the motivation of human behavior. The core of the work is an investigation of the psychopathologic problems of homosexuality in the male. But neither the biological nor the ethical and sociological aspects are neglected.

The author designates as "homosexual" persons with homoerotic preference. He speaks of "bisexuality" in

cases of approximately equivalent erotic motivation.

The characteristic traits of male homosexuals were extracted from a sample of 222 patients, examined at the Prague Psychiatric Clinic in the course of the past ten years. The sample seems to be sufficiently representative of the population of homosexual men who seek a doctor's advice in those European countries where homosexuality is punishable by law.

The characteristics of male homosexuals and their mutual relationships are presented in detail. The age preferences, femininity in the male, the preferred mode of intercourse, and narcissism are given special attention.

Homosexual males who prefer grown up partners only rarely represent danger to adolescents and probably none to children. The results of the study show that feminine homosexual males differ markedly from the non-feminine ones. The extreme cases are characterized by a strong feminine identification, by a preference for the receptive anal intercourse, by an inclination towards virile and muscular partners of ripe age, and by narcissist tendencies.

The use of projective tests in the diagnosis of homosexuality is critically evaluated. The Thematic Apperception Test and the Make-a-Story Picture Test proved to be valuable. On the other hand, it does not seem fruitful to use the tests of Rorschach, Szondi, and Blum. Masculinity-femininity can best be determined by certain questions presented to the patient in the course of clinical examination and related to the preferences for girls' company, and for girls' games and toys in childhood.

The author presents in detail his psychophysiological examination method involving the registration of volume changes in the penis of persons looking at pictures of erotically impressive men, women and children of both sexes. Freund's method differentiates satisfactorily between homosexual and heterosexual males and—to a certain extent—is suitable for objective determination of age preferences.

THOROUGH attention is paid to the relation of homosexuality to other psy-



chopathologic syndromes: neuroses, psychopathies, and psychoses, especially paranoia. The relation of homosexuality to psychosis has been presumed by many authors, particularly the psychoanalysts. The same is true of feminine identification. Clinical experiences gained so far seem to point to the fact that feminine identification in the male is conspicuously associated with paranoid schizophrenia. Psychoanalysts usually regard feminine identification in the male as a symptom of "latent" homosexuality, in Freud's sense. The author is critical of this view, as not sufficiently supported by observed facts. According to his own findings, in the patients of this type we cannot expect any marked homoerotic tendency, at least not in terms of physiologic arousal demonstrable by the author's plethysmographic method.

The observation of a strikingly high incidence in homosexual patients of statements concerning alcoholism in the father is corroborated; on the other hand, Freund's results do not corroborate the presumption that parental deprivation in childhood by death or divorce plays a significant role in the genesis of homosexuality. Some cases of homosexuality may be traced to cerebral disease, and trauma. The connection between severe cerebral injuries and the genesis of exhibitionism and fetishism would support such a view.

The author presumes that in both homosexuals and heterosexuals it is not only the preferred sex but also the non-preferred one which arouses sexuality, even if to a lesser degree. A stress situation (prison, ship, etc.) can accentuate the erotic appetite of the normally non-preferred sex and actualize homosexual activity.

According to Dr. Freund, there is no proof of the efficiency of any known treatment of homosexuality. It seems that the therapist may help the patient to achieve heterosexual adaptation without being able to exchange the homoerotic preference for a heteroerotic one. It appears probable that all the therapeutic measures, considered to be efficacious in the case of homosexuality, depend on a common principle: discouragement of homosexual

activities and encouragement of heterosexual activities.

The results of psychotherapy of male homosexuals do not justify that everyone should submit to it. But treatment should be made available to those wishing to achieve a heterosexual adaptation. The results, however, are still too uncertain and cannot be used as a substitute for a more radical intervention in pedophilics. A homosexual person who becomes heterosexually adapted continues to be in need of help, and it may be necessary that he have permanent psychotherapeutic guidance at his disposal.

The closing chapter covers the problem of the homosexual's position in society and the legal implications of

homosexuality.

The author cites some five hundred references; of these, more than half are Anglo-Saxon, one third German. Over thirty items are original Czech studies. The author's approach to literature is critical: the works in which he has found substantial methodological shortcomings are either omitted or the limitations are pointed out.

The lack of summaries in the world languages is certainly a major shortcoming of this valuable monograph. In the reviewer's opinion, its translation into English would be fully justified.

*This review was prepared in the frame of activities supported by the National Science Foundation grant G19469, awarded to Dr. Josef Brozek.*

## Epochal Psychology

Zevedei Barbu

*Problems of Historical Psychology.* New York: Grove Press, 1960. Pp. vii + 222. \$1.95

Reviewed by WILLIAM STEPHENSON

*Zevedei Barbu is presently a lecturer in social psychology at Glasgow University. Formerly he was Lecturer in the Sociology of Civilization at the University of Cluj in his native Rumania and was also at various times his country's Director of Cultural Relations, Minister of Foreign Affairs and Undersecretary of State. William Stephenson, the reviewer, is now Distinguished Research Professor at the University of Missouri. Once an assistant to Charles Spearman in London and for some years Reader in Experimental Psychology at Oxford University, he came to this country to hold visiting professorships at the Universities of Chicago, California and Washington before settling down in Missouri. Best known for his invention of Q-methodology, his involvement in methodological problems is of a very general nature and includes matters of historicism. Hence his interest in Dr. Barbu's work.*

THIS is the most interesting two dollars worth of psychological writing (except for Alex Comfort's *Darwin and the Naked Lady*, which is also English and also psychoanalytical) this reviewer has read in recent years. Mr. Barbu is lecturer in Social Psychology at the University of Glasgow, Scotland. Born a Rumanian, his English is a recent acquisition—but it is hard to believe this, and one can only compliment him on a powerful, profound piece of work, ponderously but wonderfully written.

The concern is with historical psychology, that is, with the way in which different epochs in history have influenced the psychological make-up of peoples. It is still apparently a problem amongst historians as to what comes first, the egg of personality or the chicken of history. History itself can only be understood, say some, for example, a Lamprecht or a Namier, by studying the psychology of history's



protagonists. Huizinga, on the other hand, would have nothing of such nonsense—historical events he could readily prove “thunder through all my psychology, which at best would reflect how the characters reacted to fate.” But English historians even today are taking sides about what comes first, the paranoid character of Hitler, or the social conditions of a demoralized Germany. The historians clearly have troubles.

Is it not strange, in passing, that Huizinga, so notable a scholar, so wise a man, could not accept psychoanalytic doctrine as worth even a moment's notice in his cultural history? Barbu is all psychoanalysis. His conception of historical psychology is clearly not Lamprecht's, nor is it Huizinga's. Huizinga dealt with the common people of all social classes and groups, and produces a *culture* psychology. Burekhardt's concern, by contrast, was with the lives of the *great*. Barbu's essay, quite different from both, deals with abstractions, with psychoanalytical *models* of men—as in a toy-maker's shop. But the models are fascinating enough. And, as Barbu can always say, he is merely indicating problems, not solving any.

AFTER a review of the historical psychology of perception, with Febvre's well-known studies to introduce matters, Barbu enters at length into the development of Greek and 16th Century English characters, of Greeks at the time of Pericles, and of Shakespeare's England. The results are profound models of characters in classical psychoanalytic terms, but with social-psychological interactions thrown in to offset Freudian reductionism. Thus, he remarks about Puritanism that one can discern in it the beginning of a new personality structure “based on a new balance between the individual and society,” which produces the modern Englishman as a “practical sentimentalist and a law-abiding individualist.” The personality movement in the 16th Century, Barbu argues, was “toward inner sources of anarchy,” because of this there arose, in England, an attitude of tolerance to and acceptance of this anarchy, a tendency to “yield to instinctual drives,” to accept the *id*. The Elizabethan spoke

with approval about the human individual's (not the church's or state's) need for acquisition and aggression, for acceptance of self-interest, and for inconsistencies. Human ‘appetites’ were given positive values, helped by the twin psychoanalytic mechanisms of affect displacement and rationalization. The early Englishman divested himself of the primary emotional significance of his *id* impulses, projecting them outside himself—giving rise not to religious asceticism born of repression, but to secular asceticism born of displacement. It was an Englishman who first described rationalization: it was the English, Barbu continues, who found how to use this mechanism to their social advantage. By social ‘drill’ the English have built up loyalties, regular habits of conduct and similar character qualities.

So we could continue. In such a manner Barbu outlines the Greek and English characters in remarkable detail, summing it up as, in the 16th Century, a breaking away from the suzerainty of *id* and *superego* of Medieval Man (where God and Satan, good and evil, guilt and indulgence were constantly in turmoil, the ego being lost in the duality), to a certain identity and continuity of *id*, *superego* and *ego* in the case of the Englishman: “the English are the first and most successful amongst modern nations to create a mental structure in which conscience and instincts are not fundamentally opposed. Thus they pushed the history of the mind a step forward from its medieval stage.”

Barbu offers no history of psychology as such to support these wonderful toys. Yet one cannot forbear from remarking that in recent English psychology, in Spearman's days, two great factors were “discovered”: one was *g*, the central intellectual factor, and the other was *w*, for “persistence of motives,” “consciousness of planned action,” in a word, *character*; and Spearman gave *w* pride of place in his system. Peculiarly English, factor *w* was a mystery to American and European psychologists—and still is. Such is a little morsel of history that would gladden the heart

of a Huizinga, as no doubt it does a Barbu too.

The paperback is warmly welcomed, with a hope that it might raise interest in American universities in a much neglected topic, the history of psychology; historical psychology is infinitely more interesting, however, even as high speculation.

## Scatters, Piles and Slabs

Louise Bates Ames and Frances L. Ilg. Foreword by Margaret Lowenfeld

*Mosaic Patterns of American Children*. New York: Harper, 1962. Pp. v + 297. \$9.50.

Reviewed by AUSTIN E. GRIGG

The authors, Louise Bates Ames and Frances L. Ilg, are well known to psychologists and others through their long association with Arnold Gesell and with the Gesell Institute in New Haven. The reviewer, Austin Grigg, is a Virginian by birth, rearing and location, and one who is likely to maintain that any two places in that state mutually surpass one another in quality. He did his early graduate work at the University of Richmond, then went to the State University of Iowa for his doctoral work in the clinical area, then after a couple of years of teaching and research at the University of Texas, returned to the U of R where he serves as an Associate Professor, works regularly with Psychological Consultants, Inc., moves ahead in the preparation of a book on abnormal psychology and keeps alive his willingness to work occasionally for CP and its readers.

THIS is a book in that tradition which holds age to be the grand independent variable. The book studies the effects of age on the various ways of constructing patterns with the Lowenfeld Mosaic Test. The Lowenfeld Mosaic has been around for twenty-five years, but has never been studied as painstakingly as in the present volume. The



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test consists of a box of 456 plastic objects, in five different shapes and in six different colors. The subject is asked to make something with the pieces, anything he wishes. There are classifications for the kinds of designs that the subject constructs, but no systematic scoring scheme has been developed, except for frequency counts of the number of reds employed, the number of triangles used, the number of different colors and shapes.

Those clinicians who draw inferences from mosaics are usually enthusiastic. There is enough uniqueness in the constructions to suggest that the individuality of the patient has been tapped. Also, there appear enough consistencies to arouse the clinical hope that the mosaics will follow developmental sequences, or nosological models, or both.

The authors, famous as child experts, are old guard Mosaicnosticians—they attended the "First American Mosaic Conference held in Washington on May 13, 1950" (p. 67). Their rich experience with the technique—it hardly qualifies for the term "test"—convinced them that the Mosaic provides considerable information about personality and also provides clear cues as to the child's developmental level. They have found that the Mosaic is especially useful as a procedure to evaluate school readiness.

Briefly, their study of 25 children of each sex at 2 years of age and 50 girls and 50 boys at each yearly age from years 3 through 16 shows that Mosaic patterns follow a sequence as the subjects' ages shift. As the child matures, Mosaic responses change from scattering pieces singly or dropping them into little unstructured piles, to ugly "slabs," through vague geometric designs, then to well defined geometric patterns, and eventually to designs that represent pictures or scenes.

So faithful are the authors to their examination of shifts in Mosaic patterns with age, that, as they admit, "the very thing which the Mosaic Test was set up to reveal—unique personality patterns in the individual subject—is scarcely mentioned in the present volume . . ." (p. 242). But the book provides complete summaries of the literature on the technique so that the readers can learn

some of the kinds of things that the Mosaic is alleged to tell about personality. This is not fully satisfying, since the authors are quite frank about the generally negative results in the literature in regard to the validity of the test as an assessment of personality variables. Since the technique has not won an extensive bibliography as yet, there is some foundation for the optimistic stance that this is still a poorly researched gadget and that it is too early to have another test burning. As a matter of fact, Ames and Ilg have presented here impressive evidence that should gain a stay of execution. (Clinicians may be heard cheering from the galleries. We don't like to see a technique die.)

A major portion of the book is devoted to detailed descriptions of the Mosaics at each age level from two through 16 years. Sometimes this format is frustrating to the general reader who would like to ask, "What does it all mean?" It must, of course, mean something more than simply that the child's age has changed. The authors are keenly aware of this, but they regard the study as a "beginning" and they are content to tabulate shifts in choice of color, in choice of shapes of pieces, in types of designs. Without norms such as one finds in this book, it would be (and has been in the past) easy to confuse age trends with individuality. In addition to the tabular summaries, there are qualitative summaries, "to us, one of the outstanding contributions of this study" (p. 12). These qualitative summaries are comments about the designs, as for example this comment about those by 12 year-olds (p. 155): "There is a tendency toward somewhat shapeless perseveration—some subjects just build on and on. Also, girls' products, though no longer separate and scattered, have a marked tendency toward being large and spread out."

The authors also search for sex differences—and find them.

Prior to this book there had been little in the literature about Mosaic characteristics of the specific age levels, and even less about changes associated with age. This is a descriptive report, a book of tables, graphs, 191 Mosaic color-plates, and verbal elaborations.

Unfortunately, the sample of children studied was above average in intelligence and from the top two socioeconomic levels, mostly from Level I.

The major impression is that now we may really understand exactly how Mosaic patterns alter as age changes. It would have added to the book if the authors had drawn from their rich and penetrating insights into child development and had discussed what the Mosaic changes most likely reflect insofar as personality and cognitive functions are concerned. Perhaps this will make a future publication. It certainly should be communicated.

## Readiness for Readiness

Newell C. Kephart

*The Slow Learner in the Classroom.*  
Columbus, Ohio: Charles E. Merrill, 1960. Pp. iv + 292.

Reviewed by SADIE AARON

The author, N. C. Kephart, received his PhD in the area of child psychology at the State University of Iowa and has had a varied career in both research and service before and during his nearly 20 years in the Department of Education and Psychology at Purdue University. Sadie Aaron, the reviewer, came to Houston, Texas, in 1929 to become that city's first school psychologist. She has had such a busy and effective career that any person who knows anything about the Houston School System could praise her at length. She retired in 1961 but does not seem to have slowed down very much; she has more time to enjoy her grandchildren but, among other things, engages in private practice with children and young people with learning problems.

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their respective grades. Conscientious teachers spend countless hours trying without success to teach these youngsters their assigned tasks. Why do these seemingly normal children fail to learn? What are their specific difficulties? Can they be successfully taught? If so, how?

The author of this book ably answers these questions for one group of such children; namely, those who enter our schools lacking in the basic perceptual-motor skills. "They become slow learners in the classroom." According to the author, if such children are to be taught successfully in school, these pre-school learnings which underlie the school readiness skills must be supplied first. The earlier in the child's school career these shortages can be identified and overcome, the sooner the child can learn to do the school's assigned tasks. Thus, the author plans his book primarily for kindergarten and primary teachers who have the task of teaching these slow learners. However, this book will be equally useful to school psychologists who work with teachers in the solution of children's learning problems.

The book is divided into three closely related parts. Part I presents an excellent discussion of the development of the perceptual-motor abilities in the child from birth to approximately age seven.

Part II is devoted to the description of the author's Perceptual Survey Rating Scale. Its purpose is to reveal the areas of weakness in the child's perceptual-motor development. The series of tasks or performances in the scale are arranged in accordance with the developmental stages of perceptual-motor abilities as described in Part I. The author reports that "the tasks have been designed for and clinically investigated with children six to nine years of age." This age group was chosen because "the identification of readiness problems is more critical when the child begins school."

The child's performance is evaluated in terms of inadequate performance for each task on the Scale. The observer is greatly aided in making these evaluations by the author's criteria concerning the characteristics of adequate and

inadequate performance of each given task. The purpose of each task is clearly stated. The appropriate training activities for each of the revealed weaknesses are also suggested.

Part III contains the descriptions of the training activities which are effective in mastering the corresponding pre-readiness skills set up in the Rating Scale. However, this section contains more than a compilation of good teaching techniques. Each training activity is broken down into its levels of difficulty and is described in its appropriate psychological setting. More intelligent and purposeful teaching should result when the teacher's understanding of each training activity is thus enhanced.

This reader regretted two omissions from this very informative book; namely, more information concerning the validation of the Perceptual Survey Rating Scale and the provision of a few selected case studies to demonstrate its use. However, these omissions tend to be minor when one considers the over-all importance of this book. It is the first book, to this reviewer's knowledge, to deal so effectively with the problem of lack of readiness in the beginning grades in relation to difficulties in basic perceptual-motor learning.

## A Step Toward Survival

Walo von Greyerz. Preface by Tage Erlander

*Psychology of Survival: Human Reactions to the Catastrophes of War.* New York: Elsevier, 1962. Pp. vii + 99. \$3.75.

Reviewed by E. PAUL TORRANCE

*The author, Walo Von Greyerz, is a Swedish physician who, since 1939, has been officially concerned with civil defense, first in the city of Stockholm and more recently as Chief Medical Officer for Civil Defense in the whole of*

*Sweden. The reviewer, Paul Torrance, is presently Director of Educational Research at the University of Minnesota where he, with his staff of young researchers, is concerned with the identification, development and utilization of creative talent. His recent publications, including Guiding Creative Talent and Education and the Creative Challenge have been in this area. His qualifications for reviewing the present book, however, were gained in the six years between 1951 and 1957 when he served as Research Director of the Survival Research Field Unit of the Air Force Personnel and Training Research Center. He has worked before as a willing and able reviewer for CP (CP, April 1963, 8, 164).*

THIS thin book contains a rather remarkable distillation of what can be done with the accumulated knowledge available concerning human reactions in situations which threaten survival. The author presents no research data, no description of the process by which he reached his conclusions, but occasionally, in making a point, refers to some research result. The scientist who is searching for information about the psychology of survival will be greatly disappointed. Nevertheless, as Henry Murray writes in a kind of foreword to the book, von Greyerz has given us a "concise, lucid and rational account . . . of the predictable consequences of modern weapons used in different degrees of intensity and circumstances."

Apparently, this little volume was prepared in order to provide a wide range of readers information about the types of stresses and sufferings to be encountered in disasters and about possible ways of reducing the devastating effects of an enemy attack. Tage Erlander, Prime Minister of Sweden, states as much in his preface, maintaining that the more general dissemination of such knowledge is certain to lessen the risk that we would become paralyzed under enemy attack. Such a conclusion would certainly be supported by this reviewer's research on this problem. At least, the possession of information about what to do in survival situations increases confidence in one's ability to



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cope with such situations.

More specifically, the author states his aim as being one of "showing our most probable psychological reactions when faced with the dangers of war and with war itself and to suggest what measures can be taken to increase our capacity for psychological resistance." He deals with the problems of reorganizing a society into a war society, the reorganization that a government must make in bringing about preparedness, the emotional disturbances created by the onset of the emergency, the "deep-going" adjustments of the whole body of society, periods of isolation, fear and terror, reactions after the danger is over, psychological derangements in children, accustomization and sensitization, and the like. He offers suggestions about what can be done about all of these problems. In conclusion, he urges that a people strengthen during peacetime the capacity for psychological resistance. In addition to knowledge and skill relevant to the job which one must do in an emergency, von Greyerz sees as essential skills in thinking independently, inner security, and a philosophy of life which holds up under the strains of everyday life.

Apparently the conclusions presented by von Greyerz were reached in the process of studying the civil defense needs of Sweden and the civil defense practices in England, Denmark, Norway, Finland, Germany, France, Czechoslovakia, Switzerland, Italy, Canada, and the United States. He gives evidence of familiarity with some of the better known studies of the psychology of stress in most of these countries but apparently he is not familiar with the important work of the Disaster Research Group in the United States' National Academy of Sciences and National Research Council. He lists none of this group's publications in his four-page bibliography of suitable literature for further reading, does not mention any of it in the text, and makes no use of some of the most important results of this research.

In spite of the fact that von Greyerz has packed a great deal of information into this book and has made excellent use of much additional information, it would be a serious mistake for the

reader to conclude that we have here a distillation of all that is known about the "psychology of survival." In fact, this reviewer regrets very much that von Greyerz chose the title *Psychology for Survival*. The book is too narrowly conceived to merit such a title. Much that might properly be labeled as belonging to a "psychology for survival" would have no place in von Greyerz's book. This would include knowledge which has been accumulated through research programs in the United States, Canada, Great Britain, Belgium, France, and other countries conducted in support of

survival training programs. Since survival usually involves stresses of considerable intensity and/or duration, it is doubtful that much of the research which is commonly labeled as the "psychology of stress" would qualify, but a great deal of it certainly has a place in "psychology for survival." A book that will synthesize and make use of the existing divergent streams of knowledge about the "psychology for survival" is needed, not only in planning for civil defense and national defense but in the general fields of mental health, psychology of learning, and the like. Such a book remains to be written, however.

## Child XVI

Ruth S. Eissler, Anna Freud, Heinz Hartman and Marianne Kris (Managing Eds.)

*The Psychoanalytic Study of the Child, Vol. XVI.* New York: International Universities Press, 1961. Pp. 563. \$8.50.

Reviewed by EMMY SYLVESTER

who took a PhD in Psychology and then an MD from the University of Vienna and who practiced clinically in Chicago at the Michael Reese Hospital and at the Chicago Psychoanalytic Institute before moving farther west. She is now a training analyst at the San Francisco Psychoanalytic Institute, Chief of Child Psychiatric Services at Mt. Zion Hospital and Associate Clinical Professor of Psychiatry at Stanford Medical School.

VOLUME XVI of *The Psychoanalytic Study of the Child* keeps the Annual's traditional promise to psychologists of all denominations: it makes substantial as well as challenging contributions to their field.

Greta Bibring and her coworkers give an interim report on their ongoing project, "A Study of the Psychological Processes in Pregnancy and of the Earliest Mother-Child Relationship." Their thesis—pregnancy as a "maturational crisis" in the personality

formation of women—is convincing. The aim of the study to describe phase-specific emotional phenomena of pregnancy, to explore the effects of earlier or subsequent stages and to define the impact of cultural factors and managerial forces is in line with the genetic and the adaptive approach. Workers who are themselves engaged in some type of multidisciplinary research and also those who are still skeptical about the advantages of the pluralistic approach to psychological phenomena will be challenged by the research design.

Observations on fifteen pregnant women were made by "psychoanalytically oriented" representatives of five professional disciplines during the pregnancy and post-partum period of each subject. Data were processed in recurrent "assessment conferences" in the course of group discussions under the leadership of the senior investigator. An elaborate "outline of variables" and an equally impressive "glossary of defenses" were devised to assure uni-



formity of observation and recording.

One assumes that Dr. Bibring's personal eminence in the field of psychoanalysis will enable her to integrate the raw material of her study at a level that transcends the commonplace. As behooves the subject matter, the final yield of the study is awaited with "watchful expectancy."

CONSTRUCTIVE controversy among scientists should lead to a quest for clarification of basic theoretical issues. Dr. Bowlby's earlier detour from instinct theory and ego psychology into ethology (documented in a previous volume of this annual and in several publications elsewhere) had precisely that effect. A symposium on "Psychoanalytic Studies in Object Loss and Depression" demonstrates this. Dr. Bertram Lewin's "Reflections on Depression" is of particular importance here. This clear and scholarly review describes the structural approach to affective phenomena. We are reminded of the shortcomings of the stimulus-response model for the explanation of affective experiences, "normal" or fleeting as they may be. Psychological mechanisms that involve the total structure of the personality underlie all manifestations of affect. Thus it is important though not sufficient to distinguish depression as symptom from depression as mood. Specific feelings have to be located within the particular intersystemic conflicts from which they originate.

In her article "On Sadness and Grief in Infancy and Childhood" Margaret Schoenberger Mahler deals with the earliest affective experiences. Longing, a precursor of sadness and grief, is a purely subjective experience, the consequence of unavoidably intermittent need-satisfaction. Later, when ego and object cathexis have emerged, and that is roughly in the second half of the first year of life, such "ego-filtered" phenomena as sadness and grief can be observed. But early grief bears no resemblance to the structural complexities of depression. It is short-lived and fleeting. The immature ego cannot tolerate the helplessness of an objectless state and protects itself against such exigency by the whole array of primitive defenses.

Edith Jacobson ("Adolescent Moods and the Remodeling of the Psychic Structure in Adolescence") emphasizes the close connection between affects and cathetic shifts. She departs from the turbulent emotional phenomena of this phase, during which depression and elation of extreme intensity often follow upon each other in rapid succession. Affective fluctuations have to be understood as corollaries of changes in the state of psychic energy: when existing psychic structures are melted down psychic energy is freed, it is harnessed again when new structuring follows. Elation and Depression are the respective affective concomitant of phases in this process of structure formation.

THE GENERAL psychological understanding of phenomena in the field of perception and sensation stands to gain by the application of the psychoanalytic model to them. A number of investigators show the influence of sensory stimulation on the development and the stability of psychological structures.

In "Perception, Reality Testing and Symbolism" Rubinfine concentrates on formal aspects of thought processes.

When the amount of sensory stimulation from outside sources decreases critically, or when intrapsychic perceptions prevail over the perception of contents in the external reality, thought organization drops to a lower and more primitive level, mentation becomes subjective, concrete and affect laden, and primary processing thinking encroaches upon the ability to conceptualize. This sets the conditions for symbol formation.

Dorothy Burlingham describes the effect of a specific sensory defect. In keeping with the clinical orientation of her paper: "Some Notes on the Development of the Blind," she shows how the absence of visual clues interferes with the autonomous development of the ego functions, motility, speech and reality testing.

The motor impulses of blind children are insufficiently channeled toward external objects and retain many autoerotic aims, as in the stereotype motor patterns of "Blindism." Blind children do not see the effects of their actions on people and things and this interferes with the free play of their aggression. This dif-

ficulty in reality testing also heightens conflicts over hostility and increases fear of magical retribution. Prolonged dependency and protective compliance follow and distort the development of object relations. The expressive function of speech lags behind its use for spatial orientation; age adequate curiosity is delayed and deflected from interest in sex and size to concern over the difference between blind and sighted.

Ideas about the pathogenic impact of excessive sensory stimulation are developed in E. James Anthony's brilliant paper, "A Study of Screen Sensation," and in the remarks of his discussant Paul Kramer.

The psychoanalytic material produced by a severely disturbed 18-year old girl was replete with sensory configurations. They colored her memories of people and events and changed kaleidoscopically from one session to the next. Close observance of the patient's sensory experiences during the analysis guided Dr. Anthony's therapeutic technique. This aided the emergence and the successful working through of an important screen sensation.

Dr. Anthony draws fascinating parallels between his patient and Marcel Proust's self-description: both over-reacted to sensory stimulation and tended to develop screen sensations. Paul Kramer's discussion remarks add dimension to the understanding of sensory stimulation as a source of pathogenesis in general. He shows that the intense influx of sensory stimuli is particularly traumatic to young children whose motility is unduly restrained at the same time. It follows that disruption through overstimulation is not so much a matter of the absolute quantity of excitation, but rather the result of crucial and prolonged disproportion between sensory input and motor discharge.



*It does not suffice to know the soul completely to know what we know by our interior feeling, since the awareness we have of ourselves does not perhaps reveal to us more than the smaller part of our being.*

-NICOLAS DE MALEBRANCHE





# Mind Revisited

Seymour M. Farber and Roger H. L. Wilson (Eds.)

*Control of the Mind: Man and Civilization.* New York: McGraw-Hill, 1961. Pp. v + 240. \$6.50 (cloth) \$2.95 (paper).

Reviewed by OGDEN R. LINDSLEY

Seymour Farber, one of the editors of this volume, is Assistant Dean in Charge of Continuing Education in Medicine and the Health Sciences at the University of California Medical Center, San Francisco. Roger Wilson, the other editor here, is Assistant Head of Continuing Education in Medicine at the same place. The reviewer, Ogden Lindsley went to Brown University for his AB and MA and then to Harvard for his 1957 PhD. Since 1953 he has been Director of Harvard's Behavior Research Laboratory, where, in the traditions of Hunter and Skinner, he has developed free-operant techniques for analyzing the behavior of psychotic patients, normal persons and retarded children. He helped start the Journal of Experimental Analysis of Behavior and is on the editorial board of the international journal of Behaviour Research and Therapy. In 1962, the American Psychiatric Association awarded him the Hofheimer Prize for his contributions to psychiatric research.

OURS is the Age of Paper. Scientists are admonished: Publish or perish.

An efficient way of continuing to teach, administrate, committee-meet and correspond, and at the same time "author" a book, is to get money from some foundation, conduct a symposium on an "important" topic, choose an attention-getting title, invite a number of "names" to participate, and publish the papers as a book. The chairman of the symposium can turn over the papers to his secretarial staff for editing and, magic of magics, he is the

author of a book! (For obvious reasons publishing houses reinforce this procedure.) However, since there are not enough "names" to go around, one begins to find the same story in each of several books with slightly different metaphors. The burden placed on scholar and student by such overlapping, redundant material begins to mount. Economy publishing without indexes further increases the burden to scholars.

In the scramble for imposing titles, there seems to be a return to all-encompassing pre-scientific terms which imply that a particular symposium has mastered problems which puzzled the ancients, had been shelved as insoluble, or had been broken into more manageable sub-problems.

The use of the term *mind* as a noun instead of a verb in the title of this symposium is a case in point. *Mind* as a noun, an object, a subject of scientific investigation has rightfully disappeared from the scientific literature as its function has been broken into smaller and smaller sub-parts for scientific analysis. In 1961, *Psychological Abstracts* contained 7,353 references, with only seven indexed under *mind*, but 32 under *performance*. The 1961 *Psychopharmacological Abstracts*, with 3,206 references, had no entry for *mind*, but indexed *performance* 130 times. *The American Handbook of Psychiatry* (1959) has only 11 entries for *mind* in a 90-page subject index. These data demonstrate that *mind* is not useful as a scientific term, yet it still seems useful as an attention-gaining title for books and symposia (e.g., J. Scher, *Theories of the Mind*, 1963).

So it looks as if we must again revisit mind.

WHAT of the revisitation in January 1960 by 26 eminent men at the San Francisco Medical Center of the University of California? A familiar mid-twentieth century bias operated in the selection of these renowned authorities: over thirty percent came from the University of California. Although it may have been necessary to conserve on travel funds, it is nevertheless regrettable that so many of the specialists on such a wide topic came from a single institution.

What of the bias concerning field of specialty? Of the 26 participants in the symposium, five were educators, four psychiatrists, three psychologists, three physiologists, three novelists, and one each from the fields of pharmacology, law, theology, sociology, business, journalism, history, and philosophy. The 14 major contributions were a little less biased towards the non-scientific disciplines: there were three contributions from psychiatry, two from physiology, two from psychology, two from the novelists, and one each from history, law, theology, journalism, and education. With the exception of D. O. Hebb, there was very little representation of the behavioristic approach which has recently been applied so successfully in the United States. From this biased and rather sketchy coverage of such a vast topic, some very interesting papers emerged. However, it is regrettable that they are buried under this extremely general topic of "mind." Arthur Koestler's creative treatment of the creative process, for example, is exciting reading that would be better placed in a collection of papers on creativity.

IN general, the more expert the contributor in his field, the more conservative he was both in his formal presentation and in the panel discussions.

Throughout the discussions there was a running battle between psychiatrist J. G. Miller and the neurophysiologists as to whether computers are useful as brain models.



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**The Behavioral Science Book Service** 59 FOURTH AVENUE  
NEW YORK 3, N. Y.



The three papers on the influence of drugs on the individual, given by Seymour S. Kety, J. G. Miller, and Jonathan O. Cole, are among the best and most factual in the book. In general, these experts agree that drugs are not the answer in controlling behavior. Cole summarized the possibility of using drugs for "brainwashing" as follows: "I see no reason to believe any drug would be more effective than social and psychological pressures or physical discomforts in producing changes in an individual."

A member of the audience of the panel discussion on restriction and freedom of the mind shared this reviewer's reaction. He suggested: "A more appropriate study than that of complete control of the mind (repugnant as such a concept was) might be a study of the various factors which influence behavior."

Although every article in this symposium contains a few interesting points and bears reading, it is difficult to summarize without abstracting the entire book. Several of the articles would be more efficiently contained in symposia on other topics. In general, one gets the feeling of having been on a very cultured and posh journey on the sunny side of the ship—Port Out and Starboard Home. At the end of the book, home we are! And *mind* you, we are little different for our pleasant journey. It is difficult to recall exactly where it was we went and what important things we discovered. So it has always been with the revisitation of mind. Do you have time to *mind*?

## Anecdotal Murder

John M. MacDonald

*The Murderer and His Victim.*  
Springfield, Ill.: Charles C  
Thomas, 1961. Pp. xiv + 420.  
\$11.50.

Reviewed by HANS A. ILLING

The author, John M. MacDonald is  
Chief of Forensic Psychiatry and Pro-

fessor of Psychiatry at the University of Colorado Medical School. This is his second book. The reviewer, Hans A. Illing, has his PhD in Comparative Literature from the University of Berlin and has done additional graduate work in this country resulting in degrees from the Universities of Utah and Tulane and progress toward a PhD at the University of Southern California. Most of his professional experience has been in the clinical field and he has done extensive writing. He is a contributor to *Pioneers of Psychoanalysis* and to *Group Psychotherapy and Group Dynamics*, both to be published by Basic Books this year. Presently he is associated with the Parole and Community Services Division of the Department of Corrections of the state of California.

MANKIND has been fascinated by murder ever since Cain slew Abel, and therefore it is no surprise that a large bulk of our literary output as well as of our radio and television productions are devoted to murder. Nevertheless the author of the present book is probably correct in stating that "surprisingly few books have been written on the circumstances of criminal homicide and the mind of the murderer." Hence, when a book of this rare type does make its appearance, it warrants special attention.

In orienting himself to his intended audience, MacDonald seems to have avoided one kind of pitfall while falling into another. He addresses his book only to the "intelligent reader, who is not familiar with the language of the lawyer and psychiatrist," and thereby puts a realistic limitation to his work. On the other hand, is it realistic to present to the intelligent reader a book of over 400 pages, with a bibliography of over 250 titles, and a price tag of \$11.50?

In terms of content it can also be argued that many chapters may lie outside the intelligent reader's province of interest and comprehension. Does the lay reader really gain anything from all too short (and, therefore, dangerously oversimplified) discussions of such topics as melancholy, schizophrenia, and organic brain disease, or of the passive-aggressive personality? Would he be interested in the truth serum, lie de-

tector tests, "false confessions," the Gansse syndrome, etc.? Furthermore, the average reader, professional as well as lay, may have a desire (this reviewer confesses to have an urge) for a good organization of the material. Yet the presentation is a tangle of topics, and the author weaves erratically in and out of trends of thoughts, thereby tending to create confusion, rather than clarity. For example, there is a chapter on the "Urge to Confess," followed by "Portrait of a Plane Bomber" (Contributed by James A. V. Galvin and the author); then comes "Melancholy and Murder," "Schizophrenia," and other illnesses, "Children Who Kill," etc.

This reviewer would imagine that most people (psychologists included) would be interested in the basic nature of murderers. Yet only one out of 21 chapters is devoted to "The Psychology of Murder"! The author states (perhaps with respect to the psychological aspect) that "what happens in reality, particularly with respect to violence, murder and cannibalism, is often incredible and incomprehensible; to represent such reality within the demanded credible, comprehensible framework of literature is a contradiction in terms" (p. 402). It seems to be his contention that the murderer's mind or psyche cannot be comprehended. If so, it would appear to this reviewer that the author wasted an entire book on the reading public. This, however, is doubtful. In fact, the numerous case histories make entertaining reading. Chapter 7 on "Mass Murderers" presents fifteen profiles of murderers, all now belonging to history, from Gilles de Rais (1404-1440) to Chow-Tse-ming (1955). Psychologists might object that the author nowhere attempts to analyze or even theorize on these profiles; this reviewer thinks that the intelligent reader will not object. Only occasionally does the author commit the "crime" of stereotyping a criminal, as in the case of the epileptic (who was not accused of murder but of armed robbery), who the author states "did not appear to be the kind of person who would commit a crime" (p. 235).

Of the greatest interest to psychologists probably is the chapter on "The



Psychology of Murder." The author does not find simple explanations in this psychology. He does attempt, with brief case histories, to discuss the obvious as well as the hidden causes of murder, helping himself generously to representative studies in world literature, such as Shakespeare's *Richard II*, *Hamlet*, *Macbeth*, Dostoevsky's *The Brothers Karamazov*, *Crime and Punishment*, and others; incidentally, Goethe is quoted, "There is no crime of which I do not deem myself capable." Statistics are cited from Dollard, theories from Bowlby, English and Pearson, and the "Oedipus Complex and Murder" is explained (without mentioning Freud's contribution in this connection, although Freud is mentioned on a few other occasions). Interwoven in this chapter are some theoretical considerations—considerations of overcompensation (four paragraphs), obsessional neurosis (two pages), hysteria (two pages), and suicide (one page).

And there's the crux of the trouble with the book: the author attempted "to avoid the evil of oversimplification" so that "the expert may find something of value." Actually, however, most of the case histories are oversimplified, unanalysed or unexplained. Surely, many of the book's stories, interesting as they are, could have been omitted or condensed, and thus would have made the book less bulky and less costly. Many of the technical details (psychiatric or legal) could have been omitted, since the "intelligent reader" does not gain any knowledge from them; and to the expert, they are daily routine.

All told, it would appear that the book does not add to the existing literature, either the professional or the non-fictional. Considering the amount of effort and labor put forth for such a mammoth volume, what a pity!



*All the effects of nature are only the mathematical consequences of a small number of immutable laws.*

—P. S. LAPLACE



## From Piaget to Pedagogy

K. Lovell. Foreword by Barbel Inhelder

*The Growth of Basic Mathematical and Scientific Concepts in Children.*  
New York: Philosophical Library, 1961. Pp. 154. \$7.50.

Reviewed by JOHN C. WRIGHT

*The author, K. Lovell, received his BSc, MA and PhD from the University of London and presently is Lecturer in Educational Psychology in the Institute of Education, University of Leeds. He is the author of Teaching Arithmetic in Primary School (1953, 4th edition 1962) and Education Psychology and Children (1958). John C. Wright, the reviewer, is Associate Professor of Child Psychology in the Institute of Child Development, University of Minnesota. He worked with Jerome Bruner at Harvard where he received his AB and with Robert Sears at Stanford where he received his PhD. His interests in cognition as a process and in children as organisms, combines in his recent study of mechanisms in information processing and in search behavior as they influence problem solving in children. He is co-author with Harold Stevenson of a chapter on Experimental Child Psychology in a forthcoming volume and is general editor of Prentice-Hall's new child psychology series.*

BARBEL INHELDER, in the preface to this short and simple volume, maintains that Lovell has accomplished something that she and Piaget could never manage in French—to write for schoolteachers a psychological book that is accurate, scientific, clear and complete. Perhaps she is right, for the book does indeed make some of Piaget's basic work seem intelligible, even simple, when compared with available English translations of his writings in French. Simultaneously Lovell has avoided oversimplifying some of Piaget's con-

cepts merely to make them palatable to elementary school teachers. However, one could hardly call the book a complete treatment of Geneva psychology.

Lovell appears to have had several purposes in writing this book, most of which have more relevance to educational psychologists, teachers, and teachers of teachers than to experimental and child psychologists concerned with cognition and cognitive development. The latter will not find this a survey of, introduction to, or textbook on Piagetian psychology. Rather the author tries on the one hand to alert teachers of science and mathematics to the psychological literature on the development (both how and when) of concepts in children, and, on the other hand, to make sure that teachers themselves have an appreciation of the logic and psychology of numbers and measurement. A third apparent aim intrudes itself from time to time, and that is to offer practical suggestions for directly adapting Piaget's observational methods for investigating cognitive development as teaching or enrichment techniques for promoting cognitive development. Such adaptations are offered without evidence of their effectiveness, and without reference to any formal psychological theory that can be induced from the observations or from which the applied techniques could be deduced.

Lovell believes that if teachers would only try out some of Piaget's procedures with children, they would gain valuable insights into children's conceptions of number, substance, weight, time, space, measurement, length, area, and volume; they would simul-



taneously understand these concepts better themselves and also discover effective means of facilitating their development in elementary school pupils. Throughout there is evident the attempt—thoroughly consistent with Piaget's belief in understanding by active, concrete operations—to help the child discover rather than merely absorb the constancies and generalizations about real objects and events that will presumably enable him to achieve a metrical and logical conception of the objective world. To this end Lovell's students have carried out some 7,000 replications and variations of Piaget's 'experiments' with young children.

**P**ERHAPS most frustrating to a psychologist is the mixture of raw observation (occasionally tabulated by age of subject), cookbook advice on exactly how to build and use simple apparatus in a classroom situation, sophisticated treatments of topology, geometry, and number theory, and speculation about the origins of scientific and mathematical concepts in primitive man. Indeed the latter is often used with the implication that ontogenetic development recapitulates historico-cultural development, an analogy that is as unproven by Lovell as it was by Heinz Werner two decades ago.

Lovell's own research, while attempting to impose some standardization upon Piaget's 'clinical' method, still relies heavily on verbal questions and answers, and upon verbal prompting. That Lovell has a clear appreciation of the dangers of relying on the child's verbalizations as an index of his understanding is quite clear. He cautions the reader repeatedly on this point. In attempting to reduce dependence on verbal communication with the child, a number of sets of 'materials' for use with young children have been developed. Lovell describes and classifies the Montessori materials (currently being rediscovered in this country), the Cuisenaire materials (developed recently in Belgium), the Stern materials (American), and the Dienes materials (English), all aimed at helping pre-school and primary grade children to develop

numerical concepts. Yet nowhere is there reported an attempt empirically to compare and evaluate these techniques.

Finally there is a brief but moderately sophisticated treatment of number theory, aimed at non-mathematicians, that examines the kinds of assumptions necessary to our number system and the generality of these assumptions and fundamental constancies. Lovell suggests a parallel between these assumptions (closure, commutation, association, distribution, and identity) and the most basic constancies that Piaget and his

successors have identified in children's conceptual development. The isomorphism proposed is suggestive and intriguing, though by no means conclusively demonstrated.

One notable omission in this, the first edition of the book, has been remedied in a second edition just published in England. The new edition includes references to the extensive work of those stimulated by Piaget but who were not a part of the Geneva group. These include Smedslund in Norway, Dodwell in Canada, and Elkind and Wohlwill in the United States, among others.

## O True Apothecary!

Mortimer Ostow

*Drugs in Psychoanalysis and Psychotherapy.* New York: Basic Books, 1962. Pp. 348. \$8.50.

Reviewed by NATHAN S. KLINE

*The author, Mortimer Ostow, is a physician who holds an MA in chemistry from Columbia University and the MedScD in Neurology from the Medical Branch of the same institution. A productive author, he has covered topics ranging from diagnostic electroencephalography to the psychology of religion. Presently he is an Associate Attending Psychiatrist at Montefiore Hospital. Nathan Kline, the reviewer, did undergraduate work with Köhler at Swarthmore before moving to Harvard where S. S. Stevens was his graduate advisor until he went off into medical school and into medicine, before and during World War II. After the war he returned to academia and earned an MA at Clark University. The breadth of his interests and capacities are indicated by the fact that in the psychoanalytic realm he has served as amanuensis to Schilder and has had courses with Horney, Sullivan, and Freida Fromm-Reichman, while, in 1957, and in a quite different realm, he won the Albert Lasker award of the American Public Health Association for*

*his pioneer work with the Rauwolfia Alkaloids. His breadth again is indicated in his list of publications which extend from his editing of the Pavlovian Conference on Higher Nervous Activity (1961, CP, Nov. 1962, 7, 426-427) to the 1962 production Handbook of Psychiatric Treatment in Medical Practice. Presently he is Director of Research at Rockland State Hospital, Orangeburg, New York, and Assistant Clinical Professor of Psychiatry at Columbia University.*

**T**HE present pre-scientific stage of psychology and psychiatry is characteristically populated by a variety of "schools." Similar in broad outline to the state of medicine at the time of Jesus, there are today two major traditions: 1) the psychoanalytic (equivalent to the Dogmatic or Logical) which infers the existence of unseen and untestable entities and causes; and 2) the Empirical (the same name as 2000 years ago) which stresses observation and experience. Ostow, like the early



adherents of the Methodist School which arose about the beginning of the Common Era, was originally an orthodox Dogmatist who could not resist looking at reality:

"Some of my colleagues say that... (the Atom bomb) doesn't (exist): they lump it in with all the other internal problems, like road-accidents, industrial injuries, cancer, death, and so on. Personally, I'm a middle-of-the-road sort of man: I believe that machinery, and motor cars in particular, are intrinsically dangerous. I even claim that they have the power of moving quite often in a direction opposite to the one demanded by their victim's neurosis. But be good enough not to repeat my remark in the presence of any of my colleagues: any rehabilitation of the external world injures them far more than could the heaviest motor-lorry."

Nigel Dennis, *Cards of Identity*.

On the other hand Ostow also needs the consolation of thinking (and feeling) that he knows what he is doing and hence rejects the purely Empirical approach:

"Medicine, n. A stone flung down the Bowery to hit a dog on Broadway."—Ambrose Bierce, *The Devil's Dictionary*.

There were then and are now numerous fussy little theorists and practitioners who so limit themselves to preciously circumscribed areas that they may be said to have founded kindergartens rather than schools. Today we have the added curse of nasty self-righteous intellectual pimps who try to sell what they themselves do not have—a technique whereby truth is to be achieved. Ostow avoids pandering to these doubly blind, crossed-over, statistically analyzed snobs. Instead he has attempted to formulate certain new general principles based on the observation of individual cases which was exactly the method of the Methodists.

How well has he succeeded? In a curious paper presented to the American Psychoanalytic Association in 1956 which he and I coauthored, Ostow conceptualized the problem within the framework of psychoanalytic metapsychology whereas I held for a broader psychophysiological approach. We were able to compromise the lan-

guage in a manner that satisfied us both. The basic thesis was that one major mode of operation of the drugs is to modify psychic energy. In *Drugs in Psychoanalysis and Psychotherapy* this thesis has been excitingly utilized to expand the scope of psychoanalytic theory by systematically developing the second of Freud's general concerns (psychoenergetics). In summarizing the other two areas (psychodynamics and psychotopology) Ostow's distilled statements are almost aphorisms. The knowledge gained from manipulation of psychic energy by use of the drugs forces certain revisions of traditional psychodynamics.

Ostow has summarized part of this in a recent paper:

"Freud assumed that one of the basic determinants of the occurrence and fluctuation of the symptoms of mental illness was the amount of a form of psychic impetus or energy prevailing within the mental apparatus. This assumption permitted him to relate different forms of illness to each other and to construct a theory of mental illness and its psychologic treatment.

"Recent experiences with the newer drugs, tranquilizers and 'energizers,' permit us for the first time to assess and deliberately influence this mental energy. It is interesting that most of Freud's assumptions are confirmed. We are also able to extend his theory by means of the new observations thus opened to us and to gain new understanding of the varieties of mental illness, their evolution and transmutations, and of the meanings of symptoms. Certain symptoms indicate excess, others deficiency of energy. This knowledge adds to the efficiency of our psychotherapy, offers a rationale for drug therapy, and establishes a theoretical basis for integrating the two in practice."—Mortimer Ostow, *Symptoms and Their Meanings*.

On the other hand Ostow's attempt to relate the psychoanalytic to the neurological universes of discourse does nothing but confuse me. Perhaps this is because my bias is toward a syntactical system which will subsume both psychological and physiological approaches. Much of the information in this area is stimulating but not productive. Fortunately the neuroanalytics can be skipped without substan-

tial loss and it is not necessary to be expert in navigating the cerebrospinal canal with microscope and Nissen stain to read and understand the rest of the book.

Time alone can tell how far the specific thesis will be substantiated but there exists no question that the attempt itself is an important undertaking and, even if wrong, is worth trying to understand. The drugs, if for no other reason than the fact that they work, will have to be integrated into the body of psychological theory and Ostow has made the first major effort to accomplish this by interpreting neuroanatomy, neurophysiology and psychopharmacology within the framework of Freudian metapsychology.

I would recommend reading Chapter 4 immediately after Chapter 1 and then going back and continuing the book in normal fashion. The build-up to "Use of Pharmaceutic Agents in Therapy" becomes more interesting and can be read more critically if you know where Ostow is heading. He unsettles a host of preconceptions and substantiates many others. What one accepts and what one rejects will necessarily differ from reader to reader but the book is guaranteed to contain something that will delight and something that will irritate everyone.

## Measuring With Virtues and Faults

J. Gerberich, Harry A. Greene and Albert N. Jorgensen

*Measurement and Evaluation in the Modern School*. New York: David McKay, 1962. Pp. v + 622. \$6.95.

Reviewed by THEODORE R. HUSEK

The three authors here, Gerberich, Greene and Jorgensen, all received their degrees from the University of Iowa and all have published before, both separately and together, in the field of educational measurement. Gerberich



is now Professor of Education and Director of the Bureau of Educational Research and Services at the University of Connecticut, and Alvin N. Jorgensen is President of that Institution. Harry Greene is Professor Emeritus of Education at the State University of Iowa. The reviewer, T. R. Husek, took his PhD in psychological measurement at the University of Illinois where he worked with Charles Osgood and Lee Cronbach. He moved to UCLA in January, 1960, where he teaches statistics and measurement courses at the School of Education and, around his home in the hills, watches with fascination a variety of wild animals none of which seem aware that he grew up on the West Side of Chicago. His research interests include the measurement of anxiety, the nature and measurement of teacher effectiveness, and several aspects of methodology in the use of psychological tests.

THIS VOLUME was prepared as a comprehensive first text in measurement and evaluation for the prospective teacher who knows little about measurement and less about statistics. It is not a success.

The book has some important virtues. There is an extensive and useful section on the construction of classroom tests. The last section of the book, "designed to deal briefly with the appraisal of pupil learning in ten broad instructional areas and in general education," contains much valuable material. There is a simple outline of each chapter at its beginning, and there is a wealth of useful illustrative test material.

But the faults of the book are also important. Considering that the word "evaluation" is used in the title, the treatment of evaluation in the text is woefully inadequate. Too much of the terminology is not consistent with that of other comparable texts and, furthermore, it is confusing. For example, on page 6 the reader is led to distinguish among "mental ability tests," "mental tests," "intelligence tests," "scholastic aptitude tests," and "psychological examinations." The writing is almost always dull, and it is often unclear. To cite one example, the para-

graph description on page 39 of a standardized test is abominable. (Although it should be noted that the writing is spotty—perhaps due to different authors—and some of it is quite clear. The first chapter is the worst.)

Outlining and the use of italics to separate topics are carried to the point where they sometimes distract rather than assist. The lists are sometimes quite strange, e.g., on page 39 where five types of examinations are distinguished: "(1) oral examinations, (2) essay examinations, (3) objective examinations and scales, (4) performance tests and scales, and (5) other evaluative instruments and techniques."

The number of tests treated makes the absence of some common and important tests stand out. For example, the Strong Vocational Interest Blank is discussed, with a full page of excerpts, and the Kuder Preference Record is not even mentioned. This last fault is particularly important, since the reasons for the choices of tests to be included are not given.

As a final criticism, some drawbacks of the generally excellent last section on measurement and evaluation in specific school subjects should be mentioned. The sections on mathematics and science suffer by taking inadequate account of the impressive changes presently occurring in these areas. As an extreme example, (most of the material is quite good) the list of principles and generalizations of science at the beginning of that chapter are taken from a 1932 publication.

In general, this book possesses many of the virtues and faults of its parent volumes, by the same authors, on measurement and evaluation in the secondary school and in the elementary school. The authors indicate that it is not a revision of the two previous books, and much of the material in the present volume is identical to that of the previous volumes. Instructors who liked the previous volumes will probably like this one. However the reviewer feels that a one-volume revision of the parent books would have been much more valuable than the present offspring.

## Useful Still

Anne F. Fenlason. Revised by Grace Beals Ferguson and Arthur C. Abrahamson

*Essentials in Interviewing: For the Interviewer Offering Professional Services.* New York: Harper & Row, 1962. Pp. v + 372. \$5.00.

Reviewed by LOIS LANGLAND

The author, Anne Fenlason was, at the time of her death, Professor of Social Work at the University of Minnesota. Grace Beals Ferguson, the author's sister and one of the collaborators on the revision of the book, is Professor Emerita of Social Work at the University of Washington. Arthur Abrahamson, who collaborates with Ferguson on the revision, is Associate Professor of Social Work at the University of Washington and is presently UN consultant and advisor on staff training to the Israeli Ministry of Social Affairs. Lois Langland, the reviewer, is now a counseling psychologist at UCLA where among other things she helps manage the Student Counseling Center. She started her academic career with a degree in English and taught that subject, but turned, as has many a sensible English major before and since, to psychology. She received her PhD from the institution in which she is now employed. In 1957 she helped initiate the UCLA Gifted Student Program.

ANNE F. FENLASON's classic *Essentials in Interviewing* (Harper and Bros., 1952) has been revised by Grace Beals Ferguson, who originally "helped immeasurably throughout, in . . . criticisms and suggestions on the content and composition of the book" and by Arthur C. Abrahamson, both of the University of Washington School of Social Work. The revised work must, of course, be appraised both in terms of the intent and success of the original work and in terms of changes in the present edition.

The original volume grew out of an



undergraduate course open to all students who might be using the interview in their future professions, and the expressed intent was to stress only processes which were seen as having general application. This intent is reiterated in the revised work.

A major strength of the early Fenlason volume was its stress on the relevance of cultural background for both the interviewer and the interviewed. In this area the work still outdistances many present day considerations of the interview. Emphasis on cultural background is as pronounced in the revised edition as in the original, and is perhaps enhanced by the inclusion of a chapter, "Knowledge of Role," which constitutes the one major change in the new work. There are throughout both the old and new volumes direct or indirect references to perception, expectancy, self-concept, learning, problem-solving, and communication, but little or none of the relevant literature in psychology and other fields is cited, and none of these concepts is dealt with in the light of recent research literature. Clearly the new work is still a volume that draws on sources that came to mind in a practical setting; it gives scant and inadequate indication of the vast literature pertinent to the task at hand. A glaring example is that an outstanding work in the same field, Perlman's *Social Casework: A Problem-Solving Process* (1957), is not cited at all. Kahn and Cannell's excellent book, *The Dynamics of Interviewing* (1957), is listed as recommended supplementary reading, but is otherwise unmentioned. Footnotes and bibliography refer predominantly to the 30's and 40's. Other than in the new chapter on role, there are few exceptions to this pattern. In general, references have not been brought up to date. Examples for class discussion remain essentially the same.

**D**OES all this mean there should have been no revised edition? No. Anne Fenlason was a unique woman and had many important things to say. Experienced interviewers will recognize the astuteness of her perception of relevant variables in the interview, and her remarkable synthesis of ideal-

ism and practicality. She has been described as "a social worker and a scholar with deep convictions about the infinite worth and value of every man, woman, and child . . . a skilled interviewer who knew how to apply knowledge in a practical, helpful way to others . . ." Though the tone of the book may seem a little old-fashioned by present day standards, no one need fear that students who catch the spirit and dedication of the work will ever see process as separate from outcome

in their work, or the science of interviewing as somehow more important than people. For this reason alone, if for no other, this is a good book for beginning interviewers to be exposed to and a good book to keep in circulation. But it is equally important that prospective interviewers be made aware of the wide range of studies that point up the continuing search for improvement in understanding the goals and processes of the interview. For this one has to look elsewhere.

## Assorted Assaults on Delinquency

**Sheldon and Eleanor Glueck.** With the statistical assistance of Rose W. Kneznek

*Family Environment and Delinquency.* Boston: Houghton Mifflin, 1962. Pp. vii + 328. \$6.50.

**Lewis Yablonsky**

*The Violent Gang.* New York: Macmillan, 1962. Pp. xiii + 265.

**National Research and Information Center on Crime and Delinquency,  
National Council on Crime and Delinquency**

*Current Projects in the Prevention, Control and Treatment of Crime and Delinquency.* New York: National Council on Crime and Delinquency, 1962. Pp. 659.

*Reviewed by* MARTIN GOLD

*Sheldon and Eleanor T. Glueck, authors of the first book reviewed here, are the Gluecks who together have been doing research and writing books on juvenile delinquency since the early thirties. Lewis Yablonsky, author of the second book, is a sociologist with a PhD degree from NYU and teaching experience at Columbia and Harvard Universities. He, too, is well known for his publications in the area of juvenile delinquency. The author of the third book, listed as the National Research and Information Center on Crime and Delinquency and the National Council on Crime and Delinquency, cannot here be the subject of*

*even one biographical sentence. The reviewer is Martin Gold, who now serves as an Assistant Program Director in the Institute for Social Research at the University of Michigan and who also holds an appointment in the Department of Psychology. A product of Michigan's program in social psychology, he has published not only on the problem of delinquency but also on such topics as social structure in the classroom, suicide, homicide, and family structure. He is presently on the staff of two delinquency research projects, one in Flint, Michigan, and one in Chicago, and also serves on the staff of the University of Michigan Fresh Air Camp for*



disturbed and delinquent boys. His present research interest concerns the effects of social structure on personality development and he has in press a book, soon to be published by the Institute for Social Research, *Status Forces in Delinquent Boys*.

LITERATURE on juvenile delinquency continues to pour from the presses in so great a flood that no one, even with the best intentions, can hope to keep up with it. Psychologists, psychiatrists, sociologists, social workers, criminologists, police officials, judges write about the causes and cures for delinquency. If J. Edgar Hoover is right that delinquency is on the increase, rates are certainly not rising because the problem is being ignored.

This flood of literature is especially hard to ride out because there is no discernible mainstream. Rather, the flow spreads into separate traditions, runs toward multiple purposes, and reaches varying depths of sophistication: psychological thinking, mostly of a psychoanalytic orientation, does not blend easily with sociological; makers of theory move in different channels from gatherers of data; scientists and practitioners are for the most part isolated from one another. I suppose that a great deal can be said for allowing people with essentially different interests separately to work out their separate problems, and there has been an occasional and crucial juncture. But the three books under consideration here taken together illustrate what seems to me to be dysfunctional compartmentalization, despite the separate strengths they display.

The Gluecks' *Family Environment and Juvenile Delinquency* is a volume of data without systematic theory. Yablonsky's book on *The Violent Gang* is a descriptive and theoretical work without systematic data. While the former suffers much more from the absence of theory than the latter does from its lack of data, our understanding of the delinquency phenomenon will continue to suffer from the separation manifested in both. And the third book, *Current Projects in the Prevention, Control, and treatment of Crime and Delinquency*, indicates that social action is

largely untouched by either theory or data.

It wasn't always this way. Some early collectors of hard data were also theorists and social practitioners. A good example is Clifford Shaw. He firmly established the study of delinquency as a pursuit for sociologists by his ecological studies with Henry McKay in the late 30's and 40's. However, many people overlook the fact that in Chapter VII of *Juvenile Delinquency and Urban Areas*, Shaw and McKay (1942) bring to their data a sophisticated social psychological approach which explicitly denies that they ever thought either that the data were enough or that dilapidated buildings, low incomes, and disease in themselves caused delinquency. Here, Shaw and McKay anticipated recent theorizing about the effects on delinquency of unequal employment opportunities, status strivings, weakened family controls, and the like, attempting to think through these effects not only on a population but also on the individual child. Shaw demonstrated that he could handle case materials too in *The Jack-Roller* (1930) and *The Natural History of a Delinquent Career* (1931). Furthermore, Shaw headed the Chicago Area Project, a concerted effort to do something about delinquency in ways intimately tied to his thinking and his data.

William Healy is Shaw's counterpart in psychology. From the 20's, he was a leader of the child guidance movement, which got its impetus from the problem of delinquency and much of its direction from psychoanalysis. While this psychiatrist was treating delinquents, he was also evaluating treatment effects and conducting well-designed research on the etiology of delinquency (Healy and Bronner, 1936).

Somewhere along the line, unfortunate divisions of labor were established. Psychoanalysis, dominating psychological thought in delinquency, was slow in making contact with the social environment beyond the nuclear family and so alienated sociologists. Psychological testers, with theories of method but not of the phenomenon, had been testing delinquents willy-nilly along with everyone else and became the dominant psychological data collectors, alienating

both theorists and practitioners because of the apparent irrelevance of their data to the delinquency problem.

The University of Chicago Department of Sociology set a pattern among American sociologists; the hard data ecologists and the soft data observers of the Chicago scene drifted apart and with them, organized theory parted from systematic research on delinquency. Ecologists have continued to produce research on delinquency, largely ignoring Shaw and McKay's Chapter VII; and sociologists have worked out theories of delinquency which seldom have led to carefully designed research.

Perhaps because it has proved so hard to define progress in terms of observable behavior, perhaps because the systematic evaluation of the Cambridge-Somerville Project and other action programs have turned out to be discouragingly negative, delinquency treatment programs rarely collect data systematically nowadays. And because psychoanalytic theory charges the family with causing delinquency, and sociological theory charges the nature of the social system, practitioners feel theorists are pointing up unconquerable mountains, so action programs wander without theoretical direction.

Not that all is chaos in the field today. One notes that Bernard Lander (1954) has advanced the Shaw tradition by bringing both additional theoretical elements and more elaborate statistical techniques to bear on an ecological study. Robert Andry (1960) and Albert Bandura and Richard Walters (1959) have been guided by psychological theory in systematic collections of data on delinquents. One notes also that action programs are proceeding in Provo, Utah, and in Chicago with built-in research and evaluation.

But while there are these signs of fruitful integration of theory, data, and action, I fear that the three volumes at hand illustrate the general state of delinquency efforts today—fractionation of functions and its attendant shortcomings.

THE GLUECKS are in the test-giving stream of delinquency research, although I suspect they do not see themselves this way. I place them among the testers because they present us with



wads of differences between delinquents and nondelinquents, with no attempt to tie the differences together with theory or even to tell us why they measured these variables and chose these cross-tabulations in the first place.

*Family Environment and Delinquency* continues the series which began with *Unraveling Juvenile Delinquency* (Glueck and Glueck, 1950) followed by *Physique and Delinquency* (Glueck and Glueck, 1956). This third volume is a report of the computer output on a set of runs on the same 500 delinquents and 500 nondelinquents who were the subjects of the first two books.

One might then indict this latest work with the same faults noted in the research design when the Gluecks first reported their study. For example, the delinquents are of statistically lower socio-economic status than the nondelinquents, matched only grossly by the depressed nature of the neighborhoods in which they lived; so any difference may be attributed to social status differences rather than differences in status as delinquents. In addition, the delinquents had all been residents of detention homes for some time before their psychological traits were measured; so differences in these variables may be attributed to the radically different conditions under which the delinquents and nondelinquents were living at the time of testing.

These are serious design flaws and the data could on their account be terribly misleading. But the fact is that later studies of delinquency, with more closely matched samples and generally better designs, have duplicated many of the differences which the Gluecks report. For me, the lack of theoretical integration most seriously cripples the work in furthering our understanding of delinquency.

The main intention of *Family Environment and Delinquency* is "an exploration of the combined influence of social factors and traits on delinquency." The authors attempt to ascertain why only some boys in a family or in a neighborhood become delinquent while others do not. The Gluecks' strategy is to probe for instances where a social factor—usually a condition in the family environment—produces a

greater number of delinquents among boys who are characterized by a specific intrapersonal trait, than among boys who do not markedly manifest that trait. For example "... children with low performance ability . . . are more likely to become delinquents if their mothers are unpredictably absent from home than are such children of regularly employed mothers, or of mothers who do not work at all."

Now why should more boys with low performance ability than boys with high ability turn to delinquency when their mothers are unpredictably absent from the home? The Gluecks offer no explanation here, although they do in some other cases. A student might try to explain this relationship for himself, but I for one hesitate to bother; *for the relationship may not be a real one, and any interpretation of it might easily be capitalizing on error in the data.* Certainly with thousands of relationships possible when 44 social factors are combined with 66 psychological traits and run against a delinquent-nondelinquent break, some statistically significant differences will emerge by chance. But which differences are chance and which ones reliable? Here is where a coherent theory would come in handy; here is where prediction would have been a powerful tool.

Before pursuing the issue further, I want to show how it crops up in another aspect of the Gluecks' book. In *Physique and Delinquency*, the Gluecks had distributed 66 intrapersonal traits on which delinquents and nondelinquents differ among categories of boys' body types—Sheldon's ectomorphs, mesomorphs, endomorphs, and mixed. They had noted that 23 of these 66 traits were not distributed evenly but piled up among particular body types. These findings suggested to the authors that certain traits, linked as they were to inherited body types, might also be largely inherited. In this third volume, the researchers pursue this line of investigation further:

"We proceed on the assumption, then, that when significant variation is present in trait-incidence among body types of nondelinquents it is reasonable to infer that the trait is essentially oriented in constitution although removed

from the genes by one or more steps . . . Where such variation was not found we could not be certain that the trait is largely attributed to socio-cultural conditioning. The present inquiry leads us in this direction however." . . .

"Now however we think it may be suggested that the traits not shown in *Physique and Delinquency* to vary in incidence among the body types and also not found in the current inquiry to be related to any of the 44 social factors (or related to only one) may (like eye color which is not linked to physique type) nevertheless still be largely of genetic determination. Only further and more intensive research on other samples of cases would determine the correctness of this assumption."

In the current work five traits turn out unrelated to any of the 44 social factors and another eight traits related to only one. The Gluecks conclude then that "The most that can be ventured in the light of the evidence in the present inquiry is that such traits are closer to the innate than to the environmental zone of the bio-social continuum."

Among the traits the Gluecks place close to the innate end of the biosocial continuum are tremors, genital underdevelopment, poor surface contact with others ("inability to get along with people" . . .) and aestheticism ("impulse for the more refined discriminating and artistic"). Now what little I know about genetics prompts me to regard irregular reflexes and genital underdevelopment as certainly determined largely by heredity. But *poor surface contact with others* and *aestheticism*? I suspect that the more I learned about genetics the more implausible these conclusions would seem.

Here again, the authors offer no rationale for surprising findings. And I am dissuaded from developing rationale of my own, first for the reason I have mentioned before that the findings may be in error. Second, just because no meaningful relationships were found in the current work between these two traits and social factors does not mean that there is no participation of environment in their establishment; for, after



all, the 44 social factors against which they were run do not exhaust all environmental effects. Indeed, it may be in principle impossible to exhaust all environmental effects.

Here again, then, I find the absence of guiding and integrating theory a serious shortcoming in this work. Without theory, the results of a vast computer operation are incomprehensible to me and suspect.

I should not give the impression that the authors interpret their findings dogmatically. Throughout the work they raise the theme: "Readers with special professional competence in various relevant fields may not always agree with our interpretation of the meaning of one or another of the statistical patterns which are about to be analyzed. Our aim is not only to answer but also to raise significant questions..." Over and over, they call for further studies to check their findings.

It seems to me that in this case more than most, validating studies are needed. Researchers should approach the Gluecks' findings armed with theory to sift out what may be valuable, and then check those findings with a more carefully designed study. The Gluecks advance such an endeavor by providing possible measures of variables and provocative pilot runs.

**Y**ABLONSKY'S *The Violent Gang* follows in the Chicago sociological tradition of Thrasher's *The Gang* (1927). The data are similar: verbatim statements by gang members and the researchers' personal observations treated after the fashion of anthropological field notes. In *The Violent Gang* the 'people' are teenaged boys on Manhattan's West Side, among whom Yablonsky moved for four years as director of a crime-prevention project.

This book is highly readable. In his first 110 pages, Yablonsky even excites his readers with a description of the incidents, often told in the boys' own hip words, which led up to the 1957 butchering of young Michael Farmer.

Yablonsky's major contribution (for which he won the 1959 DeRoy Award from the Society for the Study of Social Problems) is his image of the vio-

lent gang, how this type of gang fits among categories of teenage social organizations, and what conditions generate it. For the most part, the violent gang is not tangible at all, Yablonsky asserts, but a 'near group,' a fierce name without stable membership or organization, most of whose reality is in the desperate minds of the pathological youngsters who claim to lead it.

Yablonsky may be right; his theory is plausible enough. But the weight of the theory rests mainly on his observations of but two gangs. As the author points out, other observers disagree with him. Yablonsky quite rightly calls for more research, but on these terms:

"The use of personal documents, case materials, and 'live interviews' characteristic of Chicago school research approach are still valid. . . . A model for current gang research might very well be the anthropological field study approach generally reserved for more 'exotic' cultures. The researcher, literally moving into the neighborhood and experiencing the various social forces operating, would be in the most ideal position for gathering the wide range of data that would currently prove most useful toward the development of gang theory. . . . The development of a design for systematically studying the gang problem remains a challenging and formidable research problem."

There is a danger here. While these methods of collecting data may be most appropriate to the subject matter, great care must be taken lest they perpetuate a series of unverifiable opinions. Whatever observational techniques are used, they must be sharable so that other researchers can replicate the experience.

I hope Yablonsky will not rest with his call for more research. I hope he will meet the challenge himself. From my reading of the history of these efforts, it is unlikely that anyone else will.

**T**HE BOOK, *Current Projects*, demonstrates the painful separation of action programs from both theory and data. This useful directory contains descriptions in the practitioners' own words of 500 projects current in 1961, most of

them still in operation. Staff members of the National Research and Information Center on Crime and Delinquency provide several helpful introductions summarizing and interpreting the whole of the projects. Especially relevant to this review is Director Hyman Frankel's accurate observation:

" . . . very few known evaluative studies in the field of delinquency treatment meet the criteria of well-designed experimentation. None of them has been incorporated into a body of theory which would spell out the domain of its application."

This is especially unfortunate because, as Yablonsky has demonstrated, an action program is more likely to be effective if it is organized by some theory, and each action project is potentially a stimulus for further theory construction and a rich source of data.

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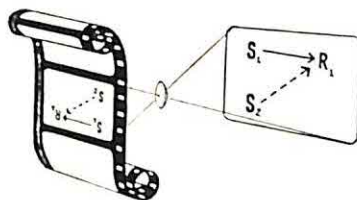
*Criticism is properly the rod of divination: A hazel-switch for the discovery of buried treasure, not a birch-twig for the castigation of offenders.*

—ARTHUR SYMONS



# INSTRUCTIONAL MEDIA

Edited by A. A. Lumsdaine



## Who Shall Provide What Answers?

Stuart Margulies and Lewis D. Eigen (Eds.)

*Applied Programed Instruction*. New York: John Wiley and Sons, 1962.  
Pp. 387. \$6.95.

Reviewed by GEORGE TEMP

Both of the editors of this book, an experimental psychologist and a mathematician, respectively, have for some time been identified with the field of programed instruction. Margulies, who got his PhD from Columbia in 1960, has worked as a psychologist with the Naval Training Device Center and with the Center for Programed Instruction. Currently he is programing director of the Automated Instructional Materials Corporation in New York. Eigen, who is executive vice president of the Center for Programed Instruction, wrote CP's first review of an auto-instructional program (January 1962, 6, 25-27) and was co-author (with Susan M. Markle and P. Kenneth Komoski) of the first volume of *A Programed Primer on Programing* (CP, February 1962, 7, (64-67)). Temp, the reviewer, recently joined the staff of the University of California's Santa Barbara campus as Assistant Professor of Education. He also recently got his EdD from UCLA, preceded by a masters from San Diego State College and two baccalaureates from the University of Minnesota. One of Evan Keislar's first proteges in programmed learning at UCLA, his main interest is the psychology of language, in which he envisions a psycholinguistics-

programed-learning merger as an unbeatable combination.

THE question before the readership is: When is a state-of-the-art book not a state-of-the-art book? Answer: When it is too late and the art is no longer an art but rather a growing body of knowledge. What are needed now are answers to the question posed by the editors in their preface. Not, "Will it work"? but "How can it be used most efficiently"? Although the editors have correctly asked the question (but see below), their selection of 'solutions' has not helped to answer it.

The problem of editorship of a book has been one that has bothered many persons, for if you invite contributions (without pay) you seem bound to take, in general, whatever is sent in. On the other hand if you select articles from the literature there are many problems of obtaining all necessary permissions, repetitions of similar points over and over, lack of available articles on a particular subject, and once again you seem bound to take, in general, whatever is available.

Therefore, I suggest that edited books are usually only appropriate when a field is either so new that a collection

of articles will tend to give form and substance to the diverse efforts of pioneering work (in programed instruction these early state-of-the-art books are classics by now) or when, by some skillful and selective editing, new relationships are illuminated by bringing together articles on a particular topic. Unfortunately, the present work does neither.

It is much too late for a mere collection of pioneering efforts (or is it still too early?) on applying programed instruction, without some unifying topic question or subject. Completeness is impossible, so selection must begin and then the criteria for selection must be made explicit and then be met. "How can it (programed instruction) be used most efficiently"? is not the best question anyway, because efficiency is easily met by giving a subject some programed material and going away. This is surely efficient. The question of interest still is, how can it be used most effectively? But in any case, neither question is answered by the collection of articles presented here.

Obviously, the criticism of the collection of articles as a whole for the announced and unannounced purposes of the editors does not mean that individual articles written in some other context or without the opportunity by the authors to see how it would fit into a larger scheme are not worthwhile contributions in themselves. It is entirely possible for a good article to be lost in a sea of unimportant articles for no other reason than that basic ideas are liable to lose their importance when the reader sees them presented over and over in adjacent pages with little research support, with undue condensation, or in a context where they are not relevant. "Bless this house" is trite or cheap in a corner bar but may be metaphysically important in the humble farmer's cottage.

It is also possible for a poor article to be carried in a good collection with a worthy purpose so that mediocrity is perpetuated along with true worth. The task for the reader in making such decisions is not easy. If good editing has been done, the task is immeasurably easier. In this book the reader is most certainly on his own.



Unfortunately I cannot provide you much help. I have tried, but the overwhelming impression of missed purpose, of hastily written and uncritically accepted articles remains so strong that pointing to the 'good apple' is very difficult.

However, I think that on their own merits at least three articles deserve further reading: Pennington & Slack, "The Mathetical Design of Effective Lessons"; Klaus & Lumsdaine, "Some Economic Realities of Teaching-Machine Instruction"; and an 'oldie' from the defunct *Automated Teaching Bulletin* by Roe, Lyman, and Moon, "The Dynamics of an Automated Teaching System."

There is also a tantalizing acknowledgement in the Preface to Annett and Marsh for "their lucid analyses of the industrial utilization of programmed instruction," but we are not treated to these analyses in the pages of the text and no reference is furnished where they may be found.

I HAVE devoted considerable time to criticism of the editing job done in this volume, for it is here that the editors have direct responsibility. Only they are in the position to see if the elements (contributed articles) go together in a unified whole. Only they are able to see if gaps have been left. Where, for instance, is applied programmed instruction in the schools (since the title of the book is not qualified in any way)? Only the editors can be responsible for the numerous typographical errors in such important matters as authors' names. If the editors of any volume do not edit or unify, what is their function in getting a book together? I merely ask.

As editors they also are directly responsible for the quality of articles included. There is no purpose served by including articles such as those by Hosmer & Nolan (acceptance of the null hypothesis) or Hain & Holder (where equality of groups was not pre-tested in any way whatsoever, and control and experimental groups were post-tested seven months apart.)

If this book were to be widely read by people concerned with applying pro-

grammed instruction I believe it would be a long time before they again looked for help in solving their problems in meeting practical problems of application. This is not because such help is not available from experts and researchers in programmed instruction, but because the selection here does not reflect the true nature of the answers available. I look forward to a book,

not necessarily edited, that addresses itself to the problems of application with a unified theoretical framework, strong research support, and a large dose of practical experience in using programmed materials. There are men with such experience. Why aren't they writing? The answer is, they're probably too busy!

## ON THE OTHER HAND



### MAJOR AIM MISCONSTRUED

May I offer a point of fact relevant to Julia S. Brown's review of *Day Hospital* (CP, May 1963, 8, 192 f.).

Dr. Brown states that my "major aim in providing an overview of this pioneering service is to make possible some evaluation of its therapeutic effectiveness." To the best of my knowledge, nowhere in this volume did I say that the aim was to evaluate the effectiveness of day care. On the contrary, I made a special point of saying that this work offers no solid evaluative data. I said that the study "firmly reveals that a broad range of individuals may receive care and treatment in an organized program of partial hospitalization. Yet it leaves unanswered questions concerning effectiveness from the standpoint of therapeutic outcome. We emphasize that such answers require careful biostatistical investigations utilizing the cohort method with adequate comparison groups and long term follow-up."

I join Dr. Brown in wishing that we had sound data concerning effectiveness of treatment modalities. The absence of such data in my volume should not, however, be judged in terms of a false impression that my "major aim" was to offer an "evaluation of its therapeutic effectiveness."

BERNARD M. KRAMER

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### PROSHANSKY ON ZIGLER ON WITKIN

It is a relatively simple matter for a book reviewer to vitiate a systematic research effort by preaching the gospel of the "true" theoretical and methodological

approach. In such instances we learn a great deal more about the reviewer's predilections in these respects than we do about the nature and scope of the research in question. Zigler's review of *Psychological Differentiation: Studies of Development* by Witkin and his associates (CP, April 1963, 8, 133) is a case in point. At first we were sorely tempted to write an elaborate statement which focused on the special biases of the reviewer, defending not so much the research itself but the importance of providing a comprehensive and thoughtful account which does justice to a ten year research effort. However, on second thought, it seemed more appropriate to permit Zigler's review to be judged in the light of another critic's evaluation of the Witkin endeavor. Thus, Zigler tells us:

"our understanding of cognitive or perceptual styles would be enhanced by placing them within a developmental framework. Indeed many investigators are making just such an effort. The concept of differentiation could have been used as a bridge to join Witkin's interesting empirical work with that of such investigators as Piaget and Werner" (p. 135).

In more than one place Zigler cites the work of Werner as indicative of the "proper" developmental approach, while proclaiming that Witkin and his associates arrive at conclusions that run counter to the thinking and orientation of Werner. The important question then is what does Werner think of these conclusions. Considering that Werner wrote the Foreword to the Witkin volume, this was a relatively simple matter to check.



The CP reader can now judge for himself:

"This is a book of prime importance. It gives reassurance to those of us who believe that the understanding of man cannot advance through the mere gathering and ordering of data, but only through the search for facts under the guidance of theoretical notions. These explorations into the little known represent an achievement of no mean proportions: solid in method, exciting in outlook, profound in meaning" (p. vii).

HAROLD PROSHANSKY  
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WITKIN ET AL ON ZIGLER  
ON WITKIN ET AL

In reviewing our book, *Psychological Differentiation*, (CP, April 1963, 8, 133 f.) Dr. Edward Zigler informs the reader only of those isolated items of content that serve as vehicles for his critical comments. The result is a distorted representation of the nature and range of the contents of the book and the program of research it describes. It is our purpose to correct some of these distortions.

*Psychological Differentiation* is a second major report of research in progress. During the period of work described in the book, "differentiation" served as a construct to conceptualize communality in behavior in diverse areas of psychological functioning. More specifically, the "differentiation hypothesis" proposed that individuals are likely to function at a more differentiated or less differentiated level in many areas of psychological activity, making for self-consistency in behavior. It predicted an association among such characteristics of a high level of differentiation as: an articulated way of experiencing the world, which includes experience of objects as discrete from field and fields as structured; a differentiated self, reflected particularly in an articulated body concept, and a developed sense of separate identity, based on stable internal frames of reference; and, finally, the use of such relatively structured, specialized defenses as intellectualization and isolation. Some of the specific ways in which these characteristics of developed differentiation may manifest themselves in behavior were identified as a basis for conducting our own research and for interpreting results by others.

The first 270 pages of *Psychological Differentiation* present the results of a

large mass of studies by ourselves and others which bear upon the prediction, made from the differentiation hypothesis, of self-consistency in behavior across diverse psychological areas.

The systematic nature of the research carried out on the differentiation hypothesis is not represented in the review of *Psychological Differentiation*. So little is the reader informed of the strategy of the research that he might easily conclude, from the review, that there was no strategy. The reviewer does not even indicate the central role of the differentiation hypothesis in guiding many of our investigations, but gives the impression that it was invoked merely as a post-hoc explanatory scheme. Actually, our theoretical framework emerged gradually in the course of our studies; it both guided investigations and was in turn modified by their results. Neither does the reviewer convey the scope of the evidence brought to bear upon the differentiation hypothesis, coming from many studies, using a great diversity of techniques, conducted by investigators holding a variety of biases, and working in many different laboratories.

Even in the area of perceptual field dependence, where the research is most highly developed, our work and that of many others is dismissed in such misleading terms as "seines psychological waters" and "correlational fish" being caught. The implication that we were engaged in a fishing expedition scarcely does justice to the detailed accounting in the book of a long history of formulating and testing a series of explicit hypotheses, leading to progressive refinement of the field-dependence concept. Before arriving at our present interpretation of the field-dependence dimension as reflecting extent of ability to overcome an embedding context, a number of alternative hypotheses were considered and rejected after experimental test. One was that individual differences along this dimension reflect differences in sensitivity to bodily sensations; a second was that they represent differences in accuracy of perception; another that they reflect ability to overcome distraction.

Still another hypothesis examined and rejected was that field independence might be a matter of superior general intelligence (p. 59). This hypothesis deserves particular attention because of the reviewer's treatment of the issues involved. Significant correlations (reported in the book) had repeatedly been found between IQ and measures for our perceptual tests, especially the embedded-

figures test. In pursuing this finding, we considered two alternative hypotheses: (a) the relation with total IQ is a function of general intelligence; in that case, substantial correlations would be expected between field dependence and a wide variety of intellectual functions; (b) the ability to separate item from context expresses itself in intellectual activities as well as in perception and the relation with total IQ is carried primarily by subtests of standard intelligence tests featuring this ability. In that case, particularly high correlations would be expected between these subtests and field dependence, defining a common factor of overcoming embeddedness. Several studies reported in the book were specifically designed to test these alternative hypotheses. In one study, for example, of the three major factors derived from the Wechsler scales by Cohen and others, the tests of perceptual field dependence loaded only one, the analytical factor as we call it, best represented by the block-design, picture-completion and object-assembly subtests, which seem to have the same task requirements as the perceptual tests. The perceptual tests did not appear on either of the other two factors, the attention-concentration factor (represented by the digit-span, arithmetic and coding subtests) or the verbal comprehension factor (represented by the vocabulary, information and comprehension subtests). Using scores for these three factors from the WISC, we found correlations with composite perceptual scores of .66 ( $P < .01$ ), .18 (not significant) and .23 (not significant). Evidence such as this suggested that the cognitive style we have been studying is in fact tapped by particular subtests of the Wechsler scales, and it is performance on these subtests which is primarily responsible for the correlations found with total IQ. These and a variety of similar findings forced rejection of the "general-intelligence" hypothesis.

After a highly selective recounting of our findings on intelligence test performance, the reviewer in effect dismisses the whole body of work to which we and numerous investigators working with the field-dependence dimension have contributed, in the following simple fashion: "It is the reviewer's view that the empirical relationships found between field-dependence measures and many of the scores employed by Witkin are due to the common relationship between all these scores and *general* (our italics) intelligence as defined by standard intelligence tests." To buttress this "noth-



ing but" position, the reviewer states that "not once does Witkin present the relationship between his many measures and his subjects' total intelligence scores." This is factually incorrect (see p. 60). Moreover, without telling the reader of our explicitly stated belief that by presenting correlations with "sub-IQs" we are providing more detailed information than by correlations with total IQ, this statement may give the impression that we are withholding information. The reviewer further states: "Even more embarrassing is the finding that in one of his groups a highly significant correlation was found between an index of the field-dependence measures and the Stanford-Binet Vocabulary score." He fails to mention, however, that this correlation is selected from 9 reported between perceptual and verbal scores, and is one of two that reach significance.

The evidence on self-consistency, as suggested by the differentiation hypothesis, came from the examination, in each of several psychological areas, of a wide range of behavior, explored by a variety of techniques (laboratory tests, questionnaires, behavioral observations, personality inventories, attitude scales, projective techniques and interviews). It is important to note this feature of the research because of the impression given by the reviewer that in the studies cited assessment was essentially limited to clinical techniques and that the validity issue was mishandled.

The range of specific behavior examined may be illustrated by mentioning those considered, among others, in evaluating sense of separate identity: solicitation of help while taking the TAT; adherence to instructions in performance of a routinized task; learning of material incidental to that assigned by an authority; responses to rating scales evaluating attitudes of dependence and independence; age on leaving the family for marriage, school, etc.; attentiveness to faces of others; conformance to group pressure; suggestibility, investigated by traditional laboratory methods; fluctuations in self-description in different social contexts. The behavior sampled in evaluating nature of controls and defenses from the standpoint of differentiation included: ability to channel the ideas and feelings generated by a TAT card showing an aggressive act; Q-sort and adjective check list characterizations of behavior; planfulness vs. impulsiveness in solving mazes; GSR lability; dream recall; reactions to sensory isolation; lability of feeling during experimental tests;

failure to refer to provocative aspects of TAT pictures.

In the face of a large group of studies on controls and defenses, we find that the only reference in the review to these studies is the statement: "... one wonders whether responses to another single TAT card measure the child's characteristic mode of handling aggression . . ." This single reference may carry the highly misleading implication that we had based all our conclusions in the area of controls and defenses on a single TAT story. Even more serious is the erroneous impression created that we had based our conclusions on clinical techniques (when in fact studies using a wide array of non-clinical methods are described), and that the validity of the clinical measures derived depended almost entirely on "clinical folklore."

With regard to the measures derived from projective techniques, they most often referred to behavior considered *in its own right*, and involved no complex clinical inference. For example, when aggressive themes given by some children in response to a TAT card are more highly rationalized than those given by other children, the proposal that these differences reflect differences in handling aggression in fantasy productions is little more than a descriptive statement of the subject's behavior. There is no issue of inferring from such behavior, taken by itself, how the child behaves elsewhere, as the reviewer implies in his reference to "*characteristic* (our italics) mode of handling aggression" in the quotation given above. Where inferences were made about a characteristic mode of impulse control, they were based on empirical relations with measures of impulse control from a variety of situations. As another example, in our principal use of the Rorschach, we considered the degree of structure of the percepts produced by children. In doing so we were simply describing the behavior that occurs within the test situation itself. Under these circumstances, the question of validity—in the reviewer's sense of one measure of behavior being checked against another "better" measure—is not particularly at issue.

There are several instances where we did suggest more complex interpretations of test scores: for example, human figure drawings were scored for sophistication of the figures drawn, and it was hypothesized that differences in sophistication are in part a function of differences in articulation of the subject's body concept. In none of these instances, how-

ever, was evidence limited solely to a particular score, let alone to projective test data. In every instance, many other studies are cited, which bear on the same dimension, using other techniques. We believe that the variety of techniques used provided considerable construct validity for the dimensions of articulation of body concept, sense of separate identity, and structure and specialization of defenses. Of course, the reader must judge for himself the relevance of these studies. By omitting this variety from his account, however, the reviewer apparently dismisses the possibility of construct validity as a method of pinpointing the meaning of a concept and gives the reader a misleading impression of the kind of evidence to be found in the book. To deny the use of construct validation is to discard much of the recent research in psychology. The use of this kind of validation needs no defense by us.

A second major area of research reported in *Psychological Differentiation* was directed at the origin of the observed broad individual difference in differentiation.

This extensive work, which requires almost 100 pages of the book to describe, is treated by the reviewer as a vehicle for criticizing the contamination of the interviews conducted with mothers because of the knowledge the interviewer thereby obtained about the child himself. The reviewer tells nothing of the plan and rationale of the study of origins, but has chosen instead to cite only the first of an evolving series of studies, the one in which mothers were interviewed. His charge of contamination and of the use of global methods of study may be seen in quite a different light when the reader is given an opportunity to learn of our own view of the status of the interview results, and of our overall research strategy. This strategy entailed movement toward elimination of sources of information about the child in order to permit evaluation of the mother alone; toward more objective methods of evaluation; and toward greater specificity in hypotheses.

This strategy is explicitly defined in the book. A detailed account is given of an initial study, specifically identified as a first step, aimed at obtaining insights and information for more explicit hypothesis development. In this study we focused by design on mother-child interaction. This objective could hardly be achieved without talking to the mother about her child. Global ratings



were made of whether mother-child interaction tended to foster or interfere with development of differentiation in the child. These ratings related significantly to characteristics of differentiation in children in an original study and two cross-validation studies. In the latter studies, global ratings were again made. These ratings, however, were more closely anchored to indicators developed in the initial study. In general, mothers whose children gave evidence of limited differentiation interacted with their children in a manner which interfered with the children's separation from them and which failed to contribute to the child's development of internalized standards.

Studies in which mother-child relations were explored from the child's viewpoint through interviews with him and through fantasies in response to TAT pictures about the exercise of the parental role gave results consistent with those obtained from interviews with mothers and further extended our conception of mother-child interaction.

The next step in the evolution of our research on the origins problem was an attempt to identify more specifically and separately the constituents of the large complex of factors that went into the classification of the interviews with the mothers. The insights gained from these interviews, as expected, now made possible the formulation of more specific hypotheses. As an illustration, one of the hypotheses tested was that undifferentiated children are likely to have mothers who are themselves relatively undifferentiated. To test this hypothesis we evaluated mothers by means of two of the techniques used in assessing differentiation in the children, the embedded-figures and figure-drawing tests.

Against the reviewer's particular criticism of contamination, it is worth noting what we ourselves wrote about this study (P. 315): "This study . . . provides an opportunity to assess the mother as a person directly, apart from her role as mother. Further, compared to the complex clinical judgments involved in ratings of interviews, the measures derived from the embedded-figures test and figure drawings are based on more limited and therefore more easily identifiable information."

As reported in the book, an independent investigator, Seder, working elsewhere, also formulated and tested specific hypotheses derived from our conceptualization and findings on mother-child relations. Seder considered highly specific items of maternal behavior, in contrast

to the global ratings we employed for interviews with mothers. Her findings were consistent with our own. Such consistency is of particular value since both her population and methods were different from ours.

The final step we took in the series of studies of origins was to push the study of mother-child interrelations to an earlier period of life and to begin an exploration of the role of early characteristics of the child in the development of differentiation. For these purposes we restudied, by our methods, in the 6-to-9-year period, a group of the children observed in infancy by Escalona et al., and we re-interviewed their mothers. As noted in the book, the data from this investigation are now being analysed.

A third and final area of research described in *Psychological Differentiation* is stability of relative level of differentiation during growth and in adulthood. Longitudinal study of children followed from 10-17 years of age suggests, from results thus far analysed, marked stability during this period of growth. Stability in adulthood was also impressive; and the cognitive style with which we have been concerned was not altered by such experimental interventions as stimulating and depressing drugs, stress due to anticipated heart surgery, or special training. The only reference to the issue of stability in the review occurs in the context of the criticism that we are interested only in stability and not in change, and that we therefore "wear awkwardly the mantle of a developmentalist." That the maintenance of stable patterns during growth is relevant to developmental issues needs no defense. Moreover, the reviewer overlooks the relevance of our work on origins, concerned with the progress of individual children toward differentiation and the factors affecting such progress, to issues of change.

Finally, the reviewer criticizes us for our handling of the differentiation concept and developmental theory. He states that the only lower-order construct we derived from the differentiation notion was decontextualization, thereby disregarding our other salient lower-order constructs, as articulated body concept, sense of separate identity, and structured, specialized defenses. He also states that we have come to the "completely untenable conclusion that a variety of verbal behaviors are at best minimally related to developmental processes" which "runs headlong into the impressive work of Werner and Piaget." This conclusion, imputed to us, absurd on the face of it,

in fact runs headlong into our own conclusion given in summary on P. 198: "Though numerous questions are left unanswered by the findings reported, the evidence does indicate that the development of at least some kinds of verbal skills may follow a *different* (our present italics) pathway than the development of mode of field approach and other characteristics of developed differentiation."

The reviewer further considers that we have misused Werner's concepts and regrets that we had not tried to make a bridge to the work of Werner and other developmental theorists. These comments appear particularly authoritative since the reviewer is described in advance as having been in Worcester, where "he came in contact with Werner's approach." It is a little surprising that in his lengthy discussion of these matters the reviewer fails to mention that Heinz Werner wrote the Foreword to the book. It may therefore be appropriate to quote one of Werner's comments, from the Foreword, which is particularly pertinent to the criticisms made by the reviewer (P. vi):

"Now, the developmental conceptualization to which the authors subscribe is for them not a mere means of post-hoc theoretical interpretation of gathered facts; with the instinct of the explorer bent toward the acquisition of empirical knowledge they transform the genetic-theoretical axiom of self-nonsel-differentiation into a hypothesis—the "differentiation hypothesis"—which in their skilled hands becomes capable of channeling the inquiry toward significant particular questions, as well as recruiting powerful and relevant experimental methods; moreover, in spite of its generality, the hypothesis is sufficiently incisive to be validated and tested against the findings without undue ambiguity."

H. A. WITKIN

R. B. DYK

H. F. FATERSON

D. R. GOODENOUGH

S. A. KARP

State University of New York

MORE ON ZIGLER ON WITKIN

Edward Zigler's review of *Psychological Differentiation* by Witkin, Dyk, Fater-son, Goodenough and Karp in the April, 1963 issue of *CP* gives a limited and distorted, as well as generally unsympathetic, picture of the purposes, methods, findings and theoretical position of what is, to my mind, a major contribution of a



vigorous and dedicated research group.

*Psychological Differentiation* is the second major product of a research program now almost twenty years old. Within a revised and elaborated conceptual scheme—the “differentiation hypothesis”—the volume continues the earlier work on field dependence-independence by reviewing numerous studies, from within and outside their laboratory, extending the range of relationships among perceptual, cognitive and personality functions which distinguish more and less differentiated people. Additional sections report longitudinal evidence of considerable consistency in differentiation among individuals over time, and explore the origins of greater and lesser differentiation in early development, particularly in the mother-child relationship.

Zigler is critical of Witkin's theoretical effort (“which promises so much and delivers so little”) but nowhere is the differentiation hypothesis described, nor is there mention of the supplementary constructs—the sense of separate identity, defined body concepts, specialized defenses, etc.—important to the understanding of Witkin's view. The judgment that differentiation reduces to decontextualization in Witkin's usage, and that this brings it into conflict with the work of Werner and Piaget, is unjustified. The reviewer's final contention that “more than the loose application of the differentiation concept is required before Witkin's findings can be incorporated in a developmental framework” (as exemplified by the tradition of Werner and Piaget) is denied by Werner's own opinion in his Foreword to the book, where he noted that the work is in the “new spirit” of developmental theory.

The largest part of the book details the variety of perceptual, cognitive and personality functions which characterize greater and lesser differentiation. An impressive network of relationships are reported, consonant with the differentiation hypothesis. The reviewer feels that it is safe only to conclude that if one “seines psychological waters” with Witkin's techniques, “correlational fish” will be caught. His own view that the empirical relationships reported between field-dependence and many other measures are due to common relationships with general intelligence, and that one could do as well in the correlational fish-catching business with a general intelligence test, runs counter to considerable evidence. Zigler's specific contention that Witkin does not once present the relationship between his measures and his subjects' total intelli-

gence scores is literally untrue (p. 60).

A number of areas treated at length by the authors are not mentioned in the review, notably the studies of controls and defenses, sex differences, pathology, and the origins of differentiation in early mother-child experiences. About a quarter of the book reports the studies of early origins, but all of this work gets mentioned only in the ambiguous statement that field-dependence measures were correlated with “the boys' mothers' interview behaviors.”

From the review, it seems that the research depended largely on clinical methods, for which little evidence of validity is given except as derived from “clinical folklore.” In fact, a wide variety of psychometric and experimental procedures were used, too numerous to detail here. Although of prime importance, clinical techniques (e.g., projective tests and interviews) were generally used circumspectly, most commonly to yield particular measures required by a particular hypothesis, to which end the validity of the technique (e.g., Rorschach) as used in general clinical practice is not in issue. Of greatest importance, however, is that the research as a whole, by pursuing the concept of differentiation through a network of empirical relations among various kinds of measures, exemplifies the logic of construct validity.

In addition to suggesting a lack of concern with validity, the reviewer also suggests that relationships reported might be inflated by contamination of some scores derived from interview data. Zigler's argument is not convincing, and I am more impressed with the evidences of the authors' painstaking concern with measurement problems. The situation seems better summarized by Wayne Holtzman in his review of the same book: “Witkin has taken unusual precautions to safeguard against contamination across different techniques of assessment and to check the reliability of his measures” (*Science*, December 7, 1962).

Certainly, thoughtful criticism and honest appraisal should be welcomed in *CP*, but there should be enough detail about the authors' intentions, efforts and accomplishments for the reader to have a fair notion of what is in the book as well as the reviewer's opinion of it. To balance the picture, and encourage independent evaluation by *CP* readers, I have tried to call attention to some of the shortcomings of the review.

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*National Institute of Mental Health*

#### OBJECTION TO

#### OBJECTIONABLE (?) OBJECTIVES

In his review of Mager's *Preparing Objectives for Programed Instruction* (*CP*, March 1963, 8, 123-24), Gotkin considers “the stated objectives trivial and out of line with the title and content of the book.” It seems to me that Mager has adequately fulfilled a programmer's obligations to state his objectives clearly and unequivocally. When this is done, their utility depends on whether anyone wishes to shape such behavior. A major obstacle to prospective programmers is their inability to talk about objectives in terms of performance. After they learn to do this, their programming skills improve. Thus I find Mager's objectives extremely worthwhile. Gotkin might have pointed out that others were free to evaluate the utility of the objectives for their own purposes.

Gotkin finds that “intelligent readers can make correct choices without reading the test,” and implies that this is a serious fault. However, he also finds that “the best standard (is) attainable without reading the test.” Fine! Isn't the trick of a program to shape behavior? If so Mager has done it, and Gotkin has “proved” it, if his results can be accepted at face value.

Gotkin also states: “However, the learner's ability to make correct choices, though necessary, is not a sufficient indication that he can prepare meaningfully stated objectives.” What evidence does Gotkin have that correct choices are either necessary or sufficient? Certainly the scrambled-book technique does not require this assumption. Moreover, Mager makes no claim that a person going through his book will be able to prepare “meaningfully stated objectives.”

Gotkin's “crude but powerful (sic) empirical checks” on the program involved six secretaries as subjects. Although he claims they were “naive” (his quotes), they wised up rapidly. Four of the six reached criterion performance “on the self-test without even going through the program.” It seems to me that Gotkin rather than his secretaries may be naive. My hunch is that, unbeknownst to Gotkin, they already had the criterion behavior in their repertoires before the “study” began. Unfortunately, Gotkin did not include a pretest in his “crude but powerful” check, so no one will ever know. Gotkin commends his procedures, which he says “can be used to evaluate a program with only a few test subjects” as a model for “consumers.



especially school personnel." If this is the level at which specialists in the area are operating, God help the school people. Altogether, it seems to me the evidence suggests that Gotkin not Mager nor the student ought "to return to page 1."

RICHARD E. SCHUTZ  
Arizona State University

#### ERRATUM

On page 215 of the May, 1963, issue, column one, line 9, Gauldner should read Gouldner.  
—F. H. S.

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Reliance on statistics tends to separate the scientist from an intuitive sense of his material. Instead of giving us a direct sense of cause and effect, they express such relationships by backhandedly remarking how unlikely it is that A was related to B by pure chance.

—ROBERT S. MORISON





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Reliance on statistics tends to separate the scientist from an intuitive sense of his material. Instead of giving us a direct sense of cause and effect, they express such relationships by backhandedly remarking how unlikely it is that A was related to B by pure chance.

—ROBERT S. MORISON





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# GRADUATE EDUCATION IN PSYCHOLOGY

Report of the Conference on Graduate Education in Psychology, sponsored by the Education and Training Board of the American Psychological Association and supported by a grant from the National Institute of Mental Health, U. S. Public Health Service; held at Miami Beach, Florida, November 29 to December 7, 1958

Prepared by the Editorial Committee:

Anne Roe, *Chairman*,

and

John W. Gustad, Bruce V. Moore,  
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Price \$1.50

THE AMERICAN PSYCHOLOGICAL ASSOCIATION  
Dept. Grad  
1333 Sixteenth Street, N.W.  
Washington, D. C. 20036



# Contemporary Psychology

A JOURNAL OF REVIEWS

October, 1963

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*Contemporary Psychology: A Journal of Reviews* is published monthly. The subscription per year is \$10.00, foreign \$10.50, single number \$1.00.

Published by the American Psychological Association at 2210 W. Dallas, Houston, Texas and 1333 Sixteenth St., N.W., Washington 6, D. C.

Second-class postage paid at Houston, Texas and at additional mailing offices. Accepted for mailing at special postage rate stated in section 132.122, Postal Manual.

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# Contemporary Psychology

A JOURNAL OF REVIEWS

VOLUME XIII

October, 1963

NUMBER 10

## History Written Backwards

Leo Postman (Ed.)

*Psychology in the Making: Histories of Selected Research Problems.* New York: Knopf, 1962. Pp. v + 785 & i + xxxiv. \$9.75.

Reviewed by ERNEST R. HILGARD

Leo Postman, who edits and contributes to this book, took his PhD from Harvard University then taught at Indiana University and back at Harvard before moving to the University of California at Berkeley where he is now Professor of Psychology and Director of the Center for Human Learning. Most psychologists know about him and his work in the area of perception and learning. The reviewer, Ernest Hilgard, needs no introduction at all. But those who haven't renewed acquaintance with him recently perhaps should know that although he has been identified in the past very largely with the psychology of learning, his interests now have veered to the study of motivation and personality, with present efforts spent upon a major investigation of hypnotic phenomena in relation to personality dynamics. He says as a writer of an introductory textbook (CP, May 1963, 8, 183) it is hard to deny interest in anything psychological. His interest in learning, though perhaps not preeminent, is not dead, however, for he is presently editing a yearbook on theories of learning for the National Society for the Study of Education.

IF you wish to embarrass a historian, just ask him about the lessons of history. His reply will usually intermingle two ideas: one, that the events

of history are unique, so that no laws or generalizations can be derived from them, and, second, that you cannot understand the present or make wise judgments about the future unless you are informed about the past. The latter idea implies that there are, indeed, some generalizations to be made. This mild confusion about the lessons of history is reflected in the different ways in which history is written, as accurate chronology, as the influence of great men as they succeed each other, or as the inevitable march of events in which the historian's task is to unravel the causal strands.

All these problems of history in general are reflected also in works on the history of psychology. The classical product of Brett, in three large volumes, takes the ponderous approach of working up from the ancient past all that has been said that appears relevant to psychology; Boring, aware of the interaction between man and *Zeitgeist*, stresses the men who made modern psychology, although he moves somewhat by decade and somewhat by geography and by topic; Murphy moved more largely according to the chapters of contemporary psychology. To these approaches (and others that could be mentioned, such as collections of classical writings) has been added this new book edited by Postman. Here,

after an introductory chapter concerned with the role of history, there follow 11 chapters on diverse topics, each by someone who is a contemporary researcher in the field, looking backward from where we now are to see how we got here and where we are going. Of the 14 authors, 12 were at the time of writing members of the Department of Psychology at Berkeley; the remaining two (Hochberg and Dukes) are claimed as alumni.

The introductory chapter (by Crutchfield and Krech) is a fascinating essay on reasons for studying the history of psychology, some conceptions of history, and some of the things to be learned from the "case studies" of the succeeding chapters. The chapter is thoughtful, and ought to arouse lively discussion. I doubt if psychology, in being concerned with its own history, is as unique as the authors believe, or for the reasons they assert; they have fallen into the historian's fallacy of asserting a generalization without examining the evidence for it, and then giving plausible reasons why it must be so. All the same, they make a good case for the psychologist's continuing interest in the history of his science.

It would be foolhardy to try to review each of the chapters according to its success in doing what it sets out to do; obviously some of the authors have a better "feel" for history than others have; some have so much investment in a given side of a controversy that, though they strive for historical detachment, their loyalties show through. Some chapters have a freshness and liveliness that others do not have. The whole, however, comes off remarkably well.

One matter that came to interest me more and more as I worked my



way through the book was the influence of *breadth of topic* on the writing of its history. Let me offer a rough classification of topic by size (Table 1). I have no hesitation in saying that I found myself somewhat more enthusiastic about the chapters that covered the more limited topics. Why? I do not believe that they were any more competent than the others, hence there must be another explanation. The explanation may be the obvious one that we all know a good deal about the broader topics through the available sources and reviews, while the details about the more limited topics are likely to be newer to us. There is something more, however, and this may be crucial: a single line of investigation, narrowed topically, shows the thought processes of the investigator more clearly, as he criticizes an experiment and improves upon it. For example, Rosenzweig's chapter on the mechanism of hunger and thirst not only throws light upon the persistence of the local stimulation theory of hunger and thirst (Cannon) in the face of much contradictory evidence, but shows also how step by step the more central theories have advanced both through hypothesis-testing and through capitalizing on chance discoveries and observations. This kind of detail would not be possible were the chapter to be concerned, for example, with physiological drive in general. The same can be said of Postman's chapter on rewards and punishments in human learning, which limits itself to Thorndike and his critics, and hence can go into the detail needed to account for the criticisms of the spread of effect and its experimental support through later studies when earlier ones seemed to have killed it. The MacKinnon-Dukes chapter on repression is better than it would have been had it attempted to cover all experimental tests of Freudian theory; this applies also to the entertaining chapter by McKee and Honzik on mammalian sucking behavior as illustrative of the nature-nurture problem. There is a danger, too, in being too limited; I have a little the feeling that Gough's chapter, while an excellent critical one on clinical vs. statistical prediction, is more contemporary than historical, even though some

Table 1  
COMPREHENSIVENESS OF CHAPTER TOPICS

	<i>Broader Topic</i>	<i>More Limited Topic</i>
Part I: Biological Foundation	2. Cortical localization (Krech) 4. The inheritance of behavior (McClearn)	3. The mechanism of hunger and thirst (Rosenzweig)
Part II: Perception, Learning, Memory	5. Nativism and empiricism in perception (Hochberg)	6. Rewards and punishment in human learning (Postman) 7. Memory for form (Riley)
Part III: Individual Differences and Personality	8. The nature and measurement of intelligence (Tuddenham) 12. Attempts to understand hypnotic phenomena (Sarbin)	9. Clinical vs. statistical prediction (Gough) 10. The sucking behavior of mammals (McKee and Honzik) 11. Repression (MacKinnon and Dukes)

references go back as far as 1923. (Perhaps this reviewer's age is showing when the 1920's seem contemporary: I suppose 40 years is enough to give a topic a history!)

The two chapters that give the broader-narrower topic comparison most clearly (Hochberg on nativism and empiricism in perception, and Riley on memory for form) do not support my preference for the smaller topic very well, because Hochberg has an admirably specific chapter on a large topic, epitomizing the whole history in 29 representative cases, using only 10 more pages than Riley does when he moves through the topic of memory for form. Still my preferential response is in favor of the Riley chapter as an account of history; the Hochberg contribution is a bit overwhelming, like a handbook chapter.

I could both say enthusiastic things and make critical remarks on each of the remaining chapters, but in a short review on such varied material elaborate substantive criticisms are hardly in order. I regret that Krech confined himself so closely to the cortex that he sidestepped the old-brain new-brain

problem and didn't convey any of the excitement of recent developments with respect to the reticular formation, the limbic system, and the rest (e.g., the EEG isn't in his historical account at all). McClearn does what most genetically oriented psychologists do when they study the nature-nurture problem in man: they rely on correlation coefficients instead of means, thus favoring heredity over environment. The environment can bring great changes in size (and perhaps in intelligence) without disturbing parent-child correlations; the Iowa arguments were always based on mean changes, the Stanford-Minnesota ones on correlations, and this in part is the reason they did not agree. Tuddenham fails to come to grips with the heroic (though undecisive) attempts to develop culture-free or culture-fair tests of intelligence, with only a tangential reference to the fact that "anthropologists repeatedly warned that tests depend heavily upon familiarity with elements of middle-class urban culture." Sarbin's review of attempts to understand hypnotic phenomena almost completely ignores the concerns of psychoanalysts; he mentions Breuer and Freud only briefly and completely bypasses Schilder, Kubie, Brenman and



Gill, and other analysts who might well have been included in a historical summary.

There is so much richness in the chapters that it is capacious to search out their limitations. The book will be rewarding to anyone who wishes to learn

about contemporary psychology as well as to learn about its history. It belongs as much in the advanced general course as in the one labeled history. My guess is that its material will be slipped into the lectures in many other courses as well.

## Selective, Historical, Biographical and Contemporary Psychology

George A. Miller

*Psychology, the Science of Mental Life.* Harper & Row: New York, 1962

Pp. viii + 388. \$6.95. \$4.95 (text).

Reviewed by DON E. DULANY, JR.

*The author, George A. Miller, came forth, from the University of Alabama twenty or so years ago and has remained in the vicinity of Cambridge, Massachusetts, ever since. He received his PhD from Harvard in 1946, stayed a while there, moved down the river to MIT, and then back to Harvard where he became and is now Professor in the Psychology Department's Center for Cognitive Studies. He started his career in psychoacoustics, moved on to research in communication, then to processing of information and soon to a concern with the problems of thinking and cognition. The reviewer, Don E. Dulany, Jr., is a University of Michigan PhD who spent some time in the army after receiving his degree and then went to the University of Illinois where he is now Associate Professor of Psychology. He has taught courses both in introductory psychology and in contemporary viewpoints. His own research is concerned with propositional verbal control.*

MILLER begins his preface by saying that "In this book I have tried to explain what the science of psychology is and how it got that way." The result is the only book of its kind that I know of in psychology. Where else can one go for a personally selective, historical, biographical, and even fairly

current introduction to psychology? To produce this in 388 pages, the author has been obviously, and avowedly, selective; at the outset he mentions a neglect of the more technological and applied aspects of the science. But it helps more to know that the book is one of the Harper Modern Science Series, edited by James R. Newman, that E. G. Boring suggested the project, and that Miller wrote the book while teaching a Harvard General Education course entitled "Psychological Conceptions of Man."

Every major topic is presented in its historical context along with a biographical sketch of the principal historical figure—Wundt, James, Galton, Pavlov, Freud, and Binet. Those names suggest a fairly conventional selection of the historical material, but the selection of the current material is less so. The current topics are mostly those that do have a history, that have always called for a little more thoughtful analysis, and that seem especially to interest the author. We are shown that the problem of attention set by Wundt's metronome lends itself to informational analysis. James' qualitative descriptions of the stream of consciousness lead by natural contrast into a discussion of scaling and psychophysics. "What do we mean by 'consciousness'?" is given a modern answer of sorts by studies of

brain waves, dreams, the reticular system, and sensory deprivation. And ancient questions of the permanence of memory are treated in the light of mnemonics, Penfield's operations, analogies to storage of information in computers, and recent experimental distinctions between short and long-term memory.

The treatment of each topic also has a rather distinctive character for an introduction to psychology. Pavlovian conditioning is not presented as a laboratory curiosity, or something cute to watch for when the fraternity dinner bell rings, or even as the "simplest form of learning." It is presented as one objective, materialistic answer to theories of the association of ideas. There is little that is detailed in the treatment of motivation and learning. But the reader is thoughtfully led from James' list of instincts through Cannon's concept of homeostasis to servomechanisms, and from hedonism through the Law of Effect and pleasure centers to decision theory. Where there are problems—for example, the adequacy of biogenic theories or of an S-R explanatory language—these are the focus of concern. In fact, most of the topics are presented as problems—problems of persuasion, of illusion, of hypnosis, of clinical and statistical prediction, or of the emergence of voluntary control and conservation of substance. And unlike most introductions to our science, this one is well-laced with iconoclastic good sense.

NO ONE will miss the sub-title of this book, but it is more wistful than realistic, I think. Psychology *could* be The Science of Mental Life, but is unlikely to be as long as our comfortable operational bromides prevent a clear look at important new analyses in the philosophy of science. What we call a mental process is a hypothetical process—whatever its ultimate ontological status. Like any hypothetical process, a mental process is beyond the reach of a proper operational definition, but can be rigorously specified within a clear and supportable theoretical network.

The movement of ideas, the excitement of a problem, the importance of



important problems—all these things are conveyed very well. Psychology as an experimental enterprise is not, I think, conveyed so well, if the aim is an explanation of the science of psychology and how it got that way. Granting the limitations of space, and of the intended audience, I would still wish for more experimental detail, perhaps only illustratively presented. The price of a direct and gracefully idiomatic style is, as usual, an occasional glibness, and the reader is sometimes persuaded when he should be convinced. But a beautifully written, semi-popular introduction primarily to experimental psychology is a welcome marvel.

Would this be a suitable basic textbook for the introductory course? Prob-

ably not for the usual comprehensive course. Neither would it be the choice of lecturers who try to present something like what this book presents—a reflective, discursive examination of selected problems, leaving a more detailed and comprehensive presentation for a text-handbook. I could, however, recommend the book to a number of people: the introductory student who would benefit from a stimulating addition to his standard reading assignment; the introductory lecturer who is willing to reconsider the nature of his course; the woman who doesn't understand why her husband is an experimental psychologist; and any psychologist who would enjoy a fresh look at some of the problems that matter.

the color-solid. In addition, it reviews briefly some of the more significant theories of emotion; reports certain experiments carried out on the basis of the proposed theory; discusses the relationship between emotions and personality, and emotions and motivation; suggests applications of the theory to psychopathology, including delinquency, and to psychotherapy. Also, it proposes research on various problems, among them further investigation of the suggested system of classification itself, which is admittedly tentative.

The book makes interesting reading. The writing flows smoothly and the reader is, unfortunately, held up by no such technical material as tables of statistics. The major question is of course, the extent to which the writer has fulfilled his aims, or how valid his propositions appear to be.

The definition of emotion is not a generalized statement of how conditions called "emotional" differ from other conditions which are non-emotional but is, rather, a listing of eight basic behavior patterns which, it is contended, may be found in some form at all levels of evolution and are gross behavioral interactions between organism and environment. The prototypes of all emotional behavior are described as: (1) "incorporation," or the "acceptance of stimuli from the outside world into the organism," (2) "rejection," or "getting rid of something harmful which has been incorporated," (3) "destruction," or an attempt to destroy a barrier to the satisfaction of needs, (4) "protection" or an "attempt to avoid being destroyed," (5) "reproduction," or "the prototypic response associated with sexual behavior," (6) "deprivation," or "the loss of a pleasurable object," (7) "orientation," or the behavior "which occurs when an organism contacts a new or strange object," and (8) "exploration," or "the more-or-less random activities organisms use to explore their environment." It is said that "It is generally evident with what emotions in man most of the prototypic patterns may be identified" (p. 71). For destruction the emotion is anger, for protection it is fear, for rejection it is disgust, for deprivation it is sorrow, for reproduction it is

## A Concept with Nine Lives

Robert Plutchik

*The Emotions: Facts, Theories, and a New Model.* New York: Random House, 1962. Pp. xviii + 204. \$1.95.

Reviewed by ELIZABETH DUFFY

The author, Robert Plutchik, did his undergraduate work in physics but sensibly switched to psychology and received his PhD from Columbia in 1952. He has taught at Hofstra College since 1951 but is currently on leave as Special Fellow, Section on Neuropsychology at NIMH where he is studying emotional and motivational behavior elicited by brain stimulation in monkeys. The reviewer, Elizabeth Duffy, is Professor of Psychology at the University of North Carolina at Greensboro (formerly, the Woman's College of U.N.C.). She received her doctorate at Johns Hopkins where her doctoral research involved an attempt to study individual differences in "emotionality" and she has maintained her interest in the emotions, although constantly questioning the scientific validity and usefulness of the concept "emotion," and has been engaged in developing an "alternative approach"—the consideration

of all behavior from the point of view of its direction, and intensity or degree of activation. The culmination of this study, and of theoretical papers involved in it, is the 1962 book *Activation and Behavior*, reviewed on page 337 of this issue.

THIS book, though a slight volume, undertakes a very ambitious task: (1) to develop a theory of emotion where, it points out, other theories are not completely satisfactory; (2) to discover the primary emotions by means of criteria applicable to all animal life; and (3) to derive mixed emotions from the primary emotions in a fashion similar to the derivation of the color spectrum from a few presumably primary colors, so that an "emotion-circle" can be formed which, by including a dimension of intensity, can be represented as an "emotion-solid" analogous to



joy, for incorporation it is acceptance, for orientation it is startle, and for exploration it is "something like curiosity or expectation." The addition of an intensity dimension to these prototypic emotions, and the occurrence of mixtures of these primaries are held to provide the "emotions" of everyday life.

THE REVIEWER is led to ask, then, whether all behavior is to be regarded as "emotional," and, if not, by what criterion or criteria these particular forms of behavior were selected for the designation. The author's reply would appear to be that "emotion [is] related to some kinds of basic adaptive, biological processes" (p. 56). But, one might ask, what behavior is not? Though it is stated that this criterion provides "a partial basis for distinguishing emotional behavior from other behavior" (p. 56), no supplementary criteria adequate to the task are suggested. In the opinion of the reviewer, "emotion" still remains a construct useful in everyday language, but of no scientific value. She would still contend that all behavior is specifically adaptive to the particular situation but may be usefully classified in terms of direction toward securing more or securing less of the stimulating condition, and of varying in a continuum in its intensity or degree of activation.

The author of the present volume has conducted a number of experiments based upon his concepts. These deal almost entirely with the subjective aspect of "emotion." Lists of words referring to emotions were rated to determine "which of a standard series of bipolar adjectives apply" to them; facial expressions were judged (and it was concluded, as Dunlap found some years ago, that "the greatest consistency of judgments were for mouth expressions"); facial expressions were synthesized by subjects from slides cut into parts representing forehead, eyes, and mouth, under instructions to make a face representing each of the eight primary emotions at a moderate intensity level, and the extent of agreement was observed; and a partial report was made of how children of different ages said they felt when they were angry, frightened,

sad, disgusted, or joyful. Most of these studies suffer from incomplete tabulation of results and lack of statistical treatment of data. Nevertheless, some interesting suggestions emerge.

The physiological psychologist will be disappointed to find that no physiological studies were made, though a few were briefly referred to in the historical account. Since it is claimed that the eight prototypic reactions are unlearned, patterned bodily reactions identifiable at all phylogenetic levels, and that in man there may or may not also be "introspectively reportable feelings associated with the different emotions," it would seem incumbent upon the author to produce experimental evidence of these patterns. Such evi-

dence as is cited from the findings of others does not bear directly on the present scheme of classification but serves rather, as the reviewer sees it, to suggest that implicit bodily reactions may be different when an organism tends to approach or seek more of a stimulus from those which occur when it tends to withdraw or seek less of the stimulus, and that, in either case, the intensity or degree of activation is an important aspect of the response. Without more solid empirical support, the theory proposed by the author of the present volume remains highly speculative. Even a psychologist of similar orientation might come out with a different list of primary emotions and of the results of their mixtures.

## A Whale Among Fishes

Elizabeth Duffy

*Activation and Behavior*. New York: Wiley, 1962. Pp. vii + 384. \$7.95.

Reviewed by FREDERICK A. COURTS

*The author, Elizabeth Duffy, received her PhD from John Hopkins and then taught briefly at Columbia University, at Sarah Lawrence and at NYU before going in 1937 to the Woman's College of the University of North Carolina (now The University of North Carolina at Greensboro) where she has been since. She is a Past President of Division I of APA, and of the Southern Society for Philosophy and Psychology, has authored many articles on muscle tension, emotion and energy mobilization. The reviewer, Frederick Courts, is a product of Stanford University who has taught at the University of Missouri and at Stanford but who has been at Reed College most of the time since 1945. He, like Elizabeth Duffy, has published widely on muscle tension and performance. In the summers of 1961 and 1962 he was Director of the NSF Summer Institute in Experimental Psychology for High School Teachers.*

SOME thirty years ago, Professor Max Meyer, unable to discover a scientifically useful definition of the concept of emotion, declared that this conceptual category, which he characterized as 'a whale among the fishes,' should be abolished from the vocabulary of psychology. Predicting that nothing but confusion would result from its continued use, he argued that the varieties of behavioral phenomena to which the term was traditionally applied could be handled more neatly by referring them to less ambiguous and more generally applicable dimensions of description. His dissatisfaction with this traditional category is shared by Professor Duffy, who since that time has repeatedly called attention to energy mobilization, or activation, as a concept capable of ordering many different kinds of behavioral data.

Duffy is not content merely to banish emotion from the society of fishes. Ob-



servations of behavior and analysis of current psychological concepts lead her to propose that there are only two basic dimensions of variation in behavior—direction, and intensity or activation. Although the direction of behavior is of utmost significance, the author chooses to focus her discussion upon the dimension of activation because it has been relatively neglected both in theory and in research. Operationally defined by measurements of organic processes accompanying the release of energy, activation is conceived as a continuous dimension of arousal or energy mobilization and release, extending from the low level found in deep sleep to the extremes of intense effort or excitement. Admitting that as yet unspecified subheadings are required under each of these major dimensions, the author argues that direction and activation “might advantageously be substituted for the traditional descriptive categories of psychology, many of which cannot be defined operationally.”

Most psychologists will be reluctant to accept such a parsimonious prescription because the price of parsimony and conceptual neatness may be far too high. It is likely that many will consider the suggestion that they view perceiving and thinking as incorporating “only the directional aspect of response” to be an over-simplification. Others will find it difficult to imagine how it might be possible to describe the phenomena of maturation and learning as only modifications in direction and activation. On the other hand, many psychologists will share the author’s hope, as does this reviewer, that the ubiquitous concepts of drive, motive, and emotion may eventually be replaced by more tractable dimensions of description. These may well turn out to be refined statements of direction and activation.

Today the concept of activation is not novel. It has figured prominently in the writings of many, including Cannon, G. L. Freeman, Lindsley, Hebb, Malmö, and others. Among these is Elizabeth Duffy, who formulated an early statement of an hypothetical inverted U-curve to describe the relationship of quality of performance to level of experimentally induced muscular tension. Subsequent experimental evidence dem-

onstrating the existence of such a relationship served to resolve the apparent contradictions produced by attempts to confirm the effect reported by Bills in his classical demonstration of the facilitative influence of squeezing a dynamometer while memorizing verbal material. In recent years a considerable amount of evidence has been adduced by a number of investigators to support the hypothesis that the U-curve may describe in general the relationship of quality of performance to level of activation.

THE VOLUME is divided into three main sections, dealing respectively with the general nature of activation and a general statement of the possible value of this concept as a device for cutting and slicing the complex phenomena of behavior into comprehensible conceptual categories describable according to quantitative dimensions, the behavioral consequences of intra-individual variations in the level of activation, and the correlation of individual differences in activation with differences in overt behavior.

Professor Duffy states that her purpose in writing the book “is twofold: (1) to show the place of the concept of activation in general psychology, and (2) to show the significance of this concept for the understanding of individual differences in behavior.” It appears to this reviewer that in the face of conflicting evidence and significant gaps in the available data she has made an excellent case for the importance of activation for understanding individual differences. Full realization of the first objective may not be possible without considerably more dependable data than are now at hand. Moreover, it will appear to some that this demonstration of the general theoretical significance of the concept of activation suffers from a tendency to oversimplify complex phenomena in an attempt to subsume all within the limits of the two dimensions of description.

The book contains a comprehensive review of the relevant literature. There are almost 900 items in the bibliography, most of which are cited several times in the text as they are viewed in relation to a variety of topics ranging from a

description of the neurohumoral basis of activation to an assessment of the role of activation in behavioral pathology. In addition, many hypothetical theoretical propositions regarding activation are advanced. Most are formulated in such general terms that they do not point clearly to experimental operations by means of which they might be tested, and when viewed in relation to one another they certainly do not constitute a miniature scientific system. Nonetheless, they clearly direct the reader’s attention toward a number of areas in which important research problems abound.

## Words on Numbers

Kenneth R. Hammond and James E. Householder

*Introduction to the Statistical Method: Foundations and Use in the Behavioral Sciences.* New York: Knopf, 1962. Pp. i + 415. \$7.00.

Reviewed by JULIAN C. STANLEY  
and GENE V. GLASS

The first author, Kenneth Hammond, a PhD from the University of California at Berkeley, is Professor of Psychology and Director of the Behavior Research Laboratory at the University of Colorado, Boulder. He is co-author, with F. Kern, of *Teaching Comprehensive Medical Care: A Psychological Study of a Change in Medical Education* (1959) (CP, Feb. 1962, 7, 60-61). The second author, James Householder, is an Assistant Professor of Mathematics at Humboldt State College, California. The first reviewer, Julian Stanley, here contributes a fourth time to CP. He received his doctor’s degree from Harvard University and has been at various places since then, including the University of Wisconsin, where he has been since 1953, and where he is now Professor of Educational Psychology and Director,



*Laboratory of Experimental Design (LED), Department of Educational Psychology. His chief work these days is producing unusually well prepared PhD's in statistics and measurement, with considerable help from NDEA Title IV fellowships in the design of experiments. Gene V Glass is a graduate student, and an unusual one. He took his AB at the University of Nebraska in 1962 and is now at Madison with Stanley and with one of the above mentioned fellowships, teaching and editing news and notes for the LED. He received an MS in June, 1963, and will probably receive his PhD in 1965.*

THIS PHILOSOPHICALLY oriented textbook is, like many other Knopf publications, attractive in format. The authors try unusually hard to make their selected subject matter learnable, even to the extent of interpolating 31 explanatory boxes as much as two pages long within the nine chapters. The authors of the quotations cited in these boxes range interestingly from Hume to Huff (*How to lie with statistics*). Quotations also introduce each chapter. The effect of all this is to give the book a literary and historical flavor, tempered by attention to logic and considerable elementary mathematical formalization. Students averse to quantitative thinking may find their "symbol shock" lessened somewhat by the extensive explanations and restrictions of content; less frightened graduate students may be irked by the redundancy and the elementary nature of some of the content, caused partly because each chapter proceeds rather verbally at first and eventually gets to the portion labeled "Mathematically . . ." A great deal of repetition, much of it probably facilitative, is caused by this progression at two levels.

With regard to the restriction of content, there appears no mention of Student's *t*, Snedecor's *F*, or small-sample theory in general. Sir Ronald Fisher is mentioned only for two quotations, "The improbable is inevitable" and a longer one concerning the mean and standard deviation. Spearman's name does not appear once in a

lengthy discussion and derivation of the rank-order correlation coefficient.

On the other hand, some of the material included is of dubious value. For example, with electric calculators and electronic computers available almost everywhere, it seems undesirable to bother with computing the mean and the standard deviation from arbitrary origins. Computational routines for desk calculators, using ungrouped measures, are preferable to historically interesting but anachronistic coding methods, we believe.

An average of 36 questions and problems follows each chapter. Answers to all of them appear in the back of the book. Quite a few of these problems are elementary and repeat the essential features of prior problems with only a few words or values of variates changed.

This handsome textbook will meet the needs of instructors and students who want an attractively detailed version of descriptive and large-sample statistics with practically no attention to modern experimental design and analysis. Probably it would serve best when taught by an able instructor who supplements the content considerably. We recommend it as a possible adjunct to the standard textbooks for psychology and education, such as those by Edwards, Ferguson, and McNemar.

Perhaps we are not being fair to the authors' intent, however. In a letter concerning a first draft of this review Hammond stated that "The book is an *introductory* text; it was written for undergraduates (mainly sophomores) who take the introductory course in statistics." This puzzles us, for nothing in the Foreword mentions sophomores or even undergraduates. Anyway, we do not see it as meeting fully enough the needs of students who will soon be taking courses such as experimental psychology wherein small-sample statistical procedures are required. Apart from this shortcoming, the book does provide an appealing general-education *Introduction to the Statistical Method* especially for behavioral-science undergraduates.



## Insufficiently Arduous?

George H. Weinberg and John A. Schumaker

*Statistics: An Intuitive Approach.* Belmont, Calif.: Wadsworth, 1962. Pp. vii + 338.

Reviewed by JOHN E. MILHOLLAND

George H. Weinberg, one of the authors here, took a PhD in clinical psychology at Teachers College, Columbia University and since has taught at Hunter College and has done research at the Handwriting Institute. John Schumaker, co-author, is a 1959 PhD in Mathematics Education from NYU, and is presently Professor of Mathematics and Chairman of the Mathematics Department at Rockford College in Illinois. The reviewer, John Milholland, did his doctoral work at the University of Michigan and, since 1950, has been on the staff there. He is now Professor of Psychology, and Chief, Evaluation and Examinations Division. During World War II he taught Naval Aviators how to navigate. For the last dozen years he has taught courses in statistics and measurement.

THIS BOOK is offered as the text for a year course, meeting two or three times a week, for undergraduate or graduate students in psychology, education, and the social sciences "who have little knowledge of mathematics." When they finish the book they won't have much knowledge of statistics, either. The book is hardly adequate for a one-semester course; a year's work based on it would really make a snap course. There is no treatment of analysis of variance; the *F* ratio is never mentioned; the *t*-test for the difference between the means of paired observations is "beyond the scope of this book" (p. 297); the correlation ratio is not mentioned, although nonlinear regression is described; the only measure of relation other than the Pearson *r* that is discussed is Spearman's rank-difference coefficient; nothing on multiple regression appears; and the



only applications cited for Chi-square are tests of association in contingency tables, wherein there is no mention of the continuity correction for the case of one degree of freedom.

On the positive side, there are good explanations of measures of central tendency and variability; of the reduction in errors of prediction when a regression equation is used; of linear regression generally; of the concept of a sampling distribution (although the term itself is not used); and of the idea of the power of a statistical test. For some of the other topics the explanations are not so clear, and might even mislead the naive student. The main value of this book would seem to lie in its use as a reference text which an instructor could recommend to students who were having difficulty with the particular topics that are treated well in it.

When a person writes an "intuitive" statistics book, in which only principles and results, without their mathematical derivations, are presented, he has a peculiar responsibility to be right in what he says. Although Weinberg and Schumaker have not made many errors, it would seem that it should be possible to write a book at this level without making any. There seems to be no point in listing errors in this review, but I should like to refer to one matter of principle that disturbs me. On page 46 appears the statement: "By converting the marks of each student into  $z$  scores, it is possible to obtain equal units of measurement despite the fact that the original units of measurement may have been different. The reason is that a given  $z$  score has the same meaning in any distribution." Certainly any such statement as this should have been preceded by a thorough and clear discussion of the restrictions placed upon the phrase, "has the same meaning," but it was not. It is of some help, however, that this passage was followed by a section entitled "No Inherent Relationship Between  $z$  Scores and Percentile Ranks."

This book is one of a number of attempts made in recent years to bypass the mathematical nature of statistical methods and somehow to make them palatable and easily understood

on a largely verbal level, and, like the rest, it doesn't quite come off.

John Gardner, in his essay on National Goals in Education, appearing in the Report of the President's Com-

mission on National Goals, said, "A college education should be a reasonably arduous experience." Perhaps learning statistics ought to be a little like that, too.

## Twains Fail to Meet in High School

Bernard Berelson (Ed.)

*The Social Studies and the Social Sciences.* New York: Harcourt, Brace and World, 1962. Pp. x + 303. \$2.40.

Reviewed by FRANCIS H. PALMER

*The editor, Bernard Berelson, is a sociologist who is currently on the staff of the Population Council Program. Not long ago he was Director of Columbia University's Bureau of Applied Social Science Research, and, before that, was Director of the Ford Foundation's Behavioral Science Division. He is the author of Graduate Education in the US (1960). Francis Palmer, the reviewer, is a psychologist who has taught at Allegheny College, has worked for HumMRO and at the University of California of Berkeley before coming to his present position as a staff associate on the Social Science Research Council where, he says, he is constantly in touch with various subspecies of social scientists as well as with psychologists. He is the SSRC representative for its Committee on Learning and Educational Processes and has developed a considerable interest in primary and secondary education and its relation to new developments in child research.*

THIS collection of papers is one of many recent efforts by social scientists and humanists to respond to the challenge of revised and apparently successful programs for teaching the hard sciences in the secondary schools. In this instance the American Council

of Learned Societies and the National Council for Social Studies want us to know that more history, geography, political science, economics, cultural anthropology, sociology, psychology, and area studies should be taught in the schools and taught in better ways. A representative of each of those disciplines tells why and how the high school student should be exposed to the content and method of his field. W. J. McKeachie writes about psychology and bears our banner high by identifying content that if learned could be useful to the school student and by stressing the importance of demonstrating the fact that human behavior can be studied scientifically. He does not discuss how psychology might contribute to citizenship nor how much should be taught in the schools. In this respect he is more modest than some of his co-authors who imply disaster for the members of a society whose schools do not teach enough of such substance.

The identification of content appropriate for the secondary schools will be useful to those concerned with the revision of curricula, but the book's unusual interest is in the development of an issue of some moment. Stated in its simplest form, the issue is whether the purpose of secondary schooling in the above subjects should be to communicate information related



to good citizenship or to develop an inquiring mind and introduce the student to the use of scientific method in studying man and his institutions. It is not often that a single volume presents such diverse opinions on a subject. For those who have assumed that the attitudes of academicians toward educational goals have been changing from support of the mass presentation of facts toward encouragement of inculcating habits of inquiry, there is a rough awakening.

To Bernard Berelson, who writes the introduction and whose credits for championing the behavioral sciences are many, the differences among authors' views with respect to training for citizenship are not really great: this is "a spurious issue that will go away if put in a different semantic perspective." He tries to find agreement among the authors, observing that each emphasizes the need for his discipline to proceed scientifically and supports the idea of communicating its methods to the student. He does not observe in his introduction that the authors differ immensely in their use of the terms "science" and "scientifically." Indeed, some of the authors appear to introduce the terms as if they were trying to take advantage of the momentum of a scientific band wagon. By applying the language of science to ancient methods of scholarship they appear to be willing to denigrate the honorable traditions of disciplines which long contributed the bulk of human knowledge.

A STRONG correlation between loose usage of scientific terms and appeals for disciplinary content as a vehicle for citizenship training can be discerned throughout the book. Representatives of the scientifically less rigorous disciplines tend to see their accumulated knowledge as necessary prerequisites for good citizenship; members of disciplines that are more cautious about information established mainly by the method of agreement suggest that the differences between experimentation and traditional scholarship and the intermediate shades of rigor between should be made clear to the secondary school student. For example, Ben Lewis writes that "the

content objective of economics in our schools is the economic understanding demanded by responsible citizenship," while insisting that economic studies must proceed scientifically. There is no mention that such a "science" of economics might vary as a function of the society concerned and its definition of good citizenship. Two chapters later and a world apart, Gresham Sykes states that "educating citizens for democracy" and such phrases are no more than an "apostolic laying on of words," and that with other professional sociologists he believes that "nothing but confusion can result from equating the objective study of society with a means of securing the good life." Lifted from context these illustrations may exaggerate the differences between the two writers. Lewis does assert that a knowledge of economics may not make one a better businessman, and Sykes admits that some sociological content may be useful for training citizens, but the fundamental difference between the two views is clear and it is not only semantic.

After reading this book one need not indulge in discussion of the merits of teaching citizenship to recognize that the attitudes and training found in some social sciences are so different from those found in others that not all should be taught together. Unfortunately, teachers trained predominantly in history have had a death grip on social studies courses, and it is there that scraps and bits of the social sciences are now taught. Furthermore, if each relevant national society persists in creating a task force for developing specialized curricula the situation is not likely to change. The usual high school cannot accommodate courses in psychology, anthropology, sociology, and all other social science disciplines. What is needed is a social science course for the high schools that stresses an inquiring approach to the study of man and his institutions and that illustrates relevant methods and techniques.

If one can read the present book without being tormented by the conflicting views described above, pleasant surprises are there. Joseph Strayer's chapter on history, Norton Long's

on political science, and Preston James' on geography make one regret the inadequacy of the treatment given these subjects during one's own high school experience and, more than that, eager to compensate through reading on a winter night. Come to think of it, I'll read Samuelson's *Economics* if Ben Lewis will read Cohen and Nagel's *Logic and Scientific Method*.

## Methodological Tour de Force

Bernard S. Phillips

*Aging in Central Illinois Community: Research Report 62-4.* Urbana, Ill.: University of Illinois, Small Homes Council-Building Research Council, 1962. Pp. viii + 93. \$2.50.

*Technical Materials for an Interview-Survey of the Aging: Research Report 62-3.* Urbana, Ill.: University of Illinois, Small Homes Council-Building Research Council, 1962. Pp. iii + 101. \$2.50.

Reviewed by ROBERT W. KLEEMEIER

The author, Bernard S. Phillips, received his PhD from Cornell University and is now Assistant Professor in the Department of Sociology and Anthropology at Boston University. The reviewer, Robert Kleemeier, started out as an experimental psychologist, moved to industry while teaching at Northwestern, taught for a while at the University of Florida, was USPHS fellow at the University of Bristol, and from 1949 to 1958 was Director of the Moosehaven Laboratory for the Study of the Aging. Then he went to Washington University where he is now Professor of Psychology. He edited the book *Aging and Leisure* (CP, Sept. 1962, 7, 316-317) and has co-authored



with W. E. Parker two volumes on industrial psychology.

It's a wayward thought, but sometimes I wonder if there might not be something to writing up the report of a survey before it's carried out rather than afterwards. Certainly it would not further the cause of scientific objectivity and accuracy, but it might make surveyors think seriously about how they were going to communicate their results to destined consumers.

Here is a case in point. In the spring of 1961, this multipurpose, probability sample survey was carried out in Decatur, Ill. with rigorous attention to methodological detail. A staff of experts was gathered, interview schedules prepared, sampling procedures (city directory and block supplement) carefully worked out, interviewers thoroughly trained and examined, and the data finally gathered, tabulated and analyzed. This was all done admirably well, though under pressing time demands (references are made to "the sudden decision to undertake the study" and to "rigid time limits"). And then the report was written.

The document reflects the attention lavished on methodological niceties, and certainly the data were gathered with evident skill and thoroughness. But the report itself is perfunctory. Actually it is the work of many hands. Dr. Phillips is the senior author, but William R. Best, Rudard A. Jones, Edward H. Storey and Bernard Lazerwitz contributed major sections.

The purpose of the study was twofold: 1) to provide "understanding" of the older people of Decatur, and 2) to furnish a model for other communities desiring to conduct surveys of their aged or other scattered, subpopulations. These are not entirely separate objectives, although the slim reports reviewed here indicate that the second was better served than the first. Thus the complete set of materials is made available including the schedule of questions asked, a syllabus of the interviewer training course, as well as information on the time and costs of the survey. This is worthwhile back-

ground for any group contemplating a similar venture.

THE substantive findings of the survey, however, add relatively little to our general knowledge about aging or the aged, although undoubtedly Decatur community planners find it useful for their purposes. In part this may be attributed to failure to integrate the survey results into the existing literature. To some extent the very brevity of the report accounts for this. In addition, there is a tendency to treat the age group above 60 as a homogeneous population; relatively few of the analyses present age regressions. This is a serious fault when one considers that

differences between 60-year-olds and 85-year-olds can be highly significant, and averages which combine the two may be profoundly misleading.

Nevertheless, some interesting findings emerge. The fact that health contributes so much to the self-evaluation of being old or elderly is a significant confirmation of somatopsychological theory of aging. Lazerwitz, in the Appendix, says: "Unfortunately, the publications describing the findings of . . . field studies almost always fail to describe, in detail, their sampling techniques, manpower requirements, and field costs." Indeed, this study scores well by this criterion, but the description of the findings themselves suffers in the attempt.

## Details, Dilemmas, Decisions and Defense

George W. Baker and Leonard S. Cottrell, Jr. (Eds.)

*Behavioral Science and Civil Defense.* Washington, D. C.: National Academy of Sciences, 1962. Pp. x + 169. \$2.00.

Reviewed by DONALD N. MICHAEL

*George W. Baker, one of the editors here, is a sociologist who is presently Associate Program Director for the Life and Social Sciences Facilities Program of the National Science Foundation. The other editor, Leonard Cottrell, is the well known sociologically trained social psychologist who is Secretary of the Russell Sage Foundation and is former President of the American Sociological Association. The reviewer, Donald Michael, is a social psychologist with a Harvard degree who is presently Director of the Peace Research Institute in Washington, D.C. He has served as staff social scientist of the Weapons Systems Evaluation Group of the Joint Chiefs of Staff and, with the National Research Council, has been consultant to the Committee on Disaster Studies and member of the Advisory Committee on Shelter Habita-*

*bility. He was the one who conducted the first and to date the only system analysis of management factors involved in operating for two weeks a thousand person fall-out shelter.*

THIS collection of papers is the result—nowadays the almost inevitable result—of a well-healed conference with a case to make. Ostensibly the purposes of this conference were: 1) to acquaint the newly formed Office of Civil and Defense Mobilization-National Research Council Advisory Committee on Behavioral Research with "the scope and nature of the problems confronting the OCDM on which help and advice were needed from the Committee and 2) to call the attention of the wider community of social scientists to the need and opportunities that the field



of civil defense presented for both basic and applied research in several behavioral science disciplines." Whatever the successes of the former, the latter purpose was only partially realized. A later volume, *Emergency Planning and Behavioral Research* (Disaster Research Group, National Academy of Sciences, National Research Council, Washington, 1962), is a far better statement of the problem area.

There is no indication here why the particular topics were chosen, why the particular authors were invited to write them and, if there was an underlying rationale, how it was that together the papers delineate the CD-behavioral science research domain. In short, this volume is a do-it-yourself hodgepodge of bits and pieces of the CD issue ranging from a description of the then "Status of Plans and Operations for Civil Defense" to Boulding's "A Pure Theory of Death: Dilemmas of Defense Policy in a World of Conditional Viability." Boulding's paper and that of Moore and Feldman, "Society as a Tension-Management System," could, except for a couple of peripheral references to recovery, have better been prepared for another conference altogether.

Only two of the papers really give a sense of the scope of the social science research problems regarding CD: Garrett's "Social Sciences Research Program Review and Prospect," summarizing his program in OCDM; and Loomis's "Toward Systemic Analysis of Disaster. Disruption, Stress and Recovery—Suggested Areas of Investigation."

THE EDITORS write of the conference that the "objective was full and free discussion of the problems of civil defense as seen by research scientists in the behavioral disciplines . . . whether or not these views were favorable to current governmental policies and programs." But what most strikes this reviewer are two crucial dilemmas about the relationship of behavioral science knowledge and research to the civil defense program which, while apparently evident to some of the authors, are not really explored by any of them.

In the first place, those papers which did attend to research problems demonstrated that for every aspect of CD there are fundamental questions requiring study if the potential utility of the program is to be evaluated objectively and if the implications for individuals and institutions are to be understood. Yet over and over both authors and their clients asserted that the program must push ahead, research or no. Why then all the hurrah about doing research? Images of locked empty barns easily come to mind.

The second dilemma variously implicit in a number of papers is nicely expressed by Loomis: "Perhaps the most important problems the differentiated or pluralistic democratic societies must face is how to obtain the advantages of maneuverability, strategy and striking force provided by power-centered and monolithic systems and at the same time retain the advantages of flexibility which our power diffuse pluralistic systems offer." Nunnley also expresses this dilemma and, indeed, is somewhat impaled on it in his "Public Apathy Toward Civil Defense: A Case of Anxiety." In his final paragraph he states, "Until we develop consistent, long range national goals, and strategies for implementing them, key people in the nation will remain divided and uncertain, programs of communications and action will be half-hearted, and the public will remain anxious and unprepared." Yet on the previous page he says, "It may be wise to devote special effort to inform and motivate civic and professional leaders, teachers, law enforcement agents, physicians and other key persons . . . It would be difficult indeed to obtain cooperation from the public at large if its key people are not well informed and convinced of the importance and practicality of the program." McDermott of the OCDM, in his opening remarks to the conference, perhaps unwittingly chose one horn of the dilemma when he suggested, "What the citizen will think is, 'If the President thinks it is important, if Congress thinks it important, if respected scientists urge this program, then it must be worthwhile.'" West, in "Some Psychiatric Aspects of Civil Defense," chose the other horn.

For he concludes that for citizens and leaders decisions about civil defense "should be based upon a rational assessment of all the facts, and they should be dynamic rather than static decisions."

Here lies the crux of the professional question for the behavioral scientist considering CD research. For, while there are several papers in the collection aimed at CD "market research," it is most significant that none of the papers dealt with the fundamental question: "What about civil defense, if through the use of honest, democratic methods, the public and its leaders are not convinced CD is in the interest of the 'long-range national goals'?" What happens then? Problems concerning the kinds of public response aroused when the CD program was pushed a few months after the conference was held were not among the research problems posed in these papers. And what happens if neither scientists nor laymen can be convinced of the utility of CD simply because the "facts," no matter how complete, are not the whole story, and values and emotions and life styles are the critical bases for people's decisions in this area? (Certainly these other bases played a large part in the government's decisions about CD, if for no other reason than all the facts that behavioral science could provide were not taken into consideration.)

The over-all feeling one gets in reading this collection is that for CD the issue is how to sell and study CD, especially how to make the "factual" case for CD: for the vast most part, the papers avoid the central, extra-scientific issues. It is this question begging which finally makes this book so unsatisfying and unsatisfactory.



*To make astute people believe one is what one is not is, in most cases, harder than actually to become what one wishes to appear.*

LICHTENBERG







## MORE ON CP AS A MAGAZINE

IN this space a month ago, *CP's* editor could be seen happily grasping and then manfully trying to appreciate E. G. Boring's distinction between a journal and a magazine. Moving toward what seemed a tranquilizing reduction in cognitive dissonance, the writer convinced himself that such a distinction carried great utility and went on to talk himself into at least a tentative conviction that *CP*, because it is more of a magazine than it is a journal, not only is but should be a *personal* publication. A personal publication, the screed went on to say, is one in which authors of reviews are encouraged to behave as individual persons, persons who, though subject to all the fallibilities that human flesh is heir to, can and should, within reasonable limits, write evaluatively and with easy idiosyncrasy about books. While the research journals and *Psychological Abstracts* must aspire to heights of impersonality, *CP* can properly hope for a plenitude of personalness and for that human juice that personalness can generate.

Personalness and idiosyncrasy, however, must be contained. A reviewer can be given a personal biography—hopefully one that helps the reader know the filter systems through which he is perceiving an absent book—but he cannot be given a clinical diagnosis. And while a personal reviewer may properly make idiosyncratic evaluations and is allowed, if he can, to write down the subjective twists and textures of his own full appreciation of a book, he must still consider some fundamental restraints. For example, he should not use an adjective in such a way that either he or the reader thinks it applies primarily to a book when in real-

ity it applies more directly to the reviewer or is more properly regarded as a characteristic of his own relation to the book. Many adjectives talk about the person who emits them, and both senders and receivers of judgmental messages need to know it.

Personalness helps make a magazine and *CP*, a magazine, can have personalness. But human fullness and juiciness must be obtained within the restraints imposed by considerations of fairness, by the search for accuracy, and sometimes by the demands of plain politeness.

And personalness, however it is defined and however elastic the restraints upon it, is not the only attribute with respect to which a magazine and a journal differ. Magazines must, among other things, be more interesting than are journals. Such a naked assertion must, of course, be at least fig-leaved by some qualifications. Interesting to whom? Interesting how? These may be answerable questions and well worth the answering.

INTERESTING to whom? Interesting to people who do not know as much as either the author or the reviewer do about the subject. *CP's* reviewers ought to write to—ought to interest—that mythical and useful being, *CP's* average reader. Even though psychology follows the trend of the times toward increased specialization—perhaps, too, toward eventual fission—there are still a number of psychologists who aspire to “keep up with” the entire field. *CP* has an unknown but fairly large number of cover-to-cover readers and would like it if every review it prints could be made interesting to these psychologists.

How to interest the average reader? Here we encounter at once the matter

of intelligibility. Interest and intelligibility are not identical attributes of writing but are related. Certainly it is very difficult to be interested for long in something totally incomprehensible. Without trying very hard a review can achieve incomprehensibility through strange and maladaptive construction of sentences and of paragraphs. More likely, however, or more likely to be printed, is incomprehensibility achieved through the use of a specialized and technical lexicon. One suspects sometimes that the technical language of a specialty is, in intent as well as in fact, a language of secrecy. And sometimes it appears (though never, never, in *CP* nor in manuscripts submitted to it) that a writer girds himself in his technical vocabulary and charges forth into a contest with the reader, a contest in which he feels ignominiously defeated if anybody really understands him. Unless a reader knows that something fascinating is going on behind the barrier an over-technical writer throws up, he will neither climb over a stack of dictionaries nor stoop to peer through keyholes to find out what is there. Where the journal writer can assume that his reader knows almost as much about the subject as the writer does, the magazine writer must deal with a reader who, though by no means either stupid or ignorant, is not a specialist in the particular field being considered. The writer must do some empathizing, perhaps some teaching. Or more inclusively, perhaps he needs to feel and act on a bit of straightforward and considerate friendliness toward his reader.

If the average reader is to be interested he must be presented with intelligible material. But intelligibility is by no means the only necessary ingredient of interest. (Actually, it may not even be as necessary as we are inclined to think, for surely young children do not find conventional intelligibility in the tales they so love to have read and reread to them.) While no one now can do much about Follettie's wish to bring rugged empiricism effectively to bear on determining the best way to get from one end of a sentence to another (*CP*, July 1963, 8, 286), it does seem proper to observe that *CP* and many of those who write for it try to go beyond the blind staggers of un-



trammelled intuition in going about the business of communication. *CP*, more so than a journal, is reader centered. How it or its writers go about taking the reader into account is not easy to say, but certainly the reader is not assumed to be either a disembodied intellect, focused with grim frigidity on pure substance, nor a scratchy purist, likely to explode into devastating hostility at the slightest departure from the stultifying stylistic stuffiness of the third-year graduate student.

Perhaps *CP* differs from journals in that it and its writers assume its readers to be people. It assumes them to be whole people, many faceted people, rather than individuals who behave now and forever in automatic accordance with the demands of a highly specialized and equally specific professional role.

And so we come again to personalness. And to the position that *CP* carries personal reviews that are written for readers who are persons. The psychologist in role, particularly if he wears his role like a straight-jacket, has no business enjoying a sentence or a paragraph except on the grounds of its explicit substance and the efficiency with which that substance is transferred from one nervous system to another. The psychologist as a person, by comparison, responds to more than naked substance. He can enjoy a bit the turn of a bright phrase or mull for a while over an enriching figure of speech. He may go so far, even, as to chuckle over a small joke, if the joke is both appropriate and non-distracting. And he sometimes responds with his scalp to the rhythms and patterns of the sentences the writer has built and honed and sent his way.

*CP* must have substance. *CP* must have clarity and fairness and accuracy and decency. But it can have also, and should have, some throb, some fire, some juice, some of whatever it is that characterizes the interactions of human beings who are gifted, educated, complex, fallible, explosive and inventive, who can perform freely within reasonable restraints, who can upon occasion enact with skillful and exceptionless rectitude a professional role but who are not permanently paralyzed by anybody's expectancies, even their own.

## Science or Propaganda?

Wesley Critz George

*The Biology of the Race Problem.* New York: National Putnam Letters Committee, 1962. Pp. 85. \$.50.

Reviewed by IRVING I. GOTTESMAN

*Wesley Critz George, the author, was born in North Carolina 75 years ago and has remained pretty much in that state ever since. His PhD degree in Zoology was obtained at the University of North Carolina and for a number of years he taught in the University's Medical School and served as the head of its Department of Anatomy. He is now Professor Emeritus of Histology and Embryology there. The reviewer, Irving I. Gottesman, is a Minnesota PhD who did his doctoral research with Robert Wirt and Sheldon Reed on a twin-study of the genetic aspects of personality. He has since completed a large scale twin-family study, under NIMH sponsorship, and now is planning to study the genetics of schizophrenics through the use of the twin-family method with Eliot Slater and James Shields at the Institute of Psychiatry at Maudsley Hospital in London. Currently a Lecturer in the Department of Social Relations at Harvard University, his interest and competence in the area of genetics is indicated by his memberships in the American Society of Human Genetics and the American Eugenic Society. On his return from London he will join the staff of the psychiatry department at the University of North Carolina Medical School.*

professor emeritus of histology and embryology, to prepare this report. The alliance resulted in a racist tract with relatively little scientific merit but a potentially large propaganda value. At the outset the author says that the object of his study is to bring to the attention of the Supreme Court "... a great deal of established fact and pertinent evidence ..." bearing on the decision which led to the illegality of segregated education. He implies that had this evidence been known, we might now not be pursuing programs which will "... result in mixing the genes of the Negro race with those of the White race and so convert the population of the United States into a mixed-blooded people."

Scattered among the book's nineteenth-century stereotypes about race are some facts from the present in such areas as human and animal genetics, psychology, neuroanatomy, and physical anthropology. The facts fall into three broad categories—relevant, irrelevant, and misleading. It is precisely the aperiodic insertion of relevant facts which leads to the potential persuasiveness of the more purely propagandistic assertions. If there is a cardinal fault in this treatise, it is that Professor George writes with a degree of certainty and conviction unwarranted in a controversial area. Ideas and hypotheses raised by credible scientists in the context of discovery are cited as incontrovertible facts in a context of justification. Important distinctions in the already mentioned areas of science are relegated to footnotes where, after cautioning the layman not to be misled, the author oversimpli-

SCIENCE and Government cooperated to produce this monograph purporting to summarize the biological evidence of Negro inferiority. The Governor (in 1962) of Alabama commissioned W. C. George, a biologist and



fies or is unclear. He writes about such variables as IQ and race as if from a cave with nary a *caveat*. The idea that IQ tests reflect accurately the genotypic intelligence of an individual is fostered as is the notion that that which is genetically influenced is fixed and unalterable.

*Genotype* and *phenotype* are conventionally defined in a footnote, but their relevance to the discussion of race differences is not appreciated. The best way to conceptualize the contribution of heredity to a trait is in terms of heredity determining a norm of reaction or of fixing a reaction range. Within this framework a genotype determines an indefinite but circumscribed assortment of phenotypes, each of which corresponds to one of the possible environments to which the genotype may be exposed. *If* and *when* the effort is expended to change radically the natural environment of a known genotype, e.g. maze bright and maze dull rat strains, there is a complete masking of the genotypic expectation and the two strains cannot be differentiated from their behavior (Cooper & Zubek, 1959). It is the plasticity of such a phenotypic character as human behavior in a heterogeneous environment and its invariance in a homogeneous environment which has led extremists to lose their perspective about the situations in which nature is more important than nurture and vice versa.

Confusion is bound to result from the way the author defines *quantitative* as contrasted with *qualitative* "... is often used in scientific writing to describe traits in a population, thereby defining the quantity of individuals having a certain quality." In other places he shows that he does not appreciate the fact that when the variance within groups exceeds the variance between groups, the difference between the means of the groups is not significant. A conceptual difficulty perhaps related to this last one is revealed in the discussion about the relation between brain size and intelligence, and again in the fervor with which Coon's (1962) interpretation of the fossil evidence as showing different species of man is embraced. Correlations between cranial capacity and IQ show that the former accounts for, at the most, 4% of the variation in the latter. Disregarding, if possible, the sampling problems and the fragmentary nature of fossil hominids, measurements of all but the earliest human fossils fall within the range of modern man.

Would that the flesh melt on the equalitarian straw man sketched by George! Babies are not born biologically equal; individual differences, in a uniform environment after matching for learning history, can be attributed largely to genetic variation; and, phenotypic differences in IQ test results do remain after groups of White and Negro children have been matched on a few of the relevant variables, e.g. paternal occupation. Propagandists omit contradictory evidence. Scientists do not. A wealth of data from the study of stimulus deprivation in young mammals leads to the conclusion that there are effects which can be subsumed under the rubric of deficit in later adaptive behavior (e.g., Casler, 1961). Any child rearing patterns leading to decreases in perceptual input or to

disruption of adult-infant affectional bonds can be expected to result in intellectual deficits. Fathers are absent in about 25% of Negro households. It has only been since 1950 that the school attendance of Negroes has approached that of Whites at the elementary level.

The monograph concludes with the hallmarks of effective propaganda, *ad hominem* and *innuendo*. Franz Boas "born of Jewish parents . . . pacifist . . . had various communist-front affiliations . . . and his students are pinpointed as the source of current dogmas asserting the absence of important, innate racial differences. The tract demonstrates that scientists can no longer remain in ivory towers but must retain an ethical responsibility to alert the public when their findings or ideas are misinterpreted or misused.

## Freud's Wrongs Righted?

Leon Salzman

*Development in Psychoanalysis*. New York: Grune & Stratton, 1962. Pp. v + 302. \$7.75.

Reviewed by LEON M. SIROTA

Dr. Leon Salzman, the author, is a practicing psychiatrist who also professes clinical psychiatry at the Georgetown University School of Medicine. With Jules Masserman he edits *Modern Concepts of Psychoanalysis*. The reviewer, Leon Sirota, received his PhD from the University of Michigan and taught at Bennington College before moving to Monteith College, Wayne State University, where he is an Assistant Professor in the Division of the Science of Society. This is a social science department that crosses traditional disciplinary lines. He reports that he is at present fully a teacher, a label he regards as neither modest nor grandiose, and an endeavor that by no means implies the cessation of thought.

WRITING from a neo-Freudian position, Dr. Salzman gives as another example of the genus of book that is a dialogue between a Freud who is brilliant but wrongheaded—and also imaginary—and a host of his brilliant, but righthheaded professional nieces and nephews. Thus the developments of the title are actually firm but tolerant corrections. The imaginary quality of the Freud here may be seen in his failure to postulate a connection between anxiety and repression; fortunately, Karen Horney seems to have corrected the lapse. This Freud also does not conceive of symptoms as being purposive and adaptive. We have Adler to thank for that idea. Our present Freud claims to be interested only in a theory of path-



ology, but this narrow refusal to generalize to all human behavior is made up for by Sullivan.

One senses the presence of unseen capital letters in this book. Some may take it as evidence of the march of science that these disembodied typographical spirits now haunt such words as culture and individual rather than instinct and libido. But one wonders whether or not the culture that Dr. Salzman speaks of is not made dim and analytically weak by the haunting. It certainly no longer clearly resembles the culture of the anthropologists. When one tries to pry it away from such related concepts as society and learning, it appears to be too flabby to stand up. In the same way, the term individual ceases to represent a phenomenal unit and becomes a positive but very indefinite value.

THE conceptual vagueness exemplified by this particular use of the terms culture and individual is consistent with the author's essays into scientific epistemology. He seems to have little appreciation of the dialectical relationship between theory and data and apparently considers them products of two essentially separable processes. He thus tells us to "avoid prejudicing the data by preconceptions," as if data-gathering were possible as a reflex behavior not involving conceptualization. Lest it be thought that Dr. Salzman is merely cautioning the reader against too strong hypotheses or attitudes which will not permit the rejection of hypotheses, I refer the reader to his epistemological appreciation of Sullivan. We are told that Sullivan's postulates "are always capable of being tested directly through observation"; that empathy is a directly observable process and therefore not a metaphysical, i.e., 'bad,' concept; and that tender feelings result from anxiety as a "creative force" rather than the more usual "inhibiting force"—an inconsistency that the author himself finds difficult to rationalize, but nevertheless "a fine example of Sullivan's attempt to frame human behavior in operational terms."

Dr. Salzman's idea of operationism is further exemplified in the section of the book (fully a third) in which he

deals with some of the classical topics of psychoanalytic theory such as female psychology and love, hostility, and depression. This part of the book represents the author's attempt to synthesize his own clinical experience, his own systematizing theoretical efforts, and the relevant theoretical statements of his mentors. His discussions of symptomatic difficulties of sexual compatibility serve as an example of utilizing a superficially detailed "operationism" to define a theoretical and human problem right out of existence by means of the equation operational equals arbitrary. These examples also display the notion of culture as an evil external interfering with the 'naturalness' of the individual.

ALTHOUGH this book is an example of the promotional use of popular terms from social science and the philosophy of science, it does display some genuine if unsystematic appreciation for the rule of parsimony in theorizing. It is also a source of synoptic information about the theories of minor neo-Freudians such as Silverberg and Robbins. On the other hand, one wonders if a book purporting to survey broadly the recent developments in psychoanalytic theory can justify total omission of the ideas of Erikson and the inclusion of only a few sentences about Hartmann, Kris, and Loewenstein on the basis that their work is "too technical." One also wonders about the advisability of employing a rhetoric which always presents criticisms of theorists unsympathetic to the author as proceeding from abstract reason, while criticism of the neo-Freudians is usually presented as coming from the opposition.

This is the sort of book that is often a candidate for the position of textbook in academic courses surveying psychoanalytic theory. In view of the criticisms already detailed and the very sketchy sections on Adler and especially Jung, the reviewer could not recommend such a use of the book.



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# Anthropologist's Self Psychology

Ernest Becker

*The Birth and Death of Meaning.* New York: Free Press of Glencoe, 1962.  
Pp. ix + 210. \$5.00.

Reviewed by FRED MCKINNEY

Ernest Becker, the author, has a degree in anthropology from Syracuse University. From 1950-55 he was with the U.S. Foreign Service in Western Europe but returned to academia and is now engaged in teaching and research in the Department of Psychiatry at the Upstate Medical Center in Syracuse. He is the author of *Zen: A Rational Critique* (CP, June 1962, 7, 228). The reviewer, Fred McKinney, has often performed for CP. He has been in and around the University of Missouri since 1931, with time off for various unusual appointments such as that of visiting TV lecturer and consultant at Stephens College, and Fulbright Lecturer at the University of Ankara. In recent years, in addition to writing reviews for CP, he has taped 26 educational TV kinescopes which are distributed by Educational TV and Radio Center and Stephens College, respectively, and, more recently, produced 31 forty-five minute video tapes in general psychology. Through it all he keeps alive his interest in personality theory.

IT is a difficult thing to write a book that the newcomer to the field will find readable and that will at the same time offer an abstract interdisciplinary scheme of concepts for the mature student of man. Each of these purposes Becker accomplishes at different points in his book, but I judge that he loses a large part of his lay audience at frequent intervals, since he assumes considerable sophistication in social theory.

Dr. Becker is intrigued with the functionalism of Dewey, as his title sug-

gests. (James and Mead and Cooley are also quoted.) At times it seems that the main thread of his system is neofunctionalism, but the lure of psychoanalysis, even though there is heavy emphasis on writers such as Adler, Sullivan, and Fromm, seems to lead to delaying fixations in the development of the author's thesis. Becker is conscious of his emphasis on Freudian critique and explains in his preface that it is necessitated by his psychiatric audience. He hints, too, that he believes many social scientists have not taken Freudian instinct theory seriously enough. The author's reference to current psychological literature is uneven. His writing is, in places, similar to those who appear influential in the existential movement in psychology, but he fails to call upon those who could assist him in the development of his point of view and, possibly, his system—Allport, May, Goldstein, and Rogers. Jung's name is conspicuous by its absence from the index, and the term *existentialism* is not included. Despite concern for the individual and his experience, and despite a phenomenal frame of reference, the book is mainly nomothetic. My own ethnocentrism prompts me to believe that had his primary orientation been a responsibly eclectic and contemporary psychology rather than a Freudian and social theory, he would have developed his system with more telling direction and clarity.

Despite the fact that Dr. Becker's intended audience includes students at some level, this volume is less a textbook than a series of related essays. He does not define precisely the basic concepts such as meaning, anxiety, and

ego at their first use. There are, however, many cogent and attractively written observations of man in his social milieu throughout the book; in some places his prose resembles poetry.

THE AUTHOR is most engaging when he is dealing with anthropological phenomena. His chapters "The Primate Scene" (which discusses vertebrate group living) and "From Animal to Human Reactivity" (which deals with the development of the sense of "I") are quite vivid and coherent. His discussion of mental disorders in relationship to culture is not systematic, but he makes a number of references to schizophrenia and states of depression as seen from a cultural perspective.

Dr. Becker admonishes social scientists to go beyond description to follow the methodological path of physical science, to formulate deductions that can be tested by some kind of controlled manipulation of data. He frankly tells his psychiatric listeners that psychiatry is a pseudoscientific discipline which directly manipulates individuals in the interest of the social *status quo* and of the personal aggrandizement of the psychiatrist.

The system of concepts that undergird the book begins with the central idea of anxiety as a phenomenon found in higher organisms. Anxiety is elaborated in man as a learned fear of object loss or a loss of the significant other person. Out of this fear grows a self system which is a means of buffeting anxiety and of keeping symbolic activity moving forward in an integrative manner. In the self system we find the locus of linguistic causality which animates the individual's conduct and moves others to action. In the psychological personal world of the individual there are relationships and values. Man's self-esteem is an integral quality of the self system together with a sense of identity which is the experience that he is agent of his powers. The culture in which the individual lives prescribes certain role behaviors for meaningful action. It is in these roles that he exercises his power and comes to know himself as a responsi-



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ble contributor or sharer of socially created meaning. Society provides for extrafamilial action and reality-oriented institutional patterns.

Becker's work reflects a wide scholarly background and is a contribution to self theory from the standpoint of an anthropologist who aspires to cut across the jurisdictional boundaries of the academic disciplines. The thirty-

six pages of annotated bibliography and notes at the end of the book are on the whole clearer and more involving than parts of the text. Had this been his predominant style rather than that shown in most of the text, which is presumably influenced by lecture experience, Dr. Becker would, in my opinion, have communicated better with his audience.

According to Wolstein's view, the existential analyst in the development of his position finds it necessary to dissociate himself from every significant philosophy, past and present. He makes the unalterable fact of death into the primary principle of life, and for him all experience becomes transformed into a barren absurdity. The totality of existence is shot through and through with absolute anxiety.

The individual's dreadful anxiety—and irrational despair—stands apart from and seems not to be rooted in the social and cultural condition of man. Thus Wolstein looks upon existential analysis as a highly subjective psychology that is obsessed with a lonely and fragmented "I-am" as its object of study and concern.

## Enchantment with the Absurd?

Benjamin Wolstein

*Irrational Despair: An Examination of Existential Analysis.* New York: Free Press of Glencoe, 1962. Pp. vii + 200. \$6.00.

Reviewed by LAWRENCE E. ABT

The author, Benjamin Wolstein, received his PhD from Columbia and, after an internship at Kings County Psychiatric Hospital, has been, among other things, a therapist at the Post-Graduate Center for Psychotherapy and on the staff of the William Alanson White Institute of Psychiatry. He is the author of *Transference* (1954) and *Countertransference* (1959). The reviewer, Lawrence Abt, is a product of NYU who trained in psychoanalysis and psychotherapy at the William Alanson White Institute. He is in private practice in New York City and in Larchmont, and is a consultant to the VA central office. He currently is Visiting Lecturer at the New School, where he teaches TAT. He is editor, with Leo Bellak, of *Projective Psychology* (1950) and with Daniel Brower, of Vols. I-III, with Bernard Riess, of Vols. IV and V of *Progress in Clinical Psychology*. His colleagues regard him as toughminded when it comes to methodological matters in psychoanalysis and in projective techniques.

BENJAMIN WOLSTEIN'S interesting and provocative examination of existential analysis represents the efforts of one psychoanalytically trained and oriented psychologist to come to grips

with significant philosophical and clinical issues presented by the existentialist-inspired psychotherapeutic points of view. The point of departure for Wolstein's appraisal is his methodological concern for the issues and problems of transference and countertransference processes, especially as these manifest themselves in on-going therapeutic transactions. His carefully reasoned and well presented work will provide little comfort to either the analysts or the existentialists.

Judged by their current writings, existentialists may be distributed along a continuum extending from those who are impressed by the significance and power of empirical data—and thus find a need to retain and reinterpret transference, unconscious processes, and other basic psychoanalytic notions—to those who have openly ruled out the basic structure of psychoanalysis altogether because they do not wish to deal with objective—hence alienated and contaminated—methods and results of scientific investigation. Wolstein finds the best way to characterize all of these wide varieties of existential analysis is in terms of absolute immediatism. The vital driving force behind the growth of existentialisms is a quest for absolute certainty.

MANY of the ideas traced in *Irrational Despair* are familiar, and although Wolstein is deeply critical of many of them, he seeks to maintain a perspective that is grounded both in the history of philosophy and the requirements of logic and scientific methodology. His presentation differs sharply from that offered by Rollo May and his colleagues in their *Existential Psychology* (CP, June 1962, 7, 227). A particular merit of Wolstein's book is that it provides a helpful and systematic, if quite critical, way of examining all sorts of ideas that have been put forward in the name of both existentialism and existential analysis. When these ideas are put in systematic context and logically scrutinized for consistency and pertinence to the transactions of psychotherapy, many of the familiar ideas acquire a status of doubtful validity and invite questions of their acceptability in the contemporary psychological scene. In his actual therapeutic role, the existential analyst appears as a spectator rather than as a participant-observer in the whole series of transactions that constitute psychotherapy, and Wolstein raises questions about the existential analyst's impact upon his patient and the nature of the therapeutic outcomes. Readers will recognize this view as sharply at variance with that expressed by Henri Ellenberger and others of the



existential persuasion. The psychoanalytically oriented therapist is committed to a thorough-going psychic determinism, and the existential analyst is concerned with the ahistorical present. The psychoanalyst is interested in working with psychological insight toward the reconstruction of the patient's personality and fulfillment of potentialities, and the existential analyst's interest is fulfilled with the attainment of catharsis. Within the framework of these ideas, Wolstein insists that there is little prospect that the existential approach will significantly further the theory or practice of psychoanalysis.

The author's greatest indictment of existential analysis is that it is essentially a psychology without meaning and one that has abrogated well-established principles of inquiry and thus has produced anarchy in the therapeutic field. This is a harsh judgment, and many will not agree with it; but it is presented persuasively by Wolstein.

What really bothers Wolstein most about existential analysis is its essential irrationality—and its essentially absolute philosophical position which downgrades experience and substitutes existence. He sees absurdity raised to the level of a theory of value—and a theory of life. There is, in short, an enchantment with the absurd.

Whatever one may think of his conclusions, and the reviewer has reservations on many issues, Wolstein reveals himself as a clear and systematic thinker who has disturbing ideas about current directions of therapeutic movement. He writes well, his book is carefully organized, and its complex ideas clearly presented. It is well worth examining critically in relation to many other points of view about psychoanalysis and existential analysis. It is certain to provoke controversy, and this may prove to be its chief merit.



*Without consciousness there would, practically speaking, be no world, for the world exists as such only in so far as it is consciously reflected and consciously expressed by a psyche. Consciousness is a precondition of being.*

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# Dreamy Usefulness

Richard M. Jones

*Ego Synthesis in Dreams.* Cambridge, Mass.: Schenkman Publishing Co., 1962. Pp. xi + 100. \$2.45 (paper) \$4.50 (cloth).

Reviewed by ROBERT A. CLARK

The author, Richard M. Jones, is a Harvard PhD in psychology who received psychoanalytic training with Andras Angyal. Since 1957 he has been a teacher and practitioner of psychology at Brandeis University. The reviewer, Robert A. Clark, received his BA in psychology from Harvard but then went on to Harvard Medical School for his MD. After that he became Director of the Out-patient Department of the Western Psychiatric Institute in Pittsburgh and Associate Professor at the University of Pittsburgh. Presently he is Medical Director of the Northeast Mental Health Clinic in Philadelphia. Besides his clinical and administrative work, he has published on dreams.

DR. JONES is interested in knowing how the study of dreams as reported by the dreamer can help psychology understand adaptive growth of the personality. He wants to find "a method as sensitive to the normality of dreams as is the free association-latent content method to pathology." It is unfortunate that he pursues these laudable aims in a most opaquely abstruse academic jargon, and that the book costs more than most would pay for a 100 page monograph. Deplorably also, the book has no index.

The foundation upon which Jones builds his thesis is Freud's "Interpretation of Dreams." He has the courage to go beyond Freud. He uses, in a modified form, Erik H. Erikson's schema for personality development. He cites approvingly that arch-heretic of orthodox Freudianism, C. G. Jung, as well

as the accepted authorities, Herbert Silberer and Thomas French. He accepts the biological function of the dream, namely, the preservation of sleep, and adds a psychological function. This function, he states, is said by Jung to be compensation for conscious myopia by unconscious vision, and by Silberer to be restoring the balance between waking and dream life through symbolization. Both French and Calvin Hall attribute to dreams the function of attempting to solve current conflicts. The author ignores Alphonse Maeder's pioneer contributions, best set forth in his *The Dream Problem*. (Nervous and Mental Disease Monograph Series, No. 22, New York, 1916). Here are quotations showing Maeder's recognition of the psychological function of dreams: "the dream seeks for a satisfying formula for the unconscious condition and strives for its expression . . . Dreams also contain a progressive side" foreshadowing future developments in the personality, much as do play and fantasy. W. H. R. Rivers, in *Conflict and Dream* (Harcourt Brace, New York, 1923), carries Maeder's thought a step further: "Problems may be solved in sleep which either have not previously been the object of serious attempts at solution or may have been beyond the powers of the sleeper when awake." More recently Werner Wolff has put forward (*The Dream, Mirror of Conscience*, Grune and Stratton, New York, 1952) conceptions similar to Jones': "The dream is an attempt at interweaving the main problem with the dreamer's other life issues and making a decision from this pattern relationship. The dream is an

attempt at a value system; the dreamer reevaluates his life situation. Both concepts . . . demand what we call a synthesizing interpretation of the dream."

THE AUTHOR presents this working hypothesis (in his typical style): the dream engages in "reconstructive activity, preconsciously re-differentiating and re-integrating pre-adaptive epigenetic successes and failures in the context of and under the problematic pressure of phase specific re-adaptive crises." In other words, a person going through a critical stage in his development has dreams which draw upon previous successes and failures in trying to find a solution leading toward further growth of his personality. His definition of adaptation improves upon the meaning found in the psychology texts of thirty years ago. He recognizes that the ego must adapt to two realities: the outward environment, and the inward processes of maturation—the *Anlage* of constitutionally determined phases of development. He dismisses Jung's observations (on the theme of primordial images symbolizing these phases) as philosophizing on the archetypal currents of "personal myth," and so misses the opportunity to add another dimension of depth to his analysis. He does add to the formulations of French by distinguishing between the "defense ego," which aims to preserve sleep from the assaults of repressed wishes, and the "synthesis ego," which supports adaptation in both directions.

He admits that his is a "loose" theory, that he has not proved his hypothesis, and that he has demonstrated only that his method of dream analysis is "feasible." His demonstration consists in illustrations from the dreams of an anonymous college student, compared with Calvin Hall's discussion of the dreams of another college student. Those ubiquitous college students! So close at hand, so willing to volunteer, so cooperative with their mark-giving and degree-conferring teachers! Like the psychoanalysts' patients, they are a select class, quite unrepresentative of the whole population, even of American



So many intricate psychological theories rest on just such a slender foundation, yet they are put forward as secure generalizations regarding the whole of humanity.

## To Q or Not to Q

WHAT then is the value of a study such as this? It is an advance over psychoanalytic orthodoxy, encouraging, may we hope, further criticism and expansion of Freud's original theories, and further creative thought on the meaning and function of dreaming. Certainly Jones is right in urging the manifest dream as the proper subject for scientific study, rather than that artificial construct, the latent content. Nevertheless, it is hoped that this preliminary study will be followed, on the part of this author or of other courageous and persistent students of the dream, by wider ranging efforts based upon larger and more varied series of personalities. An example, in the Jungian mode, of intensive study of one therapeutic case, is Gerhard Adler's *The Living Symbol* (Pantheon Books, New York, 1961). A stimulating work, in interpersonal terms, based upon an extensive series of dreams of psychoanalytic patients, is Walter Bonime's *The Clinical Use of Dreams* (Basic Books, New York, 1962). Yet to be written is a study of the occurrence of consecutive dreams, in times of crises in the growth of normal subjects, who successfully surmount crises and go on to the living of creative lives without the neurotic trends that lead to the psychoanalyst's office, or to a major in psychology. Since a knowledge of the service of dreams in adaptation is of no value in the waging of war, the money for such a monumental project will presumably not be available until after general disarmament.



Yet we think that psychic mistakes and their consequences can be got rid of with mere words, for "psychic" means less than air to most people. All the same, nobody can deny that without the psyche there would be no world at all, and still less, a human world.

—JUNG



Jack Block

*The Q-sort Method in Personality Assessment and Psychiatric Research: A Monograph.* Springfield, Ill.: Charles C Thomas, 1961. Pp. vii + 161. \$6.75.

Reviewed by JOHN E. EXNER, JR.

*The author, Jack Block, received his PhD at Stanford University, but has been across the bay at the University of California at Berkeley, ever since that time, most recently as Associate Professor in the Department of Psychology. He has published widely in the area of personality research and is currently concerning himself with ego psychology. The reviewer, John Exner, a product of Cornell University, is currently with the Department of Psychology at Bowling Green State University in Ohio, as coordinator of training in clinical psychology and as an associate professor. He was formerly Director of the Bureau of Testing and Research and also an associate professor at DePaul University. His publications fall principally in the area of diagnostic techniques, particularly projective methods.*

THE MATERIALS presented in this monograph provide a substantial argument supporting the usefulness in research of the general scaling procedure known as the Q-sort technique. The author, well grounded in his knowledge and experience with the technique, is admittedly interested in presenting this argument so as to discuss in turn the relative merits of a specific Q-sort, the California Q-set (CQ). Thus, the material becomes narrowed considerably in its total scope but nevertheless continues throughout to be thought provoking. Several of the frequently voiced criticisms of the Q-sort technique are dealt with in adequate depth and sufficient clarity to leave even the most negativistic reader somewhat impressed.

For instance, considerable space is given to clarify the necessity of observer-evaluations and to offer evidence that such evaluations can be reliable when the training of the observer meets a minimal criterion. Likewise, evidence and argument are put forth to demonstrate that specific Q-sets can be constructed in such a manner to overcome, or at least to compromise with, the contention that the results of the Q-sort technique are typically a function of the particular Q-set employed. One problem area which is possibly not dealt with in the thoroughness which it deserves is that of typology. Naturally this is not a problem common only to the Q-sort technique but to practically all instruments oriented toward a normative basis and, although it is a problem that has been and can be handled satisfactorily, we are asked to accept here the propositions that psychologists do think in typological terms, a request that seems later to lend itself to considerable overgeneralization. It is suggested that the Q technique in itself can be a valuable tool from which data can be derived to ascertain whether a genuine typology does exist for a given group of individuals, i.e. successful physicians, creative scientists, schizophrenics, etc. . . . Unfortunately we are left with the promise that studies currently underway may shed much light on these questions and others of a similar nature. In fact, there are points in the text where the reader almost has the impression that if publication of the monograph had been delayed another year or two and subsequent data had



been included, the entire impact of the material would have been significantly greater.

THE OVER-ALL description of the development and uses of the CQ set is quite good. The enormity of work devoted to its construction is very impressive and the care given to the rationale of the set is admirable indeed and could well serve as a model for the construction of other such sets. Even the decision to construct the CQ set as a forced-choice type, which ordinarily could open the door to much criticism, is based on solid reasoning to which most researchers would be agreeable. It seems realistic to suggest that if many of the earlier articles pertaining to the Q-sort technique contained the theoretical and practical thoroughness as is demonstrated in the construction of the CQ set, the entire status of the technique as a research tool could be much greater than is currently apparent. The arguments favoring the usefulness of the technique in general, and the CQ set in particular, in individual personality assessment are considerably less attractive and realistic than are the statements favoring its use as a research tool. For instance, it is suggested that significant difficulties exist in the area of communication between several persons of different professional disciplines with regard to reporting the observations of each concerning a single individual. Diagnostic staff meetings which include reports of psychiatrists, clinical psychologists, and social workers are cited as a case in point. While there is no doubt that some communication problems exist in practically every diagnostic staff meeting, no evidence is offered here either to support the idea that these are really serious differences or to negate the contention that such differences may really be an asset to a better, more global understanding of a patient.

Possibly the most unfortunate aspect of the monograph is that it is too short. After struggling through arguments, and bits of data, all of which lend themselves quite well to the notion that the Q-sort technique is really worthwhile, the reader is left with lit-

tle information concerning some of the most practical applications of the technique. For example, any teacher of clinical students could not help but be impressed by suggestions of how the technique can be helpful in evaluation of the degree to which personality appraisals agree. But alas, little elaboration is provided here. Also mentioned far too briefly is the applicability of the technique to the study of highly specific personality characteristics such as need achievement, reality testing, etc. A more thorough approach concerning the various applications of the technique would not only make the book more palatable, but could have easily made it useful as a good seminar text. In its present form it provides useful information to the reader but much of this information only serves to raise questions rather than answer them. Its full clarity is probably derived only if the reader has some previous familiarity with Q-sort. The author has done a very good job in light of his apparent objectives. But I wish the book were longer.

## A Hazily Human View?

Thomas E. Jordan

*The Mentally Retarded.* Columbus, Ohio: Charles E. Merrill, 1961. Pp. ii + 355.

Reviewed by GORDON N. CANTOR

Thomas E. Jordan, the author, received his PhD degree from Indiana University and after some intervening teaching experiences is now Professor of Education and Director, Center for Teacher Education at Tulane University. He is author of *The Exceptional Child* (1962). The reviewer, Gordon Cantor, did his doctoral work at the State University of Iowa in the area of experimental child psychology. Subsequently he taught and researched for six years at George Peabody College before returning in 1960 to his

old stamping grounds to become a member of the staff of the Iowa Child Welfare Research Station (now the Institute of Child Behavior and Development). While he now works in the area of frustration and curiosity behaviors in normal children, he has written rather extensively in the area of mental deficiency and retardation and has reviewed before for CP (CP, July 1957, 2, 180-182) and (CP, Aug. 1960, 5, 247-248).

THE 'field' of mental retardation, if such there be, draws its data from numerous sources (most notably, biochemistry, physiology, biology, psychology, and education). An author who chooses in the 1960's to write a general mental retardation text purporting to do justice to the accumulated information in all these areas tackles a formidable if not impossible task.

Jordan's book constitutes such an attempt and, in addition, is said to offer "a new conceptual key to mental retardation." Comprehensiveness is claimed in the form of a bibliography which (p. vi) "In general . . . is complete *ad hoc* 1960. It is representative of a pool of several thousand studies, and it may be used as a guide to the literature of the last several years." The "new conceptual key" is considered to stem from an extended discussion of the family, placed early in the book (Ch. 2). According to the author (p. 67), ". . . we must see it (mental retardation) as a people-centered problem from the beginning. Only by seeing how lives are affected can one move to the more segmented, truncated, aspects of the disorder with any sense of urgency . . . it is vital that we see the humanitarian core of the problem first."

Regarding the psychological materials included in Jordan's volume, the coverage is not representative, let alone complete. Examples: (1) sampling from the extensive literature on learning is completed in less than three pages (of the seven studies briefly discussed, one is inaccurately de-





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scribed; (2) in the area of personality, no mention is made of Kounin's classic research on "rigidity" in the retardate or Zigler's important follow-up work (e.g., Zigler *et al.*, *J. Pers.*, 1958).

Though lacking the necessary competence for judging the adequacy of coverage in the non-psychological areas, this reviewer cannot refrain from noting that the momentous chromosome discovery regarding mongolism is given just two brief, uninformative nods; nowhere is the reader apprised of the nature or significance of this development.

THE attempt to provide a "new conceptual key" is not, in this reviewer's estimation, a successful one. Placement of the discussion of the family early in the book does not make it a superior discussion, nor does its location appreciably enhance the quality of the remaining chapters. Sarason's text does a better if more scattered job on this topic, though Jordan utilizes a wider range of sources of data (e.g., sociological studies) and, of course, includes more recent references.

Because of a frequent use of subheadings, Jordan's book presents an appearance of being highly organized. This is not the case, however. References to given topics occur in scattered places throughout the volume, often in repetitive fashion. The content of a subsection may have little or nothing to do with its heading (e.g., an extended discussion of motor development in the cerebral palsied follows the heading "physical growth"). There are inconsistencies [phenylketonuria is "rare" (p. 90), "quite common" (p. 197), and "uncommon" (p. 233)].

It is difficult to judge the audience for whom the book is intended. At times, the writing talks down to the freshman student—e.g., (p. 249), "The technique of instruction is generally that one teacher instructs several children or more who are variously identified as groups and the aggregates of groups. These we call classes." On other occasions, material is included which is apt to be meaningful only to the trained specialist (e.g., a quote from Vera Cruz describing in highly techni-

cal terms surgical treatment of craniostenosis).

The writing is often painfully stilted—e.g., (p. 123), "The Leiter . . . Scale is an instrument sometimes used to measure ability. It is not commonly used or routinely used, but it can be very useful."

The philosopher of science would find much in this book to disturb him. The author chooses to arrive at a *definition* of mental retardation "inductively" rather than "deductively." The terms "entity" and "operational" are uncritically utilized. Such statements as the following are not uncommon (p. 184): "If the tool of the investigator is as sophisticated as factor analysis, the result is a contribution to the nature of things metaphysical as surely as the writings of Aquinas."

The book does succeed in communicating to the novice the notion that questions about mental retardation can best be answered through empirically gathered data. But the attempt to survey research studies from the several relevant areas is generally superficial and uncritical in nature. The volume is not recommended.

## Re-do by Rado

Sandor Rado

*Psychoanalysis of Behavior: Collected Papers Vol. II: 1956-1961.*  
New York: Grune & Stratton,  
1962. Pp. v + 196. \$6.50.

Reviewed by ERIKA FROMM

*Sandor Rado, author of this volume, is a psychoanalyst and political scientist who was born and trained in Hungary. In the 1920's he belonged to Freud's intimate circle and taught for some years at the Berlin Psychoanalytic Institute before coming to this country. He has had a varied academic and clinical career, and is now Professor of Psychiatry and Dean at the New York School*

*of Psychiatry. The reviewer, Erika Fromm, received her degree in Germany from Max Wertheimer in 1933, then after a stint at the University of Amsterdam came to this country in 1938. She settled down in Chicago where she has worked in a clinical capacity at the Michael Reese Hospital and at the Institute for Juvenile Research, has served as a school psychologist, has taught at the University of Illinois Medical School and at the Northwestern University Medical School. Her intellectual interests range widely from children to adults, from diagnosis to therapy, from tests of intelligence to projective techniques, from Gestalt to psychoanalytic theory, from therapy to experimental hypnosis. Presently she is Professorial Lecturer in the Department of Psychology at the University of Chicago, and is about to publish with Thomas M. French, *Dream Interpretation: a New Approach.**

THE FIRST volume of Rado's Collected Papers—reviewed by Tulin in this Journal in April 1957—spans a 35-year period of the author's publications. The second covers six years.

Strangely enough, the 18 papers comprising Volume II and written in the recent short time span are chronologically arranged, while Volume I is organized topically. In Volume II the same ideas are expounded over and over again that were already developed to the same point in the earlier book. Moreover, identical tables and figures are reprinted more than once between the covers of Volume II. This book could have been improved by adhering to topical organization, and by weeding out repetitiveness.

The field of schizophrenia is the sole area in which the author presents new research in his second volume of collected papers. He believes that schizophrenia is caused by a mutation of genes in the fertilized ovum from which the individual developed. These mutations produce two fundamental forms of damage in "the psychodynamic cerebral system": a diminished capacity for pleasure, the neurochemical basis of



which is not known, and a "proprioceptive diathesis"—neurophysiologic basis unexplored—which precipitates in the patient a distorted awareness of his own body. Rado distinguishes four developmental stages of "schizotypal" (his term for schizophrenic) behavior: (1) compensated schizotypal behavior without a breakdown, (2) decompensated schizotypal behavior marked by extreme overdependence and a severe obedience-defiance conflict, (3) disintegrated schizotypal behavior characterized by adaptive incompetence, and thought and action disorders, and (4) deteriorated schizotypal behavior with nearly complete withdrawal from reality-adaptive tasks.

RADO's claim is to have created a new science, an ego psychology which he calls Adaptational Psychodynamics. He seems to consider it as being the only psychoanalytic ego psychology in existence. Actually it differs from the main stream of psychoanalytic ego psychology primary in terminology; and in that Rado places more emphasis than most psychoanalysts do on the inheritance of psychological mechanisms. He speculates that psychological response potentials and deficiencies are coded into the genes. One can hope that Rado's genetic and biopsychologic speculations eventually will lead to a neuro-dynamic or biodynamic science. In their present state, they do not constitute a science yet. Just a theory.

In therapy, Rado places the interpretative emphasis on the patient's current life situation. So does Thomas M. French, who rates at least a footnote. Rado makes it appear as if he himself were the only one who has moved the emphasis in psychoanalytic therapy and research from the libido theory to the study of the adaptive and the integrative functions of the ego. The author discusses Sigmund Freud's work on the ego. However, one wonders why nowhere in this book, not even in the chapter entitled "Observations on the Development of Psychoanalytic Theory" written in 1960, he even mentions the so highly important development of modern psychoanalytic ego psychology that began in 1936 and came to full bloom in the 1950ies. The

work of Anna Freud, of Hartmann, Kris and Lowenstein, of Erik Erikson, to name but the most outstanding exponents, is treated as if it were non-existent. Rado, Don-Quixote-like, continues to fight the outdated windmills of that psychoanalytic period which ended a quarter of a century ago.

As to theories and techniques of hypnosis, the author seems to be unaware of the exciting new orientation of thought that began in this field 50 years after Freud turned his back on hypnosis.

The author insists that his own method of adaptational therapeutic technique is the only method of therapy that can be called reconstructive. All other psychoanalytic practice, according to him, is merely supportive and patchwork repair. He disapproves of allowing the patient to develop a transference relationship to the analyst and of using controlled transference regression as a therapeutic technique. He thinks it interferes with helping the patient attain self-mastery and self-realization.

Before 1933, Rado gained a place in the history of psychoanalysis for the important contributions he made to classical psychoanalysis, in particular to the understanding of the psychodynamics of drug addiction and of melancholia. He also deserves credit for having ever since attempted to construct a "comprehensive dynamics of human behavior that will include its cultural aspects, its physiology (biophysics, biochemistry), and genetics." Unfortunately, after an initial thrust forward, his thinking has circled within its same track, and he has not been able to enlist enough interest among his psychoanalytic colleagues, biopsychologists, geneticists and social anthropologists to motivate them to test his hypotheses.



*The soul's maladies have their relapses like the body's. What we take for a cure is often just a momentary rally or a new form of the disease.*

—LA ROCHEFOUCAULD



## Two Brains or One?

Vernon Mountcastle (Ed.)

*Interhemispheric Relations and Cerebral Dominance.* Baltimore: Johns Hopkins Press, 1962. Pp. v + 294. \$7.50.

Reviewed by MITCHELL GLICKSTEIN

The editor of this book, Vernon B. Mountcastle, one of the nation's leading neurophysiologists, received his MD at Johns Hopkins University and went back there to teach in 1948. He is now Professor of Physiology. He is chief editor of the *Journal of Neurophysiology* and is a contributor to *Bard's Medical Physiology* (11th edition) (1961). Psychologists may know best his early work with Philip Bard on the role of the limbic system in emotion. The reviewer, Mitchell Glickstein, remains essentially the same person CP recently said he was (CP, Feb. 1963, 8, 46). A University of Chicago man from one end to the other of his higher education, he left the midwest to teach at Cal Tech and Stanford and is now Assistant Professor of Psychology and Physiology at the University of Washington. He is especially concerned with problems of brain mechanisms in learning and in memory.

UNLIKE the heart or the liver, the brain is a symmetrically paired organ. It is the implications of this pairing—the problem of functional integration between the two hemispheres and the contrasting functional role of each—that provide the theme of this book. A composite of ten papers, together with discussions and a summary, this work is a report of a symposium held at Johns Hopkins Medical School in April 1961.

To some extent, all the papers deal with cortical function, but two related problems can be distinguished. One concerns the mechanism of integration between the hemispheres. The reports presented point to the major role that the forebrain commissures play in such integration. The other problem is related to the phenomenon of dominance. Although speech and language are sur-



prisingly little-mentioned, it is apparent that there is an underlying assumption by both the authors and discussants that left hemispheric dominance for language exists in man. Thus, functional asymmetry is assumed and much of the research presented is devoted to further analysis of the differences between the two hemispheres.

In this area, the book poses some old problems rather explicitly. Lesions of the left hemisphere in man are more likely to cause language disturbance than are lesions of the right. Yet Gerhardt von Bonin, reviewing gross anatomical studies of hemispheric comparison, found a remarkable unanimity among researchers on the equality of the two hemispheres. The right hemisphere weighs as much as the left, almost to the gram. Here, then, the book focuses on a problem, not a solution: if there are no obvious morphological differences, what is the basis for so extreme an asymmetry of function between the left and right hemispheres of man? Other authors explore different aspects of functional asymmetry. It appears that the left hemisphere is not completely dominant for all symbolic functions in man. Reasonably good evidence is presented that the right hemisphere may show a modest kind of dominance for some non-language functions. Brenda Milner, for example, provides data to suggest that the right temporal cortex might have a dominant role over the left for visual memory in man.

The descriptions of split-brain animal experiments by Sperry and others convey some of the excitement and promise of this area of research. It appears that the entire question of localization of memory in the brain has been reopened with the advantage of hindsight into some of the difficulties of Lashley's approach. One feels that the 'engram' is in principle localizable, even though the reported studies only confined it to one hemisphere. Much of the writing on commissural section is especially valuable since it has not appeared in other publications.

THE PARTICIPANTS in this symposium came from a variety of disciplines, and their specializations are reflected in

the type of material presented. There is a striking difference between the approaches of the clinical neurologists and the psychologists to what would appear to be a similar problem, assessment of the functional deficit following localized cortical injury in man. Psychologist Teuber's report, for example, is detailed and analytic, with a careful description of the behavioral referents for each function studied. In contrast, the neurologists tend to deal with behavioral units that are larger, more abstract and somewhat vague. Although some neurologists have discovered the contingency table, this is apparently not enough, since major disagreements persist among them. Granted that they must deal with inherently variable material, studying accidents of nature as they do, one feels, nevertheless, that the vagueness in their concepts contributes to their disagreements. Neurology seems to be awaiting its John B. Watson.

In the book's beginning, J. Z. Young poses the question, "Why do we have two brains"? He then attempts to answer it with special reference to the phylogeny of vision and the phenomenon of decussation in the visual pathways. While Dr. Young presents a stimulating discussion of the kind of sensory mapping that might be mediated by different types of neural structures, one cannot entirely agree (as did few of the discussants) with Dr. Young's postulated 'reason' for the existence of crossing in the visual pathways. It is perhaps unfortunate that the delightful Aristotelean game of "why is it there"? often ends in these unresolvable disagreements among the players.

At the other end of the book Richard Jung gives an excellent summary of the entire conference. This is a difficult task and one that Dr. Jung handles with consummate skill. He manifests a rare ability to deal adequately with data from both clinical neurology and experimental studies and then to apply them to the common problem of brain function.

Presenting as it does some of the best current work and thought on the function of the human and animal cortex, the book seems a bargain at the publisher's price of \$7.50.

## Toward Unity on Conflict

Muzafer Sherif (Ed.)

*Intergroup Relations and Leadership: Approaches and Research in Industrial, Ethnic, Cultural, and Political Areas.* New York: Wiley, 1962. Pp. v + 284. \$5.95.

Reviewed by J. STACY ADAMS

Muzafer Sherif, the editor here, is a native of Turkey who was educated at American International College, Istanbul, at Harvard and Columbia Universities, receiving his PhD from the latter. There may be in the world one or two social psychologists who might need an introduction to him and perhaps there are a dozen other psychologists who might not know who he is. His most recent book, written in collaboration with Carl Hovland, was *Social Judgment: Assimilation and Contrast Effects in Communication and Attitude Change* (CP, Sept. 1962, 7, 320-322). The reviewer, J. Stacy Adams, is a product of the University of North Carolina, who after teaching for a while at Stanford University joined General Electric's Behavioral Research Service in 1960 and remains there as a consultant. Also he functions as Adjunct Associate Professor at Columbia University. Presently he conducts experimental research in industry where he productively puzzles over the functioning of industrial reward systems and inequities, with his thinking about these matters showing the influence of both Skinner and Festinger.

WHENEVER fifteen scholars representing disciplines as varied as anthropology, geography, political science, psychology and sociology gather to present and discuss papers on intergroup relations and leadership, one might expect a diversity so great as to make a coherent volume an editorial impossibility. The expectation is not realized in the case of the book under review, for two reasons. First, Professor



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Sherif, as editor, has contributed an insightful, unifying, introductory chapter and brief, prefatory comments on each chapter. Secondly, conflict resolution implicitly or explicitly underlies the interests of many of the contributing authors. The net impression left with the reader is that this collection of symposium papers given at the University of Oklahoma in April, 1961, might better have been entitled "Approaches and Research in Intergroup Conflict Resolution." Indeed, the majority of papers are logical additions to Sherif's *Groups in Harmony and Tension*, and, whether intentionally or not, constitute a tribute to that enlightening work.

The central interest in resolving group conflict is expressed through a variety of viewpoints and data. The sociologist, R. E. L. Faris, and the psychologists, R. R. Blake and Jane S. Mouton, forcefully adopt the position that intergroup phenomena, conflict in particular, cannot be understood in the terms of individual psychology. Disposing of the frustration-aggression hypothesis, the concepts of displacement, of catharsis, and of the authoritarian personality as being useless, if not irrelevant, in explaining the arousal of conflict between groups, Faris proposes that only concepts at the level of groups have explanatory value. What these concepts are and how they may be defined operationally is not quite so clear. Yet, Faris' position, though perhaps overstated, is persuasive and is reinforced by Blake and Mouton, who caution against the "psychodynamic fallacy" in union-management relations—the attribution of motivation to factors within an individual rather than to properties of the group of which the individual is a member. Empirical support for this viewpoint also comes from the sociologist, L. M. Killian, in his scholarly historical tracing of changes in negro leadership before and following the 1954 U.S. Supreme Court school desegregation decision. He cites recent studies that demonstrate that there is little or no correlation between the distribution of individual attitudes on desegregation and "the adamance of community resistance to change."

Despite the arguments and data advanced in the plea for theories based on the dynamics of groups *qua* groups, one cannot reject individual psychodynamics as being wholly irrelevant. Statements like Faris', "The office makes the man, not the man the office, in the normal experience of persons in organizations," may have superficial validity and appeal, but cannot stand close scrutiny. Even "normal experience" suggests that intergroup phenomena are in part a function of the characteristics of individuals. The charismatic leader *does* exist. The work of G. W. Allport on the nature of prejudice, of T. F. Pettigrew on the dynamics of desegregation, and of Freud, Sears, Dollard and others *do* have validity. Fortunately, two of the authors, S. S. Sargent, in his commentary on Killian's paper, and R. M. Stogdill, in his presentation of a model for the study of leadership, restore a conceptual balance.

A number of chapters by non-psychologists bring fresh material and viewpoints to the field of intergroup conflict. One, by R. C. North, reflects the influence of psychology upon political science as the author attacks problems of international relations. North warns against the traditional, rational approach to the study of relations between nations in which the emphasis is upon the evaluation of "hard and objective" variables such as economic capacity and military strength. The political scientist, he states, must also take account of the perceptions that policy and decision makers have of these variables. He continues by suggesting methods for the study of documents and by presenting a fascinating analysis of the genesis of World War I. Professor North's paper is the clearest evidence this reviewer has seen that interdisciplinary work of the type undertaken at Stanford University on International Conflict and Integration can have high pay-off value.

The Oklahoma interdisciplinary symposium, of which editor Sherif's volume is a report, should also bear fruit; but not only because it was interdisciplinary in character, including, as it did, material by the geographer, R. E.

Crist, on the effects of land tenure systems upon intergroup conflict and observations by the anthropologist, A. R. Beals, on the rise and decline of factionalism in a village in India. The quality of the papers presented at the symposium also is such as to provoke the use of new concepts and approaches to a problem that is of more than academic interest.

## Psychomotor Epileptics

John Guarrant, William W. Anderson, Ames Fischer, Morton R. Weinstein, R. Mary Jaros and Andrew Deskins. With a Foreword by Robert B. Aird

*Personality in Epilepsy*. Springfield, Ill.: Charles C Thomas, 1962. Pp. vii + 112. \$6.50.

Reviewed by VICTOR MILSTEIN

Six of the authors of this book are physicians who were located, when the book was written, in or around the University of California Medical Center in San Francisco. The seventh author is Andrew Deskins, a psychologist, who, since his work on this project, has moved from San Francisco to the Department of Psychology at Alma College in Michigan. The reviewer, Victor Milstein, received his PhD degree in psychology from the University of Oregon and has stayed on at Oregon as a USPHS Research Fellow in the Medical School there. His dissertation research dealt with epileptiform EEG discharges and learning, and his subsequent research has been on related problems.

THIS BOOK is the report of a research project. It seems likely that the decision to publish in book form rather than in a journal was made because of the great amount of interest and con-



troversty about the subject. The hypothesis tested was that psychomotor epileptics show more psychiatric disturbance than patients with other chronic illnesses.

The book begins with a review of the recent history of personality in epilepsy. This is interestingly organized into four periods, culminating in "the period of psychomotor peculiarity." The authors emphasize that opinions in this area are not based on experimental studies. Further, "the many studies since then have scarcely gone beyond Esquirol's [more than 100 years ago in the Salpêtrière] in the selection of subjects, the control of non-experimental variables, and in efforts to restrict the effects of bias."

Three groups of subjects—psychomotor epileptics, grand mal epileptics, and patients with non-neurological medical illnesses—were selected from clinic out-patients who met certain criteria of age, literacy, and physical condition. All subjects were first evaluated by the neurologists and required to meet still further stringent criteria appropriate to the group in which they were placed. The patients were then independently examined by two of the psychiatrists who were aware of each subject's history and diagnosis. The psychiatrists rated the patients on 102 items in three categories: non-seizure symptoms and behavior, interview characteristics, and maladaptation. The subjects also had a battery of eight psychological tests, half of which were projective, administered by a psychologist who was unaware of the group to which the patient belonged. Fifty-five ratings and scores were derived from this battery. Comparisons of the ratings, psychiatric diagnoses, and test scores indicated that there was a high but essentially equal degree of psychopathology among the three groups.

THIS brief summary indicates that this research does not go as far beyond Esquirol as it might have. Despite the selection criteria employed, the two convulsive groups differed significantly from the medical illnesses group in age, race, and duration of illness. The influence of these factors on the variables investigated might be negli-

gible, as the authors argue, but this has not yet been demonstrated. The same might be said of other factors like seizure severity, degree of EEG background abnormality, and on which side a unilateral focus occurs. There is no comment on the fact that 75% of the psychomotor group had unilateral EEG foci (only 25% showed bilateral signs), while 69% of the grand mal group had generalized dysrhythmia; the rest had normal EEGs. Further, it might have been instructive to compare psychomotor epileptics with non-temporal lobe focal epileptics as well as with grand mal patients.

The authors argue that the independence of the psychiatric interviews "protect" against bias. Consequently, there is much concern for inter-rater reliability, which is good. However, there are simpler experimental designs for insuring against the intrusion of bias. There is also the 'bias' that may follow from making comparisons among the three groups on 55 mean scores derived from psychological tests without first performing some overall analysis.

There are a number of places in the report where comment and amplification would be welcome. This is especially annoying in a book-length report. The most disturbing occurrence of this concerns the psychiatrists' ratings. These are presented as headings only, and it is of limited value to know that the psychomotor group shows significantly more "memory difficulty" and is significantly more maladapted in the area of "parents" than the medical illnesses group.

The first part of the book tends to encourage a critical set in the reader which is carried over to the reported study. Nevertheless, it should be emphasized that the authors were aware of, and avoided many pitfalls into which others have fallen. This permits us to place more reliance in their findings than we might otherwise do.



... when a man runs the wrong way,  
the more active and swift he is the  
further he will go astray.

—SIR FRANCIS BACON



## Testing CP Persons

Robert M. Allen and Thomas W. Jefferson. Introduction by James F. Garrett

*Psychological Evaluation of the Cerebral Palsied Person: Intellectual, Personality, and Vocational Applications.* Springfield, Ill.: Charles C Thomas, 1962. Pp. v + 86. \$5.00.

Reviewed by SALVATORE G. DiMICHAEL

The senior author, Robert M. Allen, is a clinical psychologist who, since 1947, has been Professor of Psychology at the University of Miami. He directs the University's clinical training program and is consultant to the United Cerebral Palsy Center. Thomas W. Jefferson, co-author, is Chief Psychologist at the Miami Cerebral Palsy Rehabilitation Center and also Associate Professor of Psychology at the University. Salvatore DiMichael, the reviewer, is a Fordham PhD who taught at St. Louis University and the Catholic University of America, before entering full time work in the area of rehabilitation. Presently he is the New York Regional Representative of the Vocational Rehabilitation Administration of the Department of Health, Education, and Welfare. Earlier he was the first Executive Director of the National Association for Retarded Children and was the founding president of the Rehabilitation Counseling Division of APGA. He has also served as President of Division 22 of APA.

THE AUTHORS have sought to bring together the testing "know-how" scattered among diverse sources. They hope that the 81-page text and bibliography will "whet the curiosity of the professional person directly concerned with the administration, interpretation and application of test findings, with the cerebral palsied person." There is much useful know-how brought together, heavy on the "how-to" and light on "what-it-means." The book will be most helpful to trained psychologists with little or no



experience with (re) habilitation and cerebral palsied persons. It assumes broad knowledge of psychological and vocational tests, and especially evaluation.

Most of the manual is devoted to intelligence tests, 50 pages consecutively and 4 more under another chapter heading. This is the best part of the book. The authors present intelligence tests which can be used with all except a very small percent of severely multiply-disabled persons. The mainstay test is the picture vocabulary type in which the psychologist names an object and the client selects from several possibilities. The client's choice may be indicated by any prearranged sign, even the flicker of the eyelid.

The authors admittedly adopt a "liberal definition" of intelligence; in fact, it amounts to a global view in which intelligence is viewed as adaptation to the whole of life. They advocate that the tests results be used as part of the assessment of intelligence and that the whole case study must be employed. "In the final analysis," they say, "there is no more valid information (of intellectual status) than the knowledge of how an individual has lived his life and met his problem." Many psychologists, the reviewer included, would prefer to limit the definition of intelligence.

AFTER the long "how-to" focus on intelligence tests, the book shifts in quick sweeps to issues and viewpoints on tests of personality and vocational adaptabilities. One of the issues mentioned early in the Preface is whether one should be sentimentally easy, or as "tough-minded" in assessment as the authors declare themselves. However, being "tough-minded" is not as bad as it sounds, it means using regular test norms based on scores for non-handicapped, with whom the cerebral palsied are to compete.

The book hardly warms up to personality assessment in the 7 pages devoted to the chapter. This section briefly mentions several kinds of possible tests and techniques, and leaves the matter up to the reader himself. "In summary," says the text, "the ad-

ministration of personality tests permits much greater freedom but this brings with it a need to be more cautious in the interpretation of the protocols." The difficulty is that the latitude on the "how-to" steps up the problem with evaluation, on which little is said.

The psychologist in quest of help on vocational assessment is told that he must rely on his ingenuity in selecting or modifying existing tests. One may infer that the general suggestions picked up in the section on intelligence tests can be used in choosing, adapting, and evaluating vocational tests. Then, the text points out that test performances of most cerebral palsied persons are below the cutting scores for occupational areas on such vocational tests batteries as the General Aptitude Test Battery. The psychologist is not told what this means as he tries to be positive, and tough-minded in evaluating the abilities of the individuals in the group. Again the need for suggestions on evaluation of results becomes apparent, but the book does not so address itself.

The manual strives to be pithy and to the point. We can be sympathetic with the aim. It would have been more helpful, however, if the coverage on personality and vocational assessment, and especially the evaluation of test results had been substantially increased.

The book makes it abundantly clear that skillful assessment of the cerebral palsied is difficult. Even valid and reliable tests of intelligence, which tap different abilities, may give a wide series of scores. Moreover, there are highly individualized effects of brain damage from one person to another. The complexities and hazards are such that psychologists will base conclusions only on a full case study.

In my opinion, the psychologist confronted by the possibility of evaluating a cerebral palsied person will have to decide whether to refer to a specialized associate, or to start himself in earnest in becoming experienced. This reviewer suspects that we become "tough-minded," "empathic," "realistic," and "positive," only with sound experience and the self-corrections learned in follow-up on our recommendations.

## Please Not What, Only How

William W. Cooley and Paul R. Lohnes

*Multivariate Procedures for the Behavioral Sciences.* New York: Wiley, 1962. Pp. vii + 211. \$6.75.

Reviewed by QUINN McNEMAR

*The authors, William Cooley and Paul Lohnes, both hold EdD degrees from Harvard, with educational measurement as their area of specialization. Both have been influenced by Phillip Rulon and Jack Carroll, and both are now Associate Professors of Education, Cooley at Harvard and Lohnes at New Hampshire. The reviewer, Quinn McNemar, a three-hat (Psychology, Statistics, and Education) professor at Stanford, will, when pressed, profess a mild interest in global methods but still sees merit in an experiment so neat that a desk calculator suffices.*

ABOUT half of this slim volume is devoted to explication of, and examples for, the following techniques: multiple correlation, canonical correlation, analysis of variance and multiple covariance (both up to and including three-way designs), multiple discriminant function analysis, classification of persons via the centour (*centile contour*) method, and factor analysis (principal components and varimax rotations). The authors do not pretend that their sketchy introduction to these procedures is adequate—the novice will need to turn to the cited references if he desires understanding.

The other half of the book is made up of flow diagrams and computer programs built around the FORTRAN coding language. Either without or with modification, the programs are usable with IBM's 650, 704, 709, 1620, and 7090 and with Autonetic's RECOMP, Control Data Corporation's 1604, Philco 2000, Honeywell 800, and Univac 1107. Some of the programs were originated by the authors, some are borrowed. This



## HISTORY, PSYCHOLOGY, & SCIENCE Selected Papers

By EDWIN G. BORING, *Edgar Pierce Professor of Psychology, Emeritus, Harvard University*. Edited by ROBERT I. WATSON and DONALD T. CAMPBELL, *both Professors of Psychology, Northwestern University*. "Here is a selection of essays coming from a specialty that may be called the *science of science* . . . Since the time of Hume it has been recognized that the inductive achievements of science cannot be proven in any logical, deductive fashion. Not as well recognized is the fact that this limitation holds not only for specific inductions but also for any general principles of efficacious inductive procedure . . . science of science—propositions as to how science develops, propositions as to effective strategies, criteria of

proof, doctrine as to the necessary controls in experiments—will involve assumptions as to the nature of the world and of man as knower which are not deductively provable and will involve questions upon which empirical evidence is relevant . . . E. G. B. is a scientist of science, for he has been contributing to these three major areas—the history of science, the sociology of science, and the psychology of science—for some forty years. Here lie his best papers . . . from widely scattered journals devoted to other specialties and consequently unread by many to whom they are relevant."—from the Editors' Foreword. 1963. 372 pages. \$8.95.

### MATERNAL BEHAVIOR IN MAMMALS

Edited by HARRIET L. RHEINGOLD, *National Institute of Mental Health*. A stimulating and informative collection of original research reports on maternal behavior in infrahuman mammals. 1963. Approx. 336 pages. Prob. \$7.50.

### THE DESIGN OF ELECTRIC CIRCUITS IN THE BEHAVIORAL SCIENCES

By TOM N. CORNSWEET, *University of California, Berkeley*. This book combines basic electrical theory with pictures and descriptions of real pieces of apparatus—and enables the scientist who has no training in electricity to design complex switching and timing circuits to meet his own research needs. 1963. Approx. 344 pages. \$8.95.

### HANDBOOK OF MATHEMATICAL PSYCHOLOGY—In Three Volumes

Edited by R. DUNCAN LUCE and ROBERT R. BUSH, *both of the University of Pennsylvania*; and EUGENE GALANTER, *University of Washington*. A compact and convenient summary of research, and a bibliography of all the work done to date. Volume I: 1963. 491 pages. \$10.50. Volume II: 1963. 606 pages. \$11.95. Volume III: In Press.

### READINGS IN MATHEMATICAL PSYCHOLOGY—In Two Volumes

Edited by R. DUNCAN LUCE, ROBERT R. BUSH, and EUGENE GALANTER. A companion to the HANDBOOK and a valuable reference source for readings that have been widely scattered in journals of mathematical psychology. Volume I: 1963. 535 pages. \$8.95. Volume II: In Press.

### SYMBOL FORMATION: An Organismic- Developmental Approach to the Psychology of Language

By HEINZ WERNER and BERNARD KAPLAN, *both of Clark University*. This is the first attempt to deal with symbol-formation from an organismic-developmental point of view. It develops both linguistic and non-linguistic symbolization in normal ontogenesis, dream states, and schizophrenia. 1963. Approx. 496 pages. Prob. \$11.50.

### THEORY AND RESEARCH IN PROJECTIVE TECHNIQUES (Emphasizing the TAT)

By BERNARD I. MURSTEIN, *Connecticut College*. A thorough examination of the determinants of responses to projective techniques—with special stress on the TAT. 1963. Approx. 392 pages. Prob. \$8.50.

### THE PRACTICE OF SCHOOL PSYCHOLOGY: PROFESSIONAL PROBLEMS

By ROBERT E. VALETT, *Sacramento City Schools*. A comprehensive treatment of professional problems in school psychology—both theories and applications are illustrated by actual case studies. 1963. Approx. 352 pages. \$7.50.

### T-GROUP THEORY AND LABORATORY METHOD: Innovation in Re-education

Edited by LELAND P. BRADFORD, *National Education Association*; JACK R. GIBB, *Consulting Psychologist*; and KENNETH D. BENNE, *Boston University*. The first complete account of the training laboratory and of current theories about its most distinctive methodology—the unstructured T-Group. 1964. Approx. 504 pages. Prob. \$9.50.

### A THEORY OF DATA

By CLYDE H. COOMBS, *University of Michigan*. An original analysis of the foundations of psychological measurement and the inferential classification of stimuli and people. 1964. Approx. 608 pages. Prob. \$14.95.

### SYSTEMS OF PSYCHOTHERAPY— A Comparative Study

By DONALD H. FORD, and HUGH B. URBAN, *The Pennsylvania State University*. The result of five years of research, this book compares and contrasts ten systems of psychotherapy. 1963. Approx. 816 pages. In Press.

### GAME THEORY AND RELATED AP- PROACHES TO SOCIAL BEHAVIOR

By MARTIN SHUBIK, *IBM Corporation*. Discusses applications of the theory of games to the political, military, social, and behavioral sciences. 1963. Approx. 384 pages. Prob. \$3.95.

JOHN WILEY & SONS, Inc.

605 Third Avenue, New York, N. Y., 10016.



may be a useful compilation, but the book is not sufficiently self-contained to permit the reader to secure an understanding of programing.

As frequently happens, the illustrations are not always good examples of what a behavioral scientist should be doing when he comes under the spell of a computer. For instance, in an example for multiple correlation not a single one of the 10 predictors correlates significantly (.05 level) with the criterion—why do a multiple? (It, too, was insignificant.) Why a covariance adjustment via WAIS scores (V and P) for means on ACE and for means on five other aptitude tests? This is analogous to partialling intelligence out of intelligence. A second example of

multiple covariance adjustment will not be inspirational; since the seven group differences for seven control variables are small, the adjustment reduced the difference between the means of an experimental variable by a trivial .06 of a standard score unit. As for factor analysis, the application to Rorschach variables, 48 in number, of the principal components solution followed by varimax rotation turned out to be an unhappy example because of the failure to partial out total number of responses—a general productivity factor gets lost in, and fouls up the meaning of, the rotated factors.

Let's hope that the programmers and the computers don't tell us what to do but only how to do it.

ratory investigators and systematists, the result is often far from felicitous.

One of the most obvious and specific ways in which the eccentricity of this volume is reflected lies in the fact that although numerous other writers are mentioned in the text, no bibliography is provided; and there is also no index of authors or subjects. We may read: "As Adolph has said, 'Regulations in organisms are maintenances of relative constancies'" (p. 66), or "Young has showed that rats have a hierarchy of food preferences for different parts of their diet" (p. 67). And sometimes the work cited is unique, extremely interesting, and not generally known (cf. that of Crile, p. 152 ff.). But we are given no indication as to when and where Adolph, and Young, and Crile originally reported their findings.

Then there is a peculiar selectivity in the way writers or investigators are mentioned, or not mentioned. When someone has reported data which Dr. Tomkins can reinterpret or utilize in supporting his own conceptions, such data are likely to be mentioned; but when another author or authors have already published an interpretation very similar to that which Tomkins is elaborating, their work is likely to be completely ignored. For example, beginning on p. 115, there is a long discussion of "human-like automata" which contains a great many ideas which I first heard set forth, more than a decade ago, in a series of lectures by another Princeton scholar, Dr. John Von Neumann. Yet there is no reference whatever to von Neumann's work. Repeatedly, concepts are stressed which are very similar to those presented by Miller, Galanter, & Pribram in their book, *Plans and the Substructure of Behavior* (1960). But again this work is not alluded to. Many similar instances could be cited.

**B**UT aside from questions of style and conventions, what is the book about? It is about so many (often not very clearly related) things that it is not easy to say. However, on p. 342 we read:

"For some time now both Psychoanalysis and Behaviorism have regarded

## Reductionism in Reverse?

Silvan S. Tomkins. With the editorial assistance of Bertram P. Karon

*Affect, Imagery, Consciousness. Vol. I: The Positive Affects.* New York: Springer, 1962. Pp. vii + 522. \$8.00.

Reviewed by O. HOBART MOWRER

The author, Silvan Tomkins, received his PhD from the University of Pennsylvania and then went to Harvard where he stayed for about ten years as research assistant, instructor and lecturer before moving to Princeton in 1947, where he has remained. His list of publications include, *PAT Interpretation* (1959), and with J. B. Miner, *The Tomkins-Horn Picture Arrangement Test* (1956). The reviewer is Hobart Mowrer, and he says of himself this time "same as before, nothing new. Was recently 'written up' in *The Christian Advocate*—copy enclosed but not relevant." See CP, July 1963, 8, 259, for his last review and for CP's brave but hopeless attempt to put his biography in a small space.

**T**HIS is a book abounding in contrasts and paradoxes. It contains, on the one hand, some of the most brilliant and

insightful writing in all psychological literature; but it also contains some of the dulllest and least useful. In areas where the author has first-hand data and has thought deeply, the result is prose of rare lucidity, compactness, and beauty; but when the literature of a field remote from his special competence is under review, the result is often sheer academic hackwork.

There is a strange admixture of a philosophic-naturalistic emphasis in the over-all approach, combined with wide familiarity with and reference to the experimental findings of many fields. A part of the charm and force of certain sections of this book is undoubtedly the capacity of the author to let his own excellent powers of observation and induction play over empirical data and to look at them in novel, often shrewd and refreshing ways. But when an effort is made to mesh this approach with the more conventional perspectives of labo-



interest as a secondary phenomenon, a derivative of the drives, as though one could be interested only in what gave or promised drive satisfaction. We have turned this argument upside down. It is interest or excitement, we have argued, which is primary and the drives are secondary."

In some ways this statement can be taken as epitomizing the entire book; and certainly the title, *Affect, Imagery, Consciousness* is congruent with such an emphasis. But there are many other loosely related (if not quite irrelevant) themes; and the book as a whole is diffuse and poorly organized. The concluding chapter is a tedious and detailed discussion of the startle response and in no sense a summary of the book as a whole. At no other place is there a master plan (except in the Table of Contents) for this book or any indication of just how it relates to the other two companion volumes which are projected. This volume is subtitled "The Positive Affects"; the second one, "The Negative Affects," and the third, "Cognition and Ideology." But all these topics are repeatedly alluded to in the first volume so that a clear principle of organization is still not apparent.

In some respects, Chapter I (entitled "Consciousness and Affect in Psychoanalysis and Behaviorism") may be taken as thematic for the book as a whole, with subsequent chapters constituting merely gargantuan footnotes. This reviewer heard the original version of Chapter I presented as a paper at the 14th International Congress of Psychology in Montreal, in 1954, and can testify to the great interest and excitement which it occasioned. And in the form in which it is here published, it is still a remarkably penetrating and cogent document. But the rest of the book does not build directly and explicitly on this chapter; and by the time one reaches the last chapter, one wonders how the author (unless by a kind of "free association") ever got on the subject of the startle response—and why it deserves such elaborate consideration.

With whom, then, will this book be popular? It is easier to indicate those with whom it will *not* be. The book

could have been written in such a way as usefully to extend systematic learning theory. Instead, it is a rebuke to such theory and not likely to have much appeal to learning theorists. It could also have been a major contribution to the re-emerging field of cognition, but the author's reluctance to acknowledge the work of others seriously reduces its usefulness here too. It is not a clinical or social psychology, in any conventional sense of these terms. In some ways it comes closer to being physiological psychology than anything else. The author has obviously been deeply influenced by Darwinian concepts and applies them in a pervasive, often very ingenious way. But physiological psychologists and neurophysiologists will be repelled by the general looseness and lack of rigor.

What will be the reaction of students? If space permitted, it would be possible to quote sentences so long and so complicated that any but the most leisurely and sophisticated reader will find them quite unintelligible. And there are no pedagogical aids which would assist the untutored reader in grasping and retaining the subject matter.

In over-all appraisal, many readers will regard this book as grandiose and solipsistic—qualities which the author himself recognizes when he says that he deliberately chose "to stress what was novel, idiosyncratic and theoretical rather than the historical, the polemical and the empirical bases and implications of my views" (p. x). One can only admire an author for such splendid indifference to conventional and utilitarian considerations; but it may cost him a reciprocal indifference on the part of many others. This volume is not a text, not a reference book, and certainly not a "popular" book. It is rather the "journal" of a gifted mind which, however, is apparently more intent upon being "original" than orderly.



*Genius consists in an infinite capacity for enduring pain—and for inflicting it.*

—WILFRED TROTTER



## Educating for Citizenship

Herbert H. Hyman, Charles R. Wright and Terence K. Hopkins

*Applications of Methods of Evaluation: Four Studies of the Encampment for Citizenship.* Berkeley, Calif.: University of California Press, 1962. Pp. v + 396.

Reviewed by KENNETH RING

All three authors, Hyman, Wright and Hopkins, obtained their doctoral degrees from Columbia University and all three, in spite of the fact that Herbert Hyman started off in social psychology, are now functioning as sociologists. The reviewer, Kenneth Ring, did graduate work at the University of Minnesota with Harold Kelley. Stanley Schachter and Ben Willerman, which leads to his pending doctoral degree. Presently he is Assistant Professor of Psychology at the University of Connecticut where he pursues his research on small groups.

FOR six weeks during the summer about one hundred young people (most of them are between 18 and 23) attend a rather unusual camp. Through a variety of techniques, ranging from formal instruction to casual 'bull-sessions,' an attempt is made to inculcate in the campers a persisting concern with social problems and social action and, in general, to equip them for responsible citizenship in a democratic society. This, of course, is the Encampment for Citizenship.

Now, while it may seem ungracious to do so, one may ask: just precisely what does this Encampment accomplish? Does it in fact realize its stated objectives? Are the changes it seeks to effect relatively permanent or merely transitory? Indeed, might not such a program even engender unanticipated and undesirable consequences, e.g., increasing feelings of alienation or impotence? To the sponsors of the Encampment, these are obviously serious questions; to the authors fell the task of providing the answers.



As is made clear in the first part of the book, the path of evaluation is one strewn with all manner of traps and snares. It is easy to see that it will not always be possible to apply rigorously the canons of experimental design in the Encampment setting in which the investigators' control over relevant conditions is negligible; additional problems arise when an assessment of the long-term effects of the Encampment is attempted. What is not so easy to see are reasonable and *meaningful* alternative procedures when the standard tools of evaluation are no longer applicable. It is precisely with such issues that the authors deal in the book's first section—here they present their approach to problems of conceptualization of the program's objectives, the design of suitable measuring instruments, the establishment of *appropriate* control groups and the like. Not surprisingly, the authors are not uniformly successful in dispatching the many difficulties which confront them (their treatment of alternatives to conventional control group procedures is, however, especially thorough, thoughtful and imaginative); keen awareness of the problems of evaluation, rather than facile solutions for them, is reflected here. It is, of course, scarcely the authors' fault when they fail to propose acceptable answers to questions for which there may well be none.

The rest of the book consists of the application of the principles and suggestions of Part I to the Encampment program. The authors were able to conduct an extensive study of the 1955 Encampment; in addition, there were three replications in later years which, by and large, confirmed earlier findings. In the most detailed investigation (1955), the effect of the program was not only assessed at its termination, but two follow-up studies were conducted: one six weeks later, another after an interval of four years.

Did the Encampment make a difference? The authors' data lead them to conclude that it did. In spite of the fact that on many issues the campers couldn't 'improve' much (they were initially imbued to a considerable extent with the values the Encampment sought to instill: as compared to others of their

own age and status, they were—before the Encampment—extremely liberal, tolerant, and democratic in their views), still many of them moved closer to the Encampment's ideals (this was consistent over replications). In spite of this over-all favorable outcome, several cautions must be indicated: (1) many of the changes were of small magnitude

(even when restriction of range was not a factor); (2) on many relevant variables, the Encampment had little, if any, effect and occasionally a deleterious one; (3) the long-term findings, both with respect to attitudes and actions, while they appear to be positive, are not so unambiguously interpreted as those from the six week follow-up.

## Making Perceptive Physicians I

Ralph W. Heine (Ed.). C. Knight Aldrich, Edgar Draper, Mary Meuser, Jean Tippet and Harry Trosman

*The Student Physician as Psychotherapist.* Chicago: University of Chicago Press, 1962. Pp. xiii + 241. \$5.00.

Reviewed by JEANNE S. PHILLIPS

*All of the authors are at the University of Chicago in the Department of Psychiatry, and have been intimately involved in the teaching program described in their book, begun after Aldrich became Department Chairman in 1955. The editor of the book, Ralph Heine, is well known in APA circles. He has a PhD from the University of Chicago and served with the VA and OSS and at the University of Minnesota before returning to Chicago to profess in the Department of Psychiatry. The reviewer, Jeanne Phillips, has, since 1953, been successively peer, teacher, and researcher in relation to senior medical students as they learn, in such settings as described in this book, about personality and psychotherapy. After completing an internship in clinical psychology at Washington University Medical School and a PhD from Washington University she went for a while to the Massachusetts General Hospital to do clinical and research work oriented around the psychotherapeutic interview, before she took her present position as Assistant Professor of Medical Psychology and Director of the Psychology Outpatient Clinic at the University of Oregon*

*Medical School. Her research endeavors, shared with several collaborators, are concerned with defining and refining the process whereby medical students learn interviewing skills.*

THIS book recounts the experiences of the authors in teaching psychotherapy to three classes of senior medical students (some 200 individuals) during a seventeen-week externship in the psychiatry out-patient department of the University of Chicago School of Medicine. The aim of the four year sequence of psychiatric instruction is "broadening the students' understanding of the personality factors in themselves and in their patients which affect all forms of medical treatment." The aim of the senior externship itself is re-integration of earlier learned principles through the student's experiencing, with one patient, in a sustained and closely supervised fashion, the "use of himself as a therapeutic agent."

Beyond describing and evaluating their own program and thereby stimulating others to do likewise, publication of this account was intended



to induce medical educators to re-evaluate the attention they give both to "the inculcation of humanistic values" and to systematic instruction in the interpersonal maneuvers which make up the "art of medicine." This goal the authors see as increasingly significant because, for multiple reasons, "while the medical profession has been moving in a direction which has the net effect of de-emphasizing the doctor-patient relationship, the patient population has been seeking more services which can best be provided in the context of a close continuing relationship."

The probable consequences of the report of the Joint Commission on Mental Health and of current federal advocacy of expanded mental health facilities and training programs give this volume a broader relevance than the title or stated purpose would at first suggest, however. Housewives and policemen, ministers and welfare workers (as well as the generalist and specialist physicians focused on by Heine and his co-workers) are being viewed as potential therapeutic agents both within their traditional roles and as psychotherapists so defined. The observations and lessons discussed in the context of the medical student's initiation into utilization of psychological knowledge will be relevant and valuable to the educators of this more varied cadre of therapists, and to those who would evaluate the effectiveness of these programs.

Especially useful, since they provoke thoughtful consideration if not necessarily agreement, are sections dealing with difficult faculty decisions: whether and how to pre-select and 'prepare' patients to be seen by novices; whether and how to promote self-understanding without violating the student's role by 'therapeutizing'; to what degree should the breadth and realism of diagnostic screening activities be sacrificed for the depth and continuity of focusing on one therapeutic case. Descriptions of behaviors and difficulties (and absence of anticipated difficulties) of the typical neophyte therapist, as well as a section on special 'problem' students, offer helpful guides. Extensive questionnaire data depict a generally favorable student reaction, and em-

phasize the novice's tendency to utilize 'distancing' himself from his patient as a major self-protective device.

A MAJOR virtue of the presentation, its comprehensiveness, is its major vice as well. In trying to cover so many aspects of a large educational endeavor, including both broad philosophy and details of method, anecdote and objective data, the exposition is uneven and at times repetitive. Extensive information is provided, for example, about characteristics of the patient population while only appetite-whetting hints are given about typical student interview behaviors and content. The reader is left frustrated by wanting to know *either* more about course content, teaching and supervisory techniques, typical sequences of student learning, etc., *or* more about 'interaction effects' of patient and student characteristics, *or* more detail concerning the variables involved in the questionnaire data.

The chapter on the questionnaire results, which comprises over one-quarter of the text, is especially frustrating in this regard. For example, chi-squares are reported for some (but not all of the 'significant') relationships without indication of the number of subjects, basis of cut-off scores for forming categories, or definition of such terms as "defensiveness." Statistical association is discussed frequently in terms of cause-and-effect despite the authors' own careful warnings in this regard. This large section suffers, then, for the psychologist reader, from its incompleteness as a research report and from the fact that it is based on an empirical study with the usual consequent problems. Unavailability of comparative data (do supervisors, as some comments hint, 'distance' themselves from students?) is probably an unavoidable but nevertheless regretted factor. Nonetheless, in suggesting hypotheses for further investigation and in describing apparent attitudes of novice therapists, these data are instructive for planners and evaluators of similar programs.

Although such elements as the heavy emphasis on teaching of self-knowledge while techniques are left to

'just grow' (and are assumed to be desired by students largely as a defensive maneuver) will leave some readers searching for a compromise orientation, Heine and his collaborators have provided both a provocative discussion of an ambitious program, and a comprehensive overview of the multiple factors in such a program, factors varying from the influences of the institutional and cultural environment to the problems of (and norms for) measuring the program's results in patient improvement and in student learning.

## Making Perceptive Physicians II

Stanley H. King

*Perceptions of Illness and Medical Practice.* New York: Russell Sage Foundation, 1962. Pp. 405. \$6.00.

Reviewed by JULIA S. BROWN

The author, Stanley H. King, received his PhD from Harvard University and stayed there for a year as a research fellow before going to the Graduate School of Public Health at the University of Pittsburgh. He came back to Cambridge in 1959 as Associate Director of Research at the University Health Services and as Lecturer on Clinical Psychology in the Department of Social Relations. He is the author with Otto von Mering of *Remotivating the Mental Patient* (1957) and with Funkenstein and Margaret Drolette of *Mastery of Stress* (1957). The reviewer, Julia Brown, is now well known to many CP readers. (CP, May 1963, 8, 192). She remains a sociologist, remains an Assistant Professor of Psychiatry (Medical Sociology) at the University of Oregon Medical School, and still has an educational history including experience at Radcliffe, at Wisconsin



sin, at Yale and, more warmly, at the University of Florida.

**I**N *Perceptions of Illness and Medical Practice*, the author surveys the contributions of the behavioral sciences to the understanding of problems concerning sickness and its treatment. He addresses his text to members of the health professions, claiming that acquaintance with the theoretical ideas and empirical findings of psychology, sociology and anthropology will widen the perspective of these individuals and increase their effectiveness in treating the ill.

With this objective in mind, King proceeds to delineate concepts he considers useful for interpreting human behavior. Among these concepts are psychogenic need, ego adaptive mechanism, attitude, value, and perception. Perception, defined broadly as "cognitive processes concerning . . . stimuli," is selected as the organizing concept, the major intervening variable between psychological, physiological, and socio-cultural stimuli, on the one hand, and individual behavior, on the other. The concept is said to have special relevance for medical practice since it is only through perception that psychosocial variables allegedly influence the behavior of the patient, the behavior of the practitioner, and the interaction between the two.

Next Dr. King turns to the task of applying this conceptual scheme to three major areas. First, he broaches the subject of the interpretation of disease, listing primitive medicine, folk medicine and scientific medicine as three major systems of beliefs and attitudes about illness. Then through a review of the results of several surveys of health knowledge and health attitudes, he attempts to demonstrate the varying significance of these "alternative ways of perceiving illness" for differing segments of the American population.

The second area investigated by the author is "the people who treat disease." The roles, values, attributes, rights and duties of physicians, nurses and medical social workers are all detailed. The interrelationships among these roles are examined, conflicts are analyzed, and

implications for patient care are outlined.

Last, King deals with "the place where disease is treated." Here he considers the social organization and culture of the hospital, the role of the patient, and the effect of all these on the treatment and recovery of the patient.

**I**N covering this wide variety of topics, the author has for the most part managed to strike a judicious balance, presenting neither so many details as to overwhelm the reader, nor so few as to result in an impoverished text. Nevertheless, some subjects have been over-emphasized and a few have been entirely neglected. Twenty pages have been devoted to primitive beliefs about the etiology of disease, but no space has been allotted to epidemiological studies concerned with the differential incidence and prevalence of illness among differing populations. The entire area of the effects of illness on the social structure and the culture of the wider society has been neglected. And finally, there is no mention of the role in the health field of the behavioral scientist. Not even the psychologist is mentioned, a surprising omission in view of the fact that King is himself a psychologist.

All in all, however, the coverage of the book is adequate. The organization of the selected materials is satisfactory, and the literary style tends to maintain reader interest. In the opinion of this reviewer, King has succeeded admirably in his aim of providing a text for social science courses designed for students of medicine, nursing, public health and medical social work. The book should also serve as a satisfactory text for courses in medical sociology. For students in such courses supplementary readings might be selected from the numerous references supplied at the end of each chapter.

**I**N recommending this book as a text, two cautionary comments are in order. First the author tends to be somewhat uncritical of the studies he summarizes. The impression he conveys of a state of excellence in the behavioral sciences may be flattering but not wholly

warranted. For instance, he writes: "Many behavioral scientists will say that control of collection of data and statistical analysis of the results are often the ideal rather than the practice, but it is said with less conviction each year."

Second, while the author's perceptual-cognitive approach may prove intellectually appealing to some, it may also alienate readers of a more behavioristic persuasion. The latter may argue that the behavior of patients and health practitioners may be adequately and perhaps more parsimoniously explained without recourse to the concept of perception. In either case the reader's orientation to these issues will doubtless affect the degree to which he favors the book.

Notwithstanding such reservations, the present reviewer believes that King has fulfilled a very valuable function in summarizing succinctly the present state of behavioral knowledge in the field of medicine. His book deserves a wide audience not only among members of the health professions, but among the increasing numbers of anthropologists, sociologists and psychologists working in medical settings.

## From Principal to Practice

Edward Arthur Townsend and Paul J. Burke

*Learning for Teachers*. New York: Macmillan, 1962. Pp. v + 313. \$5.50.

Reviewed by F. J. KING

Both authors, Edward Arthur Townsend and Paul J. Burke, may be found hard at work in the School of Education, City College of the City University of New York, the former as an Associate and the latter as an Assistant Professor. The reviewer, F. J. King, works equally hard. He is Assistant Professor in the



*Department of Educational Research and Testing at Florida State University at Tallahassee. He took his 1960 PhD in the Department of Educational Psychology at the University of Texas.*

IN THE foreword to *Learning for Teachers* the authors state plainly that the book is ". . . intended to bridge the gap between the general principles of psychology and the courses in methods of teaching." If one can assume from this statement that its use as a text should be confined primarily to classes of students who have already had introductory psychology courses and who seriously intend to become teachers, then one can say that the author's purpose is at least relatively well accomplished. The topics covered are to be found in most undergraduate educational psychology texts, but many of the other books also contain sections dealing with such subjects as human development, personality, etc. Introductory material discusses the school situation as it is related to personal adjustment of teachers and learners, conflict between conformity and individualism, and "the individual in a democratic society." In many cases technical material, set in type distinctive from the rest of the book, has been included. This device presumably will allow the casual reader to go along rapidly at one level at the same time providing a more penetrating discussion for the serious student.

While those chapters dealing with learning in its more academic aspects are considerably less complex than chapters covering the same topics in texts for students preparing for careers in psychology, most of the exposition is adequate for the use indicated by the authors. It is made clear that wholesale application of the results of learning experiments to any and all classroom situations is impossible. However, there is no indication that the authors have gone to the other extreme of saying that although learning studies may be of interest to the classroom teacher, their findings are unrelated to real schools and real children. The majority of references to experimental work are concerned with cases where human subjects were used; thus, the problem of

whether results of animal studies may be applied to human learning is almost entirely avoided. In the few instances where animal experiments are discussed, for instance the extinction of learned behavior in the pigeon, care is taken to point out differences between the laboratory situation and conditions as they would exist in the classroom.

Toward the end of the book, there is a chapter that deals with communication as it pertains to learning and that contains a number of concepts of importance to teachers. In this chapter is found the following statement: "Although important, writing ability is not

as critical as oral-aural skills and reading, because more persons communicate through listening, speaking and reading than through writing (p. 243). Unfortunately, the book itself leads one to disagree with this proposition. Much of the writing is fuzzy and imprecise enough to necessitate frequent 'back-tracking' in order to gain understanding. A certain lack of precision can be tolerated in most spoken communication because of the opportunity afforded for questioning and clarification, but in a book intended to be used as a teaching aid, a lucid presentation is of utmost importance.

## Insured Peace of Mind

Helen H. Avnet (Ed.)

*Psychiatric Insurance: Financing Short Term Ambulatory Treatment.* New York: Group Health Insurance, Inc., 1962.

Reviewed by HERBERT DÖRKEN

*The author, Helen H. Avnet, is a medical economist who started her professional activities in this field around 1940. Her other published works have been on the subject of medical care insurance. As Research Director of Group Health Insurance, she is now engaged in several projects, one of which is an attempt at a follow-up survey of Psychiatric Project patients. Herbert Dörken, the reviewer, was trained as a clinical psychologist at the University of Montreal and now serves as Deputy Director, Liaison and Prevention Services, of the California State Department of Mental Hygiene. While he was consultant to Canada's Department of National Health and Welfare, he began to concern himself with the potential and value of widely available insurance coverage for mental disorders. This interest was carried with him through a period in Minnesota, where he served as Director of Community Mental Health Services and on to California where he continues to work toward extension of insurance coverage for mental disorder as a major catalyst to the development of com-*

*munity and private alternatives to state hospital care.*

THERE are signs in the literature of a developing public and professional interest in insurance coverage for mental disorder, increases in coverage and favorable experience. The November, 1962 review by the Joint Information Service (NAMH-APA) covers many reports and serves as an excellent reference source. This year's annual review by the Health Insurance Institute (NYC) adds further data on the coverage and experience of many companies. Further detail is contained in the Transactions, Society of Actuaries (1953, 1957 and 1961). The present insurance study however, is unique as an *a priori* attempt to assess and plan for the usage of psychiatric services, to assure their availability, to analyze the resulting claim experience by many patient characteristics and to determine the cost of reasonable but limited benefits.

Co-sponsored by the American Psychiatric Association and the National Association for Mental Health, this



study was financed by a grant from the National Institute of Mental Health. Coverage was offered at no extra premium to a sample of GHI membership, 30,000 subscribers and their dependents, a total of 76,000 persons. The study was undertaken to gain experience with a stable population utilizing a well-established health insurance plan.

Professional interest was high and participating psychiatrists agreed to the project fee. Despite considerable promotion and coverage from the first visit, psychiatric claims were filed by less than 1½% of those covered (1,077) during the 30 months of the project, and consequently, service was sought from less than half the psychiatrists. Utilization rates decreased after the initial backlog of demand was satisfied. The new case experience was then but 5 per 1,000 eligibles annually. Since usage fell far short of assumed need, some public resistance may be inferred. Over 400 persons advised of actual eligibility for benefits never became patients.

Coverage involved co-insurance, the fee (benefit: co-insurance) varying with the service such as: 45 minute office psychotherapy, \$20 (75:25%), limit \$225; that is, 15 visits; hospitalization to 30 days at up to \$25 per diem (60:40%); etc. Marital status, education and occupation were major variables; the unmarried, the college graduate and the professional being relatively high users.

Main referral sources were physicians—(38%) and self—(35%), but the majority of psychiatrists—(70%) stated they were not reporting to the patient's family physician; both referral and reporting reflecting less than desirable medical collaboration. Only 27% of the patients sought treatment within six months of the onset of symptoms while 62% of project cases had no prior experience with psychiatric treatment. Neurosis accounted for 42% of the cases, psychosis 20%, personality disorder 14%, transient situational disorder 11%, and all others 13%. The sole criterion for acceptance was any condition treated by a psychiatrist.

**W**HETHER by type of service or demographic characteristic, the findings were clear that psychiatric patients used

substantially more medical-surgical service than non-psychiatric patients (176%) and this extended even to members of their family. For four out of five patients, individual office psychotherapy was the only form of treatment they received under the project (many of these had drug therapy at personal expense). But 6% of project cases were hospitalized; 77% of them having no prior such hospitalization. They stayed an average of 22.8 days, 39% up to the 30-day limit allowed. Despite special arrangements made for day care and night care these services were never used. Electroshock was given to 7% of the patients, 7% had psychological testing (mostly younger patients) and but 2% had group therapy. Of the office limit cases (15 visits), it was noteworthy that 2/3 continued treatment privately without project aid.

The average cost per terminated case was \$186. Benefits were not renewable; if so, a margin of 50% might have been added to second year costs. A rough conversion to the cost per subscriber for annually renewable services at project limits yields an encouraging figure of 36¢ per month (allowing 25% for operating and administrative costs). The main conclusion of the study then, that "short term, ambulatory, psychiatric treatments are insurable," is certainly warranted. Such findings call for action—the provision of coverage for mental disorder in all health insurance policies.

**P**SYCHOLOGISTS will take an interest in certain aspects of this study. Testing done by certified psychologists on the referral of a participating psychiatrist was covered up to \$45. Most of the testing was performed on younger patients, 26% of these under 13, but none beyond the age of 55. Of all 746 terminated cases, there was less than a 10% positive response by psychiatrists to the question of whether treatment by other than a psychiatrist would help—treatment by a psychologist was suggested in only two instances. This is probably a reflection of their professional confidence and isolation, mixed with resistance to the value of treatment by others and a

striving to maintain professional control in the face of developing competition.

The insurance facts reported are encouraging but professional practices prompt questions. Thus, quite apart from the data on claim experience and the practicality of coverage I cannot help but wonder about the future. We look to an extension of community and private alternatives to state mental hospital services. But specifically what services, and by whom? It is not credible when these patients had such diverse backgrounds, occupations, training, interests, and living conditions that 96% of them should require individual psychotherapy, (for 80% it was the only form of treatment). Is it the professionals who are preoccupied and impaired in their functioning?

Despite arrangements for day care services and current professional reports favoring such facilities, they were never used. When so many personality problems are evident in interpersonal relations, why did only 2% of these patients receive group therapy? The psychiatrist seldom thought that another profession could be of further help. Can they really be unaware of the significant contributions daily being made by the clergy, social workers, educators, psychologists, and physicians? With the present concern for continuity of care and the need for psychiatry to be in the main stream of medicine, the infrequent reporting (23%) to the patient's family physician suggests the hiatus will continue. It also seems wanting in the professional courtesy traditional in medical circles.

For patients seen to the project limit of 15 interviews, additional individual psychotherapy was held desirable for 90%, including 59% of "recovered" patients! Questioned on the extension of the project limit to 25 visits, 52% considered it would not be helpful. If the incidence of mental disorder is anything approaching what we claim, such logic and methods are guarantees of social impracticality.

Thus, this project, while a milestone in the study of psychiatric insurance, points to many very real problems. Not all of them lie in the realms of finances and patients.



# Strange Faces of Intellect

E. Philip Trapp and Philip Himmelstein

*Readings on the Exceptional Child: Research and Theory.* New York; Appleton-Century-Crofts, 1962. Pp. v + 674. \$7.00.

Reviewed by I. IGNACY GOLDBERG

The first editor here, E. Philip Trapp, received his PhD from Ohio State University under George Kelly and did his clinical internship with David Shakow at the Neuropsychiatric Institute in Chicago. In 1951 he joined the staff at the University of Arkansas, and has stayed there ever since. The second editor, Philip Himmelstein, also now at the University of Arkansas, received his undergraduate training and MA at NYU but improved at the very least his geographical status by going to the University of Texas for his PhD. The reviewer, I. Ignacy Goldberg, was born in Poland during World War I and came to America three decades later, via several European countries, the Middle East and South Africa. After receiving an EdD from the Department of Special Education at Teachers College, Columbia, he spent three years on the staff of Muscatatuck State School for the mentally retarded in Indiana and one year as educational consultant for the National Association for Retarded Children. In 1957 he returned to Teachers College where he is now a Professor in the Department of Special Education, deeply involved in the preparation of teachers, administrators and researchers in special education.

THE TERM 'exceptional child' has been generally accepted to mean either: 1) the child who has a physical handicap, such as a crippling condition, deafness, or blindness; 2) the child who deviates mentally, whether he is very bright or very dull or mentally retarded; or 3) the child who is maladjusted or emotionally disturbed.

In this book the term 'exceptional child' refers to one who deviates from the average or normal child in *cognitive, psychomotor or affective abilities* to such extent that he requires special educational services and treatment in order to develop to his maximum capacity. To those who view with alarm the increasing effort toward special education for handicapped children and youth, this publication should bring some comfort.

This collection of experimental studies with exceptional children may stimulate constructive thinking on the part of those who have a concern about this matter. Trapp and Himmelstein have shown excellent judgment in the selection of 48 contributions by 58 authors. Although only a relatively small number of the contributors might be known even to the sophisticated reader of *CP*, psychologists, special educators, social workers, and physicians should welcome this volume, for it effectively synthesizes the heretofore scattered literature in the extensive and complicated field of research and theory about exceptional children.

THE ORGANIZATION of this book is straightforward and follows Guilford's model of the three dimensions of human abilities. There is a section on exceptional intellectual processes, one on exceptional sensory processes, and one on exceptional emotional processes. The material is timely: 73 percent of the sources were published in the 1950's; 19 per cent in the 1960's. The remainder was published between 1940 and 1948 and represents the classics which deserve their space. One of the

original contributions, that by Iscoe, has very significant implications for psychologists and educators, since it proposes a functional educational classification of exceptional children.

Various disciplines may view the mentally retarded from different angles. Medical people might be more interested in looking at mental retardation from an etiological standpoint; they might use classifications which attempt to attribute the condition to a variety of causes, such as, infections, metabolic disorders, psychiatric disorders, or sensory dysfunctions. The psychologist might be interested in classification based on the behavior of the individual, his ability to learn, or most often, on his 'measured' intelligence. The educator's interest might be in the degree of educability of an individual, or in determining whether he is manageable or not manageable in a classroom situation. Heber, in his article specially prepared for this volume, attempts to establish common denominators for functional classifications which could be comfortably applicable to all disciplines.

Although the *CP* editor's instructions say explicitly that one should refrain from referring to single articles in a book of readings, this reviewer cannot help his professional bias and must single out the original contribution of Eugene E. Doll dealing with the historical survey of management of mental retardation in the United States. Most texts trace the beginnings of interest in education of the mentally retarded to Victor, the wild boy of Aveyron, and usually leave it at this by stating that Itard became discouraged with the supposed lack of progress of his pupil and discontinued his efforts. In his review, Doll skillfully and in a scholarly manner traces the development of philosophies and facilities for the treatment and education of mentally retarded individuals from the pre-Christian era to the present. He emphasizes that the so-called special education for exceptional children as developed in the U.S. has its own firm philosophical, psychological, and sociological foundations.

Trapp and Himmelstein devote the greater part of their book to mental retardation. One could accuse them of



imbalance in distribution of the areas of exceptionality. However, this is timely in view of the present tremendous impetus in this country in treating, combating, and preventing mental retardation.

ACCORDING to the editors, the purpose of this volume is to assist the student in an introductory course on the exceptional child to "be better equipped to pursue more confidently his own research interests, and interpret more intelligently the core of knowledge and development in the field." This reviewer believes that an introductory course should fulfill at least two functions. It should enable students in the course to examine their own attitudes towards those who are exceptional and it should orient them to the facilities and provisions for education and treatment of exceptional individuals. In view of these considerations, this reviewer believes that this book would better meet the needs and interests of more advanced students in psychology or in education.

Any collection of readings is usually open to the charge that the 'wrong' articles were included and that the 'classics' were excluded, or that topics near to the hearts of editors were given undue emphasis to the neglect of topics of interest to the potential reader. Trapp and Himelstein have not erred in either regard. They have performed the rare feat of balancing previously published studies, original contributions and condensations of several unpublished doctoral dissertations.

The format of the volume as well as the length and quality of the articles force the reviewer to the conclusion that we have here an outstanding book of readings for students interested in psychological, sociological and educational problems of the exceptional child.



*It is thus certain that neither my judgment nor my will dictated my answer, and that it was the automatic consequence of my embarrassment.*

—J. J. ROUSSEAU



## Rats, Rabbits and Fur Coats

H. J. Eysenck (Ed.)

*Behaviour Therapy and the Neuroses*. New York: Pergamon Press, 1960.  
Pp. xi + 479. \$10.00

Reviewed by ROSALIND DYMOND CARTWRIGHT

*H. J. Eysenck, who edits the present volume, needs by now no introduction to any of CP's readers who concern themselves either with everything CP prints or primarily with those books dealing with the functioning of the whole human organism. This is the seventh of his books to concern CP in the almost eight years of its existence. The reviewer, Rosalind Dymond Cartwright, was Associate Professor and Chairman of Psychology in the Department of Psychiatry at the University of Chicago College of Medicine but has recently moved to the University of Illinois College of Medicine. She took her PhD from Cornell University and taught there, at Mt. Holyoke College, at the University of Chicago, and at the University of Colorado before taking up her present position. For the past twelve years she has been doing studies in the area of the validity of psychotherapy and served as co-editor, with Carl Rogers, of Psychotherapy and Personality Change, a book regarded by many—but perhaps not by Eysenck—as a landmark presentation on the effects of psychotherapy.*

IN 1952 Dr. Eysenck seriously questioned the validity of psychotherapy as a treatment method for the psychoneuroses. After comparing the recovery rates for those who were treated by psychotherapy and those who were not, he concluded that there was as yet, no proof that psychotherapy had any special effectiveness. This position stirred many to action: many took directly to the pen to answer the chal-

lenge with counter-challenges of the argument on the grounds of its logic, others took to the lab in hopes of supplying the evidence that Eysenck felt was so sorely lacking.

During this past ten year period, the research literature on the effects and effectiveness of psychotherapy has grown enormously. We can now state much more precisely the probabilities of a successful psychotherapy experience for a given type of patient in treatment with a given type of therapist. In terms of recovery rates then, the picture could be greatly improved in the light of our superior knowledge of how to select appropriate patients. However, in terms of our understanding of the processes involved in bringing about therapeutic change, we are still floundering in a state of semantic and substantive confusion. It is highly probable that there is more commonality in the underlying processes addressed by the different theoretic positions than is apparent in the differences in terminology used to describe them. Eysenck's position in this book is that the reason psychotherapy has not been more successful in the past is that it has been encumbered by an unscientific theory full of unnecessary postulates, like the 'unconscious,' which led to techniques which were only successful by accident. The book is organized around the principal that neurotic patterns of behavior are unadaptive but learned and as such their acquisition must have followed the laws of learning and so must their extinction.

The book is made up of a rather loose collection of papers, some theoretical, some case studies; most of the



papers have appeared previously, and all stress the importance of following learning theory principals in order to change bad emotional habits. Here we read again Watson and Raynor's account of how by strict Pavlovian conditioning they induced in poor little Albert a phobia for rats, rabbits and fur coats. And again we see Mary Cover Jones' account of how she eliminated such fears in Peter. Here, too, is Shoben's excellent logical analysis of psychotherapy as a problem in learning theory. Wolpe's paper describing his therapy method called reciprocal inhibition, Lehrner's restatement of Duntlap's negative practise technique and several papers espousing aversion therapy. The bulk of the book is made up of reports of the use of some one of these techniques with one or more patients suffering from some behavior problem such as enuresis, alcoholism, writer's cramp, or stuttering, but also some on the more classical psychoneuroses.

WHAT is the evidence that behavior therapy meets Dr. Eysenck's own criteria of an effective treatment method for the psychoneuroses? Does behavior therapy in fact have a higher recovery rate than verbal psychotherapy? There are no comparative studies of those who were treated with behavior therapy and those who remained untreated and only two brief mentions of comparisons between behavior therapy and psychoanalytic psychotherapy. Eysenck himself points out this deficiency. He rests his case not on data but on the logic that if you isolate the appropriate elements of neurotic behavior and tackle them directly according to the well established principles of learning theory, a much surer and quicker change in the offensive behavior should result. If the common condition of neurotic patients is anxiety which is being reduced but not sufficiently controlled by maladaptive symptoms, then reinstating the anxiety producing stimuli and coupling these with pleasant rather than unpleasant responses should reduce the anxiety and thus the need for the symptomatic behavior. In this manner the way is paved for extinction. To the problem of symptom substitution, Eysenck offers

this answer: there is no neurosis underlying the symptom, but merely the symptom itself. Get rid of the symptom and you have eliminated the neurosis. If a new symptom occurs following behavior therapy it is likely to be a newly learned pattern and not part of the old successfully treated one (p. 9). (Cold comfort to the patient.)

WHERE does this approach fit in our current state of knowledge and research in the field? As of now behavior therapy is less well validated than the more traditional forms of psychotherapy but stands a good chance of reaching at least their level of effectiveness. That it should do at least as well is a personal bet based on the observation that it shares in common with other forms of therapy certain elements which have been established to be directly relevant to therapy outcome. We know that therapy is most likely to be successful when the patient is highly motivated to change and meets with a therapist who is deeply committed to helping him change. Another common feature of successful therapy seems to involve the structuring of the therapy role relations by the therapist. The patient is commonly defined as one presently having difficulties because of inefficient but involuntary control operations. Part of this definition involves the notion that he can be helped by breaking through these old control patterns in the presence of the therapist. This is to be accomplished by following the particular therapeutic procedures. These may be any one of a number of things: deliberately making the mistake (as in the negative practise effect method) to get this behavior under voluntary control, hearing his own denied feelings reflected back (a la Rogers), getting in touch with his primary process material (through a combination of LSD and mescaline, or sensory deprivation), reexperiencing the anxiety producing experiences (through Wolpe's desensitization procedure or through free association). In each case, if the patient's neurotic controls can be circumvented in the safety of the therapist's presence, the need for the inefficient controls is said to vanish. The person not only feels that he is in touch

with more of himself, but also that he is better able to exercise reasoned voluntary controls in the future.

Shoben states it directly and succinctly:

"Catharsis will be effective when it involves a) the symbolic reinstatement of the repressed cues for anxiety, b) within the context of a warm, permissive, non-judgmental social relationship. Under these conditions the situation is ripe for counter-conditioning to take place whereby the patient learns to react non-anxiously to the original stimuli" (p. 73).

These, then, are elements common to therapies of many different kinds. Does behavior therapy offer something unique in addition to these common properties which makes its further exploration valuable? It is the personal opinion of this reviewer that the answer to this question is "yes." We know that there are many people with emotional difficulties for whom verbal psychotherapy is not likely to be successful, for example those who are low in ego strength as measured by various psychological tests. Perhaps some of the methods collected in this book which depend a good deal less on one's ability to handle emotional material verbally, could extend the range of those who can be helped. The state of our present ability to help those burdened with emotional difficulties is such that we should close no doors prematurely because the method differs with our theoretic bias. But neither should we adopt every passing fad and fancy. It is our scientific responsibility to treat each new technique as one to be evaluated systematically and adopted or discarded on the basis of its proof.



*And therefore the ill and unfit choice of words wonderfully obstructs the understanding. Nor do the definitions or explanations wherewith in somethings learned men are wont to guard and defend themselves, by any means set the matter right. But words plainly force and overrule the understanding, and throw all into confusion, and lead men away into numberless empty controversies and idle fancies.*

—SIR FRANCIS BACON





# A Longitudinal View of Youth

Robert J. Havighurst, Paul Hoover Bowman, Gordon P. Liddle, Charles V. Matthews and James V. Pierce

*Growing up in River City.* New York: Wiley, 1962. Pp. vii + 189. \$4.50.

Reviewed by PAUL H. MUSSEN

Robert Havighurst, the senior author here, is the well known Professor of Education at the University of Chicago. His collaborators in this book have been associated with him either at Chicago or on the Quincy Youth Project or both. Bowman and Liddle are now on the staff of the University of Chicago. Charles Matthews is at the University of Southern Illinois. James V. Pierce, a Stanford MA, was a consultant on the Quincy Youth Project. The reviewer, Paul Mussen, took his PhD at Yale and then taught at the University of Wisconsin and at Ohio State before moving to the University of California at Berkeley in 1955, where he is now Professor of Psychology and Vice-Chairman of the Department. In recent years he has been quite peripatetic, moving from Berkeley to the Harvard Florence Project in Italy, and to an international meeting on cognitive development in Oslo. In the spring of 1964 he will be a visiting professor at Harvard and will also direct a research project on the impact of industrialization on Puerto Rican families. He is author, with Conger and Kagan, of *Child Development and Personality*, the second edition of which has just been published.

THIS is a brief, well written and lucid report of a longitudinal study of youth in a small midwestern city. The subjects, 247 boys and 240 girls, were studied from 1951 when they were eleven years old and in the sixth grade, until 1960 when they were at-

tending college, working, or, in the case of more than half the girls, married and starting families. Social class data were collected, subjects and their parents were interviewed, and batteries of various kinds of tests (intelligence, personality, sociometric, rating scales) were administered.

The purpose of the research, according to the Preface by Havighurst, was "to show in what way the social backgrounds and personal characteristics of boys and girls determine how well they perform their tasks of growing up in this kind of community. For that reason there is a theme of prediction running through the study. The question is asked and answered, again and again: 'Knowing these facts about boys and girls in the sixth grade, or in the ninth grade, what can we predict about their performance in the later years of high school, or after they graduate from high school?' Then a further question is asked: 'Under what conditions do boys and girls grow into competent young adults, and under what conditions do they become incompetent young adults?'" (p. viii).

The ultimate evaluation of this research should probably await a later publication, since these subjects "are a control group for an action-research program designed to help a community do a better job of rearing its children" (p. vii). In this volume, the investigators examine the relationships between these subjects' progress toward becoming mature, responsible citizens (as reflected in their school achievement or dropout; delinquency; church

participation; attending college; early marriage; social mobility) and familial socioeconomic status, aspects of personality, and social-emotional adjustment under "natural" (nonexperimental) conditions. These findings will presumably provide a standard or control against which to assess the effects of some intervention and experimental manipulation by social scientists.

BUT the present volume, considered in light of stated objectives, will disappoint the professionally trained social scientist or educator. It will inevitably be compared with many significant earlier studies conducted under the auspices of The University of Chicago's Committee on Human Development—viz., the work of Davis and Havighurst, Hollingshead's *Elmstown's Youth* and Havighurst and Taba's *Adolescent Character and Personality*. These have most effectively demonstrated the processes by which social structural variables affect value orientations, child rearing practices, and consequently, personality structure and behavior.

To this reviewer, the research reported here seems to be of a lesser caliber: less thoughtfully conceived; more naive and superficial in design, in instrumentation and in analysis. Systematic theory apparently played no role in either its formulation or execution, or in the interpretation of the findings; no attempt was made to integrate conceptually the psychological, educational, and sociological data.

Consequently, the book makes only a limited contribution to knowledge. No new facts or unusual insights are presented. The findings are essentially descriptive, and, at most, confirm already well-established, generally accepted propositions. To illustrate, their study of factors related to boys' college attendance led the authors to conclude that "in our society there is an innerrelated set of circumstances and forces that tends to send some to college while the absence of these things keep others out of college. These are: intellectual ability; socioeconomic status; examples set by parents and other 'significant persons'; drive for achievement; and pleasure in study and do-



ing things of an academic nature" (p. 110). From the investigation of adult competence, "It is a fair conclusion, but an oversimple one, that the children who are endowed with advantages make the most of them during adolescence and become the most competent young adults" (p. 158).

This lack of depth, typical of most of the conclusions, seems particularly disturbing in view of numerous missed opportunities to investigate more subtle relationships among central personality characteristics (underlying motivations, self concepts, and interpersonal attitudes), social structure variables, and subsequent academic and social adjustment. For example, the general adjustment score of the California Psychological Inventory (CPI), administered in high school, was related to college attendance, adult competence and work adjustment, but there are no reports of relationships between other characteristics that can be validly assessed by this test—e.g., capacity for status, socialization, self-control, need to achieve independently, flexibility, and intellectual efficiency—and these criteria. Interview and projective test data were "scored" only in terms of gross, general categories, although more subtle and complex analyses might have yielded more stimulating and meaningful results.

In spite of these shortcomings, the book may serve important didactic purposes for intelligent laymen and beginning students of social science or education. For them it may be thought-provoking and may help to dispel preconceived simple notions of single variable explanations of phenomena like school achievement and social adjustment. The findings of the study, together with the rich illustrative case material, provide impressive and clear evidence that many complicated antecedents are involved.



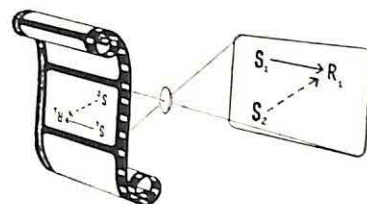
For a man to achieve all that is demanded of him he must regard himself as greater than he is.

—GOETHE



# INSTRUCTIONAL MEDIA

Edited by A. A. Lumsdaine



## Good Grief, Still More P.I.P.'s

PROGRAMED INSTRUCTION PUBLICATIONS, of which two batches were briefly summarized here not so long ago by Instructional Media's Editor (*CP*, June 1962, 7, 242-244, and Sept. 1962, 7, 354-355), continue to pour forth. Most of such publications, including those previously summarized and the further group dealt with below, are about programing although not in themselves programed. Two exceptions to this are the Center for Programed Instruction's "Programed Primer" by Susan Markle *et al.* and the anonymous T.M.I. Grolier program, both about programing and also in programed format, reviewed by Della-Piana (*CP*, Feb. 1962, 7, 64-68). A third example is Markle's further "Primer" volume (see *CP*'s annotation, June 1962, 7, 243).

During the past two and a half years *IM* has also presented (aside from reviews of films and books about TV and other instructional media) more substantial reviews, at varying lengths, of some dozen or so auto-instructional programs and a like number of books, of one sort or another, about programing. The growing number of programs that are of direct or indirect interest to psychologists, and research reports of studies in which programs are either the object or vehicle of experimentation by psychologists, pose a problem for this Department's review coverage which will be further commented on in a subsequent issue.

In the present issue we attempt to augment and update the June and September '62 set of summary reviews, by commenting briefly on some recent

books (of indisputable status as such by virtue of hard covers), and on some other book-like publications of less certain status (see below). Some of these, as in the case of last year's June and September summaries, are hopefully scheduled for fuller review in forthcoming issues of *CP*; the present summaries will help, meanwhile, to combat the time-lag problem on review production. To help fill another gap in information dissemination, we also present below a few notes on some other sources of periodical information about programed-learning developments.

### Books (Hard Covers)

"PRIMERS" in programed form (see above) and otherwise (Cram's, Foltz's: *CP*, June 1962, 7, 243-44; Epstein & Epstein: *CP*, Feb. 1962, 7, 64-68) have been followed by several edited compendia such as those by Margulies & Eigen, (*CP*, Sept. 1963, 8, 361f), and by Smith & Moore (see *CP*, Sept. 1962, 7, 354), which have partially duplicated and partially supplemented some of the content of the source book edited in 1960 by Lumsdaine and Glaser for the Department of Audiovisual Instruction (DAVI) of the National Education Association (reviewed by Holland, *CP*, Sept. 1961, 7, 319-321). Such books of readings have now been complemented by several more systematic textbooks, or quasi text-like books, on "programed instruction" and/or "programed learning," which have been put forth by several publishers during the past year or so, rushing in to fill a demand for use in university courses, workshops, and other instructional projects (we can't



say instructional "programs," since that word is pre-empted for a different and narrower, if still ill-defined, denotation). The text for Holt, Rinehart and Winston by Edward Green (see Markle's review, *CP*, Jan. 1963, 8, 26-28), for SRA by John Hughes (see Wittrock's review, *CP*, April 1963, 8, 172-173), and by Deterline (Prentice-Hall's paperback: see *CP*, Sept. 1962, 7, 354) have recently been joined by entries into the text field by Wiley and McGraw-Hill, on which, pending fuller review in subsequent issues of *CP*, brief comment is provided below.

JEROME P. LYSAGHT and CLARENCE M. WILLIAMS. *A Guide to Programmed Instruction*. New York: John Wiley & Sons, 1963. Pp. ix + 180. \$3.95. (Library of Congress No. 63-11440).

The initial chapter of this highly readable text purports to present programming's "origins and fundamentals" (from the *Meno* to Skinnerian and neo-Skinnerian variants of reinforcement theory, and on what is represented as its application in classroom practice). The book then gets practical with three chapters, totaling 41 pages, that deal with "selecting a unit to be programmed," "assumptions about learners" and appropriate definition of objectives. It proceeds to the down-to-earth business of selecting a programming paradigm (Ch. 4), constructing a program (Ch. 5), and editing and review (Ch. 6). Having disposed of these difficult matters in some 64 pages, it winds up with a chapter on "evaluation" (14 pp.), one on "application and implications" (20 pp.), and a useful 8-page selected bibliography of books, articles, source materials, journals, technical papers, popular articles and films.

EDWARD B. FRY. *Teaching Machines and Programmed Instruction*. New York: McGraw-Hill, 1963. Pp. xi + 244. \$5.95. (Library of Congress No. 62-17024).

Dedicated with a double obeisance to Pressey and to Skinner, this attractively presented book is, following a brief introductory chapter, sub-divided into two

main parts: (I) Basic Considerations in Programmed Instruction (Chapters 2-7), and (II) Construction and Evaluation of Programs (Chapters 8-14). Chapter 2, on Teaching Machines, has the most profuse (since Foltz: see above) and, perhaps, best pictures yet of teaching machines and their use (as well as photos of their above-named inventors). Chapters 3 and 4, in Part I, and 8-13, in Part II, treat principles, techniques and procedures for program construction in an aggregate of some 97 pages, comment on which we shall reserve to a future review. In between are some chapters on objectives and applications and, at the end, a 13-page chapter on Judging Program Quality. Appendices present Fry's version of a classification of programmed-learning variables, and reprint the T.M.I.-Grolier program on programmed learning, as well as a 28-frame excerpt from the Holland-Skinner program and a 12-page Crowder "intrinsic" sequence.

#### Paperbacks: Monographs and Others

INFORMATION ABOUT PROGRAMS. The U.S.O.E.-published 383-page sampler, *Programs '62*, prepared by the Center for Programed Instruction (C.P.I.) (see *CP*, Sept. 1962, 7, 354-55) is being supplemented by a similar, follow-up volume, *Programs '63*, of which *IM* has seen partial page proof as of press date, and on which it will provide further comment in due course. Meanwhile, in addition to other less comprehensive listings, a new cumulative bibliography of programs has appeared: *Programmed Learning: A Bibliography of Programs and Presentation Devices*, edited by Carl Hendershot at Delta College, University Center, Michigan (Pp. vi + 40, 8½ x 11, multilithed; \$2.00). Unlike *Programs '62*, this contains no illustrative program samples, but several hundred published and reportedly about-to-be-published programs are listed, first by subject matter, then by publisher, with such summary descriptive data as author, proposed level (through college), number of frames, cost, availability and presentation mode (machine or book). The second edition is updated with supplementary listings as of February

1963, and further quarterly supplements (April, June, etc.) can be ordered. (The June supplement is fifty cents.)

Stolurow's comprehensive (173 page), 1961 bargain-priced (\$.65) U.S.O.E.-published monograph on teaching machines—(see *CP*, June 1962, 7, 243)—dealt at some length with past and then-present machines, some with research, and some (though perhaps less comprehensively) with programming techniques. A newer, briefer, and highly readable treatise in pocket-size (5½" x 7¾" x ¾") format, published this past winter by the Ford Foundation's Fund for the Advancement of Education, is Wilbur Schramm's *Programmed Instruction: Today and Tomorrow* (Library of Congress No. 62-22138). Written with Schramm's typical conciseness and lucidity, it virtually ignores (or dismisses) machines, also summarizes a wide range of research (see below) and looks mainly toward the future, taking as its theme the thesis that "programed instruction is, in the best sense of the word, a truly revolutionary device . . . but the potential of freeing schools from old bondage and outward theories . . . is, so far, largely unrealized." Schramm stresses the need for more effort at making programs on "the growing edge of the art rather than the safe and conservative commercial 'center'." The research results reviewed include some of the 1951-57 experiments on programmed film sequences included in Lumsdaine's 1961 volume, *Student Response in Programmed Instruction* (which Schramm calls "a confluence of another current of research with the Skinner current . . . on the same group of variables—cueing, fading, confirmation of results, size of step, overt vs. covert response"), as well as more recent experiments on verbal learning programs. (For information of *CP*'s readers, some of whom have tried unsuccessfully to obtain *Student Response in Programmed Instruction* from the National Academy of Sciences, which published it, this 550-page collection of research reports can be obtained as publication No. AD 281936 from the U. S. Department of Commerce's Office of Technical Services for \$5.00. Because its editorship precludes



*IM*'s editor's arranging for its review, it will be reviewed, subsequently, elsewhere in *CP*.)

An additional paperback not previously noted in these columns but containing several papers dealing with programmed instruction (by Alvin C. Eurich, Donald A. Cook, E. Z. Rothkopf, A. A. Lumsdaine and John Blyth) is *Improving the Efficiency and Quality of Learning* (Pp. 175; \$2.50). This is a report on the 26th Educational Conference, sponsored by the Educational Records Bureau and the American Council on Education (available from A.C.E., 1785 Massachusetts Ave., N.W., Washington, D.C.; Library of Congress No. 34-9880).

The *Audio Visual Communication Review* (published by DAVI: see *CP*, June 1962, 7, 243) has over the past few years issued several special issues or supplements of interest to psychologists concerned with programming and research aspects of instructional media. The earliest of these, *Graphic Communication and the Crisis in Education* (120 pages), by Neal Miller and collaborators, appeared back in 1957 as Vol. 5, No. 3, of *AVCR* and, blurring further the already hazy distinctions among books, monographs and journal supplements, also came out concurrently in hard covers as a separate, as well as an extra-thick issue of the journal. The first of the more recent *AVCR* supplements, previously noted in *IM*, was *Teaching Machines: an Annotated Bibliography*, by E. B. Fry, G. L. Bryan and J. W. Rigney, which was *AVCR Supplement 1* (1950, Vol. 8, No. 2: see *CP*'s note in the June 1960 issue, pp. 206-207). This bibliography was, however, shortly outdated by that of the 1960 Lumsdaine-Glaser volume. A further *AVCR Supplement* was *Teaching Machine Programs and Programing Techniques*, by Rigney and Fry (*AVCR Supplement 3*, May-June 1961: see Angell's review, *CP*, Oct. 1962, 7, 384-385), which included illustrative program excerpts that are partially, but not wholly, replaced by C.P.I.'s more recent and comprehensive sampling of commercially available programs in *Programs '62*. During the past two years these *AVCR* supplements have been augmented by three

more, noted below, which have not as yet been reviewed in *IM* but which are of interest to psychologists concerned with programmed instructional media (All of these are obtainable from the National Education Association for the prices indicated.)

LEARNING THEORY AND AV UTILIZATION. (W. C. Meierhenry, Ed.) *AVCR Supplement 4*, Vol. 9, No. 5, September-October 1961 (actually appeared in 1962). Pp. 87. \$1.50.

Among many evidences of the recently growing "togetherness" of the professional educator and the experimental psychologist (e.g., cf. Glaser's comments, *CP*, January 1960, 5, 24-28), this supplement foreshadows the forthcoming 64th Yearbook of the National Society for the Study of Education the first yearbook concerned primarily with learning theory since 1942). Of the 87 pages in this *AVCR Supplement*, 72 comprise a series of five papers, by Abraham Luchins (Gestalt-psychology implications for audio-visual learning), Howard Kendler (S-R psychology), Robert Glaser, Frederick McDonald and Leo Postman—as yet still rather strange, but welcome, names in the professional instructional-media circles. These papers are introduced by Meierhenry's foreword and capped off with a commentary and summary by James Deese.

PERCEPTION THEORY AND AV EDUCATION. (Kenneth Norberg, Ed.) *AVCR Supplement 5*, September-October 1962, Vol. 10, No. 5, Pp. 108. \$2.00.

This is another collection of papers, again largely authored by psychologists invading (by invitation) the audio-visual education field. The four papers that follow Norberg's introduction include one of 12 pages on "What Do the Eyes Contribute?" (Rudolf Arnheim), a generously illustrated 33-page job on "The Psychophysics of Pictorial Perception" (Julian Hochberg), another 33 pages on "Perception, Communication, and Educational Research: A Transactional View" (Hans Toch and Malcolm MacLean, Jr.), and a final 31 pages on "Human Communication," by Franklin Fearing (completed, fol-

lowing his death, by Mrs. Fearing and Evelyn Hooker).

THE CHANGING ROLE OF THE AUDIO-VISUAL PROCESS IN EDUCATION: A DEFINITION AND A GLOSSARY OF RELATED TERMS. (D.P. Ely, Ed.) *AVCR Supplement 6*, January-February 1963, Vol. 11, No. 1, Pp. 148. \$3.50.

This is primarily a lexicographical work, prepared by Henry A. Bern, James Q. Knowlton, Samuel Cohen, Susan M. Markle and Sidney Eboch, who comprise the Commission on Definition and Terminology for DAVI's Technological Development Project. A foreword by James D. Finn (TDP Director) and an Editor's Introduction precede Part I, which comprises four other brief introductory chapters (pp. 6-28) on needs for definition, historical perspectives, defining the field of audio-visual communication (inclusively, as "that branch of educational theory and practice concerned primarily with the design and use of messages which control the learning process"), and functions of AV personnel. Part II contains the major lexicon of 33 pages in which the authors define nearly 500 terms (with numerous cross references) in such closely to tenuously interrelated fields as communication, learning, information theory, audio reproduction, computers, electronic learning, laboratories, projected visual media, TV broadcasting equipment, and photography. The resulting potpourri is analyzed out into ten appendix sections which relist the same definitions in more homogeneous topical listings of 4 to 12 pages each. Because of the range it spans, this useful lexical effort is necessarily less consistently and tightly knit together than William Verplank's 1957 *Glossary of Some Terms Used in the Objective Science of Behavior*, with which it occasionally conflicts slightly and which might well be consulted in connection with some of the non-hardware aspects of the *AVCR* glossary.

#### News of Other Journals

IN addition to numerous journals that carry occasional to frequent papers on programmed instruction in addition to other content (e.g., *Psy-*



chological Reports, Audiovisual Instruction—cf. *CP*, June 1962, 7, 142-43), several journals have started out with programmed instruction as their sole province. Two fairly early entries into this field, Inrad's quasi-monthly *A.I.D.* (Auto-instructional Devices) and C.P.I.'s bimonthly *Programmed Instruction* have persevered since the spring of 1961. *P.I.* continues, while *A.I.D.* has recently merged, after going it alone for some two dozen issues, with the relatively parvenu monthly *NSPI Newsletter*, which started in mid-1962 upon the founding, in San Antonio, of the National Society for Programmed Instruction. These have carried general news of the field, bibliographic listings,

summaries of research, and miscellany; *P.I.* has furnished the most in the way of helpful hints and examples for the program writer.

The impressive but apparently abortive start made in 1961 by Thomas F. Gilbert's *Journal of Mathetics* represents another casualty of a field growing with unruly zeal; and Rheem-Califone's *Automated Teaching Bulletin*, the first journal to appear (in 1959), similarly lapsed, in 1961, after only a few issues had appeared, to be superseded as a vehicle for research papers after a year-plus hiatus by C.P.I.'s new, quarterly *Journal of Programed Instruction*.

—A.A.L.

poor; psycho-analytic books are also reviewed by psycho-analysts, and non- (or anti-) psychoanalytic books are also reviewed by psycho-analysts (including under this terms all those of a "dynamic" persuasion.) This has certainly been my own unfortunate experience, and a casual tally of relevant reviews of books on personality (in its widest aspect) in *CP* has tended to bear it out. I don't know the answer to this problem, but I would like to plead for some measure of impartiality and justice for those of us who do not share current charismatic and chiliastic beliefs.

H. J. EYSENCK

*The Maudsley Hospital*

## ON THE OTHER HAND



### BUTCHERED FOR A FREUDIAN HOLIDAY

Carmi Harari's review of Trasler's "The Explanation of Criminality" (*CP*, June, 1963, 8, 236) illustrates a weakness in *CP*'s policy which I think deserves mention. Trasler attempted to construct an explanation of criminal behaviour in terms of modern learning theory, conditioning principles and laboratory findings. No doubt there is much to be criticized in his specific theories, but seen in its particular context the book strikes me as interesting, constructive, and possibly even important. Harari fails to mention the main contentions put forward in the book, and reserves his criticism almost entirely for the rather unconstructive task of lambasting Trasler for his temerity of having refused to embrace the "dynamic" framework which he, Harari, obviously prefers. "Thus he dismisses the most fruitful leads of the past fifty or more years, leads that have resulted in vast gains in the understanding and modification of human behaviour." Harari is of course entitled to his view, but Trasler obviously is rather less impressed with these putative "fruitful leads," and seems rather to agree with the present writer in judging these "vast gains" to have been illusory. Is he not entitled to be judged by a reviewer

less obviously prejudiced against his whole approach? Is not the reader entitled to a review which tells him just what Trasler does in fact suggest, instead of giving him a catalogue of the things which he did not say, but which the reviewer would have liked him to have said? And should not criticism be clearly related to the author's stated views, instead of consisting of a simple enumeration of alleged facts ("deviant behaviour is at times paradoxically linked with the unconscious need for punishment in order to relieve anxiety, stress and guilt") which are at best hypotheses in search of verification, and at worst simple mythology?

The review would have less general importance were it not for the monopoly position of *CP* which makes it unlikely that psychologists will see the book reviewed anywhere else. In the good old days there would have been at least half a dozen reviews, with reviewers of all persuasions, and each side would have had its say; the monolithic structure now erected makes selection a toss-up, and may lead, in this case, to grave injustice. Indeed, one might go farther than this and say that even a toss-up would be less likely to result, as does the present system, in the perpetuation of one law for the rich, another for the

### WRONG SLANT ON FLIES

I am pleased with *CP*'s editorial policy permitting occasional reviews of fictional works provided they are of special interest to psychologists. *Lord of the Flies* qualifies on these grounds, but I for one was disappointed with the review it received (*CP*, June 1963, 8, 231-232). To put it bluntly, almost every sentence in Bennis's review is unexceptionable, yet very few of his statements are specifically relevant to Golding's book.

The reviewer started off unfortunately, it seems to me, by classifying his subject among children's classics of the *Huckleberry Finn* variety, and subsequently pondering the change in social climate which could have altered the genre beyond recognition. *Lord of the Flies* is not a children's classic. It neither portrays a society's idealized version of childhood (*Huckleberry Finn*), nor does it vitalize a social or historical myth for children (like *The Leather Stocking*, to which Bennis also refers), nor is it a fable, as the reviewer proposes. It is a book about man and society which uses children as a vehicle for the expression of wholly intellectual content. *Gulliver's Travels*, and *Alice in Wonderland* with a dash of *A High Wind Off of Jamaica* come to mind as related works.

Nowhere does the reviewer mention the fact that *Lord of the Flies* is an anthropological treatise. Each and every one of the evolving rules and regulations among the children on the deserted island, as well as the sequence of their evolution, is old hat to social scientists. From the creation of fire, through the transferable symbol of authority to the invention of evil spirits and the final emergence of thought—all this and more is to be found in *The Golden Bough*.



in Levy-Bruhl, and in related literature. The author's thesis clearly is (elucidated in his own notes at the end of the book) that primitivity in men and social institutions is a function of extreme and immediate threat to survival; that civilization, emerging as mastery over the forces of nature, brought in its wake mastery over destructive impulses from within (at the expense of ecstasy); and lastly, that atomic danger could reactivate primitive patterns of adaptation as it constitutes anew an immediate threat to survival. To psychologists the book does not offer a single new idea and the symbolism is heavy-handed indeed.

Why, then, has the book captured the imagination of readers, and especially of young people? This is a question to interest psychologists, and an answer to it might have been expected in *CP's* review. In part the answer may lie in the fact that the anthropological material is unfamiliar to the lay public. Mr. Golding writes exceedingly well and makes the ideas readily intelligible. More significantly, *Lord of the Flies* expresses in symbolic and general form a widely shared contemporary mood. It crystallizes some of the dark fears and the elements of inner conflict that are felt but seldom articulated by the vast majority of people in their effort to come to terms with the age of nuclear danger.

In short, one view of the matter is that Golding's book is so successful not because of originality (which it does not possess) nor because of outstanding literary value (which seldom makes for popularity in any case) but for opposite reasons. It deals with universals using facts and ideas that have withstood the test of time. It is anything but complex. It is topical. And finally, Mr. Golding is a writer of considerable talent.

SIBYLLE K. ESCALONA  
Albert Einstein College of Medicine

#### ON SPANKING

#### CP YES, CHILDREN NO

In "CP Speaks" in the June, 1963 issue of *CP*, you declare a resolve to operate the journal in 1964 with 1963's number of pages. So I enter a caveat on the use of *CP's* limited space for the description of an effective spanking quoted from "Management of Emotional Disorders" (*CP*, Mar. 1963, 8, 104) along with the reviewer's approval of

this as one of the author's "practical recommendations."

The description details how the spanking parent "can turn the child over one knee while his other leg clamps down the child's struggling legs," the while "spanking with an attitude of firm execution of justice, with perhaps a touch of justified indignation."

I am doubtful how "practical" is this recommendation and how appreciative can be the child of these nuances of parental attitudes, especially in view of his prone and clamped-down position!

The subtitle of the book is "A Manual for Physicians." From the review, it does not seem appropriate to have been allotted a page in *CP*. I have tried to follow your admonition that criticism for "On the Other Hand" be *ad verbum*, not *ad hominem*, even though it has cramped my style.

ESTHER K. ROSEN  
Philadelphia, Pennsylvania



## BOOKS RECEIVED

- AEBLI, HANS. *Über die geistige Entwicklung des Kindes*. Stuttgart, Germany: Ernst Klett Verlag, 1963. Pp. 124. 11.80 DM.
- ATKIN, I. *Aspects of psychotherapy*. London: E. & S. Livingstone Ltd. (U.S. Distributors Williams & Wilkins), 1962. Pp. v + 103. \$3.00.
- BARRY, RUTH & BEVERLY WOLF. *Modern issues in guidance-personnel work*. New York: Teachers College, Columbia University, 1957. (Second printing, with additional material, 1963). Pp. vii + 256. \$1.95.
- BOTTENBERG, ROBERT A. & JOE H. WARD, JR. *Applied multiple linear regression*. Lackland Air Force Base, Texas: 6570th Personnel Research Laboratory, 1963. Pp. iii + 139.
- BRECKENRIDGE, MARIAN E. & MARGARET NESBITT MURPHY. *Growth and development of the young child*. Seventh edition. Philadelphia: Saunders, 1963. Pp. iii + 564. \$6.75.
- BROOM, LEONARD & PHILIP SELZNICK. *Sociology: a text with adapted readings*. Third edition. New York: Harper & Row, 1963. Pp. v + 746.
- BROWN, ROBERT. *Explanation in social science*. Chicago, Ill.: Aldine, 1963. Pp. v + 198. \$5.00.
- CONDRAU, GION. *Angst und Schuld als Grundprobleme der Psychotherapie*. Bern, Switzerland: Verlag Hans Huber, 1962. Pp. 189. Fr./DM 26.—
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But like one walking alone in the dark,  
I resolved to proceed so slowly and with  
such circumspection, that if I did not  
advance far, I would at least guard  
against falling.

—RENÉ DESCARTES





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*Contemporary Psychology: A Journal of Reviews* is published monthly. The subscription per year is \$10.00, foreign \$10.50, single number \$1.00.

Published by the American Psychological Association at 2210 W. Dallas, Houston, Texas and 1333 Sixteenth St., N.W., Washington 6, D. C.

Second-class postage paid at Houston, Texas and at additional mailing offices. Accepted for mailing at special postage rate stated in section 132.122, Postal Manual.

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# Contemporary Psychology

A JOURNAL OF REVIEWS

VOLUME VIII

November, 1963

NUMBER 11

## Speaking in General

Joseph H. Greenberg (Ed.)

*Universals of Language.* Cambridge, Mass: The MIT Press, 1963. Pp. x + 269. \$6.00.

Reviewed by GEORGE A. MILLER

Joseph H. Greenberg, Editor of this volume, is an anthropologist with a 1940 PhD from Northwestern University. He spent the years 1958-59 at the Center for Advanced Study in the Behavioral Sciences and is now Professor of Anthropology at Stanford University. The reviewer, George A. Miller, is Professor of Psychology at Harvard's Center for Cognitive Studies. Since the early 1940's, when he came to Harvard from the University of Alabama, he's been the best thing that many people in Cambridge, Massachusetts, know about the latter institution. He took his degree at Harvard then moved to MIT and then back to Harvard. All during this vacillation, which included some movements of slightly larger amplitude—one to Princeton for a year at the Center for Advanced Study and another to Stanford for a year at the Center for Advanced Study in the Behavioral Sciences—he has maintained his interest in the field of psycholinguistics. As a matter of fact, he was interested in the field before its present name was invented for it. Among his most recent productions is the book *Psychology: The Science of Mental Life* (1963. CP, Oct. 1963, 8, 369). He is spending the current academic year as a distinguished visitor at Oxford University.

“UNDERLYING the endless and fascinating idiosyncrasies of the world's languages there are uniformities of universal scope. Amidst infinite diversity, all languages are, as it were, cut from the same pattern.”

These are the opening sentences of the “Memorandum Concerning Language Universals,” drafted by Joseph H. Greenberg, Charles Osgood, and James Jenkins—the memorandum that set the stage for a Conference on Language Universals. That conference (held April 13-15, 1961, at Gould House, Dobbs Ferry, New York, under the sponsorship of the Linguistics and Psychology Committee of the Social Science Research Council) assembled some thirty linguists, anthropologists, and psychologists in order to explore what is or could be known about language in general and to consider the possibility of creating “something of the order of cross-cultural files for a large sample of languages.” One result of the Conference was eleven papers, here collected and published under the editorship of Professor Greenberg. Of these eleven, eight are invited papers prepared in advance for the Conference, and three (by Roman Jakobson, Joseph Casagrande, and Charles Osgood) are final summaries from the

viewpoints of each of the disciplines represented.

Psychologists with an active interest in language will be especially grateful for this collection. Although it contains only one contribution from a psychologist (the summary statement by Osgood), although some of the essays are unnecessarily difficult for a lay reader (the language of linguists is often highly technical), although the volume itself is hardly handsome (it is photo-offset from typewritten copy with unrectified margins, has numerous typographical errors and no index), it is still a book that every psycholinguist should have on his shelf. The information it contains is of great potential value to psychology, but it is information that would be difficult for most psychologists to dig out of the technical literature of linguistics and to evaluate for themselves.

PRESUMABLY, psychologists aspire to the discovery of general laws. Many of our most cherished descriptive principles are assumed, either explicitly or implicitly, to hold true not only across different cultures, but even across different species. In the area of social psychology, however, and especially where linguistic phenomena become involved, it is all too easy to fall into an ethnocentric fallacy—to explain too much because we confuse our own social conventions with universal laws of human nature. Fortunately, our anthropological friends have not been reticent about correcting such errors; by now enough psychological theories have been punctured by ethnological shrapnel to make us all properly self-conscious in our generalizations.

One of the best publicized of these anthropological injunctions—at least among psycholinguists—is the Whorf-



Sapir hypothesis of linguistic relativity. The notion that a person's whole conception of the world he lives in is, to use Whorf's phrase, "controlled by inexorable laws of pattern . . . , the unperceived intricate systemizations of his own language," was often taken to mean that men in different cultures, speaking different languages, must organize and think about their worlds in very different ways. Psychological principles established in one linguistic community, therefore, cannot be automatically generalized beyond that community. Teamwork between ethnology and psychology is obviously necessary.

As this teamwork has developed, however, it has become increasingly clear that there is not as much difference between men in different societies as we had been led to expect. This is not to say that the Whorf-Sapir hypothesis has been discredited—a Scottish verdict of "not proven" is as much as we can presently say on that score—but rather that, even if the hypothesis were true, the conceptual differences between cultures could scarcely be *greater* than the differences between their languages. And these differences, apparently, are not as enormous as Whorf had feared. The fact seems to be that there are uniformities of surprising scope among the many diverse languages that have developed in different societies, uniformities that are probably of greater psychological significance than the differences we had expected to find.

This conclusion should not come as a complete surprise. In a sense, the very existence of educational programs that prepare linguists to cope with any new language they might encounter in their field work argues that a human language is not a completely plastic product of whimsical imagination, but that there are features common to all languages that an anthropologist can learn to expect and to recognize. Every human society has a language; every human language has both a lexicon and a syntax; the principles of organization underlying every lexicon and syntax are remarkably similar from one language to the next. If it is true, as Whorf and Sapir believed, that our language shapes our psychology, then it is at least equally true that our psychology shapes

our language. The strongest evidence supporting this converse proposition comes from the existence of language universals; it is this evidence that the present volume attempts to survey and evaluate.

WHAT are some of these language universals? They come in all sizes and varieties. There are phonological universals: all languages are spoken and can be analyzed into sequential patterns of phonemes; about a dozen phonological features serve to mediate all the phonemic distinctions that any language requires. (Charles Ferguson and Sol Saporta contributed chapters on these matters.) There are grammatical universals: all languages can be analyzed into sequential patterns of morphemes, where the morphemes fall into a few basic categories ("parts of speech") and the sequential patterns that are admissible ("sentences") can be generated by a finite set of grammatical rules. All languages have subject-predicate constructions, and in declarative sentences with a nominal subject and object, the dominant order is almost always one in which the subject precedes the object. (Greenberg lists 45 universals of this latter sort, based on a survey of 30 different languages.) There are semantic universals: all languages are capable of expressing the basic logical operations, all deal in some way with the concepts of number and time, all have systems of pronouns at least rich enough to distinguish "I," "you," and "him," all share the property of combining signs to produce new but intelligible semantic compounds, etc. (Uriel Weinreich and Stephan Ullman explore these semantic universals.) There is even (according to H. M. Hoenigswald and to Warren Cowgill) some hope of finding universal generalizations that can be made about changes that occur in the history of a language.

Many of these language universals—and the more important ones in particular—seem, by their very universality, to be almost trivial, as if no successful communication system could be conceived that would not share them. This failure to appreciate how very arbitrary and improbable are our human solu-

tions of the communication problem is probably what leads some of us to expect that, if intelligent life has developed anywhere else in the universe, we will be able to communicate with it. One way to appreciate how peculiarly human language really is, is to consider some of the alternative communication systems that have evolved in other species; Charles Hockett has looked at human language in this wider biological context, and his impressions form part of the clear and helpful essay that is the first chapter of this book.

ONE WONDERS, of course, how complete this catalogue of universals really is. There is such variety and richness among the language universals listed here—each painstakingly documented by studies in many different languages—that one hesitates to suggest that anything might be missing. But consider, for example, the remarkable fact that conversational partners alternate between talking and listening. This reciprocity, which I assume is universal, is not a necessary consequence of any auditory or physiological inability to speak and hear simultaneously; one voice is a poor masking noise for another. There is no a priori reason why two people who have questions to ask one another could not question simultaneously and answer simultaneously. Nevertheless, we alternate. There are several interesting lines of speculation one might pursue from such a language universal—if it is indeed universal. Perhaps there is some limit imposed by the agility and attention, perhaps some critical component of the speech apparatus must be actively involved in the process of understanding speech, etc.

But, complete or not, the book makes a good beginning. It is difficult to read it and not acquire an impression that these universals fit into a pattern, that someday they will be derivable from a few basic propositions central to all the others. When that day comes, when we are ready to describe the underlying principles that explain why all human languages are so much alike in so many ways, we will undoubtedly understand human psychology a great deal better than we do today.



# Consonance on Dissonance

Jack W. Brehm and Arthur R. Cohen

*Explorations in Cognitive Dissonance.* New York: John Wiley and Sons, Inc., 1962. Pp. xv + 334. \$7.95.

Reviewed by EZRA STOTLAND

*The authors, Jack W. Brehm, with a PhD from Minnesota, and the late Arthur R. Cohen, with a PhD from Michigan, are both known for their contributions to the Yale studies of attitude change. Brehm is currently Associate Professor of Psychology at Duke University and Cohen was, at the time of his recent death, Associate Professor of Psychology at NYU. The reviewer, Ezra Stotland, is a Michigan kind of experimental social psychologist, with an interest in cognitive factors in interpersonal processes. He stayed on at Michigan for several years after receiving his PhD, but finally moved to the University of Washington where he is currently Associate Professor in the Department of Psychology.*

THIS BOOK is one of the rare instances in social psychology in which researchers have constructively and creatively built upon previous theoretical work. Within Festinger's theory of cognitive dissonance, Brehm and Cohen have elaborated the concept of commitment as a necessary condition for the arousal of dissonance. An individual's commitment to a given line of action or attitudinal position has the effect of making certain of his cognitions relatively unchangeable, so that other cognitions, dissonant from the unchangeable ones, are made more consonant with them by the individual. The authors report in great detail and with great methodological sophistication the astonishing variety of studies that show the very uncommon-sensical results of dissonance reduction. The focus of these studies range from attitude change, to

self-report of thirst and amount of drinking, to desegregation, to defense mechanisms, to social influence, to effort exerted in working etc., etc., thus covering far more than social psychology. Much of this research is the authors' own—much previously unpublished—but the impact of Festinger's original publication is well indicated by the number of different researchers cited. And the results generally hang together. It is to the authors' credit that they present the research in the context of an on-going research movement. The book is rich in suggestions for further research to show more "unexpected" behavior, and for work in new problem areas.

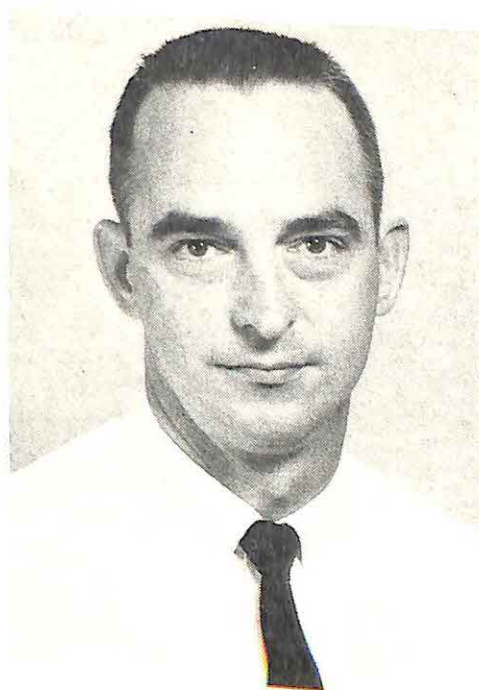
THE MAJOR and most stimulating original contribution of Brehm and Cohen is the use of the concept of perceived choice or volition. They present evidence that dissonance reduction occurs mainly when the person has made a choice he perceives as free, of his own volition. They also present evidence on the determinants of this perception of volition; for example, the negative relationship between this perception and amount of reward offered for taking a given line of action. Unfortunately, they do not develop this concept of perception of volition fully, nor relate it to other relevant work. For example, the importance of the perception of volition is central to Heider's theorizing (1958), quite separately from his ideas about balance. They do not relate volition to the research they cite by Deutsch, Krauss, and Rosenau.

On the other hand, the authors are overzealous to show that their theory



ARTHUR R. COHEN

contradicts other theories and approaches. For example, they attempt to show that the concept of attitude is theoretically dispensable, since it is interpretable as a handy summary of a series of related but discrete cognitions. Yet, Osgood, Suci & Tannenbaum's repeated finding (1957) of an evaluative factor on their semantic differential suggests that some such separate "cognition" does exist psychologically. Incorporation of attitude as a recurrent type of cognition would greatly simplify Brehm and Cohen's theorizing, since much of the



JACK W. BREHM



research concerns attitude. Perhaps their disinclination to accept the concept of attitude stems from the lack of a formal definition of a cognition or cognitive element.

Their difference with conflict theory hinges on the occurrence of a decision or choice, conflict theories being assumed to be relevant prior to the decision, dissonance theory, afterwards. (Factors leading up to decisions are not treated extensively, which leaves one with the uneasy feeling that something *important for dissonance theory might be missing in the antecedents.*) However, it is not clear that all conflict theories do imply some such event as a decision; and the authors indirectly admit this by sometimes describing the scope of conflict theories as ranging up to the attainment of the goal—which is often several steps beyond the decision. The difference between some, more behavioristic, conflict theories and dissonance theory may stem from the very useful distinction the authors make at another point between motivation and cognitions about motivations. Conflict theory may concern motivational states, dissonance theory concerning cognitions about the states.

That the problem of the relationship between dissonance theory and other theories (in addition to those mentioned) can be resolved in terms of a theoretical definition of the scope of each theory is suggested by the authors' use of the concept of commitment to draw the boundary between the scope of dissonance theory and of what they term a rational, judgmental theory of attitude change, as exemplified by many of the Yale studies. This boundary is theoretically integrated with dissonance theory, which is not the case for conflict theory, since the authors attempt to show how dissonance and conflict theories might explain each other's data.

AT A MORE methodological level, the usual, pointless comment can be made that some of the results seem easier to explain in terms of other theories, despite the authors' careful attempts to justify their interpretations. Nevertheless, two problems run through a number of the studies. One, which the

authors examine at length, is that of subject loss, since some subjects have to be coaxed into making a commitment. The reader would have a clearer idea of the degree of seriousness of this problem if the number of subjects lost were routinely, rather than occasionally, reported. The other problem concerns studies of the change of extreme attitudes. The greater importance to the person of his extreme attitudes is used to explain the paradoxical finding that people with extreme attitudes change their attitudes more toward a discrepant cognition than those with less extreme attitudes. It is simpler to explain this result in terms of differential possibilities of change along the attitude dimension. This issue could be resolved by an experimental manipulation by which extremity of attitude and importance are independently varied.

With regard to dissonance itself, aside from its reduction, the authors go beyond Festinger in suggesting that only if different cognitions are related to the same goal or motivational state can they be relevant to each other and thus subject to dissonance reduction. However, there is still no way except the intuitive for judging the dissonance between cognitions. Furthermore, although the authors occasionally use the term "tension" to refer to the state generating dissonance reduction, it is not clear whether tension is intended to have empirical correlates in addition to dissonance reduction. If it is, some of the research on the effects on anxiety of unexpected stimuli would become relevant.

The tenor of this review should convince the reader of the stimulating value of the book.

## Complexity in Design and Theory

Marshall R. Jones (Ed). With W. Edgar Vinacke, Jack W. Brehm, George A. Kelly, Seymour Epstein, Albert Bandura and Morton Deutsch

*Nebraska Symposium on Motivation 1962*. Lincoln, Neb.: University of Nebraska Press, 1962. Pp. vii + 330. \$3.25 (paper). \$4.25 (cloth).

Reviewed by IRWIN G. SARASON

Marshall Jones, the editor here, functions in what is by now a thoroughly familiar role. This is the 10th volume to come from the *Nebraska Symposium on Motivation* and the 10th to be edited by him. He left the University of Nebraska in September, 1963, to become Chairman of the Department and Professor of Psychology at the University of Miami, Coral Gables, Florida. The reviewer, Irwin G. Sarason, is Associate Professor of Psychology at the University of Washington. He received his degree from Indiana in 1955, went East for an internship in the VA Hospital at West Haven, then went thoroughly West in 1956 to join the staff at the University of Washington,

where he stayed. His research specialty concerns anxiety as it relates to learning and to intellectual ability. Also, and more recently, he has been involved in the study of psychological research as it seems to vary with and relate to the personality of the experimenter and to the social aspects of the experimental situation. He is the editor of *Contemporary Research on Personality*, a book soon to be reviewed in CP.

IF THERE is one thing that most strikingly characterizes this tenth volume in the Nebraska series it is the generally high level of methodological ingenuity shown by the contributors who report on their experimental programs. If there



is a second striking characteristic, it is that the theories presented in relation to these experiments tend to be complicated and, often, overly elaborate.

Consider Seymour Epstein's paper in which he presents a series of well-conceived studies dealing with what he calls simple drives (e.g. hunger, sleep), conflict-producing drives (e.g. sex, hostility), and acute conflict-producing events (e.g. parachuting). Among Epstein's major interests are the effects of drive and conflict and individual differences on fantasy. He interprets his experimental findings as indicating an interaction between autistic drive-oriented and reality-oriented processes. Epstein has also uncovered evidence that should contribute significantly to the experimental investigation of projective techniques and, more generally, of ambiguous stimuli. For example, employing stimuli which vary in terms of their relevance to the drives being studied, his data suggest that inhibition of hostility is less apt to be correlated with conscious conflict than is inhibition of sex. Also, in studies of the behavior of parachutists and of sleep-deprived individuals, Epstein found convincing evidence that there is far from a one-to-one correlation between approach and avoidance tendencies in real life and those inferred from the TAT.

Unfortunately, before getting to the main feature (Epstein's experiments) one is forced to plow through a B picture whose complicated plot attempts to synthesize psychoanalytic and learning theory concepts (with a predominance of the latter), and that requires the reader (1) to view drive simultaneously as a concept referring to a directed force and as a class of behaviors, and (2) to understand inner and outer drive-producing cues as combining additively with each other and, then, interacting multiplicatively with non-cue aspects of the organisms' inner state.

One of Epstein's conclusions merits at least a comment. After several years of work with drives, he feels that investigators should spend more time studying the effects of intense drives and less time on motivational conditions of low intensity (e.g. ego-involving instructions). In support of this conclusion

Epstein observed that "apparently, when drives are sufficiently intense, they reduce individuals to a common denominator" (p. 202). While the study of the effects of extreme conditions on behavior is obviously of value, this reviewer would still like to intersperse studies of starvation and parachute jumping with occasional studies of verbally-induced failure or stress. Let's not solve the problems of individual differences by eliminating them!

**J**UST AS EPSTEIN used hunger as an independent variable, so does Jack Brehm use it as a dependent variable. Brehm honestly admits that he is trying to pull off a *tour de force*. Briefly, his aim in the studies on which he reports, is to show that cognitive dissonance, his focal concept, doesn't influence just cognitive processes but physiological motives as well. True, in order to do this he must sneak in through the back door (at least, at first) the notion of cognitive components of hunger. Still, his research reflects very favorably on him as an experimenter and on cognitive dissonance as a heuristic formulation. Brehm's paper makes a strong argument for joint study of cognitive and non-cognitive aspects of physiological states.

As Brehm spoke of cognitive and non-cognitive aspects of motivation, so Edgar Vinacke, in a scholarly review of literature, suggests that situations be viewed in terms of intrinsic and extrinsic components of a motivational system. Extrinsic variables refer to environmental events; intrinsic variables are treated as inferred constructs. Vinacke's approach is to present an eye-opening treatment of the many independent variables (particularly organismic ones) which affect behavior in a wide variety of situations. One of Vinacke's conclusions is that as we increasingly recognize the relevance to behavior of variables such as sex, socio-economic status, etc., we also must develop more sophisticated and complex experimental designs. This reviewer would also conclude that as we delineate more and more relevant variables, we should concomitantly become increasingly cautious in our theorizing. This conclusion is, obviously, debatable. Vinacke, ap-

parently, doesn't go along with it because much of his discussion consists of a quite theoretically oriented discussion of a drive-modification approach to behavior.

**O**F THE SIX papers in this collection, the one which, in the eyes of this reviewer, had the best ratio of empirical evidence to theory was Albert Bandura's. Bandura's experimentation has thus far been primarily with children, but its implications are broad. He is interested in the social aspects of learning situations and is very dissatisfied both with theoretically restricted Skinnerian approaches to learning and Hullian based social learning theory. With respect especially to the latter it is somewhat embarrassing to see a social learning theory criticized for lack of social awareness. Bandura, however, makes a very strong case for the need for researchers in the area of learning to pay much more attention to what he calls the "social transmission of behavior." In his analysis of this transmission, Bandura directs our attention to (1) imitation as a significant part of many learning situations, (2) the characteristics of social reinforcers (e.g. the mother's personality), and (3) the social context of learning. While regarding imitation as an active rather than a passive process, Bandura wisely avoids over-involvement in the many speculations which have been offered concerning imitation and identification. For Bandura, these latter terms are synonymous.

Only brief comments will be offered with respect to the papers of George Kelly and Morton Deutsch. Kelly, misleadingly, begins his paper with references to his psychology of personal constructs. It soon develops, however, that his major contribution is an entertaining and enlightening travelogue of Europe. This reviewer must confess that his knowledge of personal construct theory was only slightly extended by Kelly's paper. But his understanding of life in the Soviet Union and in particular its educational system was significantly affected. Kelly's descriptions, his polished style, and his sage remarks make his paper a reader's delight.

Deutsch's paper is far from a reader's delight. His concern is with the precur-



sors of interpersonal cooperation and the psychological consequences of cooperation and competition. In discussing these concepts, Deutsch makes reference to two other concepts, trust and bargaining. Not being a social psychologist, this reviewer felt overwhelmed at times by this paper's proliferation of terms and concepts such as promotively and contritely interdependent, substitutability, the pathology of the vested interest, the pathology of the self-perpetuating leadership. Difficult as the Deutsch writing is, two things seem clear: (1) his efforts to explore experimentally his conceptual scheme are brave and promising, and (2) the implications of his work on cooperation and competition may have important implications for the analysis of social and international problems such as desegregation and nuclear disarmament.

OVER ALL, as was suggested above, the papers in this volume are stimulating, often controversial, and of high quality. One suggestion that might improve these symposia is to make them truly live up to that name. As they are now, each volume is a collection of papers, each paper being briefly commented upon by one or two other participants. More interchange among participants would be welcome, and, in particular, more interchange between contributors who are best equipped to take up the cudgels (e.g. social psychologist Brehm reviewing social psychologist Deutsch's paper). An additional suggestion is that, as an experiment, one symposium be directed at one important problem or concept with all of the participants actively engage in its study.

In Postman's review of the 1955 Nebraska Symposium, he raised the question: "Is the concept of motivation necessary?" (CP, Aug. 1956, 1, 230). In Jones' preface to the 1962 symposium he, apparently, answers this question in the negative. What implications this answer has for future volumes in this series cannot be predicted and is of only minor importance. What is important is that the series continues to be an invaluable collection for psychologists working in a variety of areas.

## Meliorative Milieux

John and Elaine Cumming

*Ego & Milieu: Theory and Practice of Environmental Therapy.* New York: Atherton Press, 1962, Pp. 292. \$7.50.

Reviewed by SAUL M. SIEGEL

John Cumming, a psychiatrist, is Director of the Syracuse Mental Health Research Unit of the New York Department of Mental Hygiene and also Clinical Assistant Professor of Psychiatry, Upstate Medical Center, University of New York. Elaine Cumming, his wife, is a sociologist and Associate Research Scientist in Sociology, Department of Mental Hygiene, State of New York. The two Cummings collaborated earlier on *Closed Ranks* (1957) and Elaine Cumming is co-author with William Henry of *Growing Old* (CP, Nov. 1962, 7, 414). Saul M. Siegel, the reviewer, received his PhD from the University of Buffalo in 1955 and shortly thereafter joined the Department of Psychology at Topeka State Hospital where he became Chief Psychologist in 1958. He has his diplomate in clinical psychology and is President-Elect of the Kansas Psychological Association. At the moment he is involved in two NIMH supported research projects, one concerned with psychology and architecture and the other with reevaluating the role of the psychiatric aide.



ELAINE AND JOHN CUMMING

pen' and such methods as lobotomy, insulin treatment, and more recently the tranquilizers and group therapy are seized upon and continued without systematic theoretical probing and explanation.

One of today's current treatment methods is milieu therapy, that is, the therapeutic structure of the total environment of the hospital setting with which the patient comes into contact. In *Ego and Milieu*, John and Elaine Cumming have attempted systematically to integrate the practices and principles of this kind of treatment with the theoretical framework of ego psychology. Milieu therapy stems from the realization that psychosis is in fact a devastating process and that therefore nothing less than a total treatment program can affect this process. Ego psychology has changed our view of the patient from the helpless, hopeless victim of id impulses to one who has the assets available to him to cope with his illness if the setting gives him the opportunity to use them and if there is a recognition and channelization of these assets by staff. In this book the Cummings have related these two subjects of cur-



rent thinking and have explored many of the practical implications of their convergence.

The volume begins with an excellent and well-communicated summary of ego theory: Hartmann's conflict-free portion of the ego, Federn's concept of ego feeling, and the current ideas of Bibring and of Freeman, Cameron and McGhie. The second major section deals with the structure of the milieu, including the physical structure, role relationships, the culture, and communications. The final section integrates the ideas of ego psychology with those on the structure of the milieu, and discusses implications for treatment.

ALTHOUGH not explicitly stated, the authors imply that many hospitals today are pathology oriented and hence focus treatment on the lowest common denominator of patient behavior. In response to this they suggest a hospital milieu that provides the patient with information, freedom of action and protection against the consequences of failure as he develops his repertory of behavior. With these as basic givens, they feel that it is possible to manipulate the patient's environment scientifically to produce growth in his personality by the presentation of a series of "graded crises under circumstances that maximize his (the patient's) chances of resolving them." This attitude toward crisis, with its assumption that problem solving by patients will lead to greater integration, is in marked contrast to usual hospital staffs' orientation of protection of the patient and the avoidance of problems. The context within which these experiences may take place includes all aspects of both the social and physical setting of the hospital. The patient's need for a clear and unambiguous environment necessarily requires a social structure with specific role definitions; it also requires an architectural setting that provides consistent cues.

In their discussion of architecture the Cummings have presented many fascinating research ideas. They discuss too the difficulties for the milieu inherent in charismatic authority in the hospital structure, and the importance of delegating authority to the personnel directly involved with the patient (usually

the psychiatric aide). They do not point out that these two variables are frequently two sides of the same coin, but they do discuss their relationship to the establishment of the therapeutic milieu.

IT IS A TRIBUTE to the authors that their generalizations are primarily made on the basis of their personal experience. This tribute, however, can also be seen as a criticism in that it does not note the pioneering work in this same area by groups scattered throughout the country, especially George Saslow's group at the University of Oregon Medical Center (which has dealt extensively with graded learning experiences), the program of the Fort Logan Mental Health Center in Denver, and Ellsworth's studies at the Rapid City, South Dakota, Veterans Administration Hospital. The authors also fail to note the contributions of the concept of supportive psychotherapy. This procedure accepts the patient where he is at the moment, but makes consistent demands upon him to function at higher and higher levels. There is a great deal in common between this concept and the philosophy of the authors.

Another problem is to be found in the Cummings' argument that specificity of staff role is necessary for adequate treatment of the mentally ill. While they make a rational case for this, there are many programs today that indicate the contrary—that a realistic and diffuse role rather than an arbitrarily specific one probably provides a better identifying model for a patient.

One other major deficiency in the book is lack of sufficient attention to the problem of change. Mental hospitals, like other complex organizations, are notoriously resistant to changing their mode of functioning. There are in this volume many fine ideas concerned with the direction that change should take, but there is little said about how to introduce such change or, once introduced, how to integrate these changes in the culture of the institution. Veblen has indicated that it requires a fire or funeral to produce fundamental changes. Presumably, there are alternatives to these measures, but what are they?

Despite these omissions, this volume reveals a fundamental respect for the confused and disoriented person and offers a well-considered program for his treatment.

## Cherchez l'Homme

Betty Friedan

*The Feminine Mystique.* New York: Norton, 1963. Pp. 410. \$5.95.

Reviewed by MARY ENGEL

The author, Betty Friedan, is a freelance writer for Harpers, Good Housekeeping, Ladies Home Journal, etc., who was once a brilliant undergraduate student of Kurt Koffka at Smith College. After her Smith graduation she did some graduate work at the University of California before leaving academia for matrimony. The reviewer, Mary Engel, with two recent reviews to her credit (CP, Apr. 1963, 8, 169; CP, May 1963, 8, 212f.), is known both to CP readers and to many psychologists who ought to be but aren't. She is Assistant Professor in Harvard's Graduate School of Education and her current research activity focuses her curiosity on the inference process in psychological diagnosis.

PERHAPS no other book has attracted more popular attention recently than that which contains Mrs. Friedan's cleverly phrased complaints about the feminine mystique. Praised by Pearl Buck, Ashley Montagu and Lillian Smith, reviewed in countless journals and newspapers, frequently "on order" in bookstores, this volume will no doubt affect the thinking of many. One may wonder: did not Betty Friedan do for women what Rousseau did for children? Discover them?

Indeed, the book lacks not in passionate expression and in repeated, forceful articulation of its main theme: "... the core of the problem for



women today is not sexual but a problem of identity . . . our culture does not permit women to . . . gratify their basic need to grow and fulfill their potentialities as human beings, a need which is not solely defined by their sexual role."

In documenting her message, the author ranges freely over the domains of art, literature, psychoanalysis, education and labor statistics. She places heavy blame for the American woman's sense of anomie at the doorsteps of teachers, "Freudians," and the followers of Margaret Mead. While for the most part Mrs. Friedan's observations are well documented, the selection of her references reveals her bias. For example, psychoanalysis is depicted as Freud's ". . . attempt to translate all psychological phenomena into sexual terms,"—an interpretation which ignores just about all of ego psychology's emphasis on the thought processes and on the varieties of adaptation.

THE reader may be offended when asked to consider some analogies, as, for example, when Mrs. Friedan declares: "I am convinced there is something about the housewife state itself that is dangerous . . . housewives . . . are in as much danger as the millions who walked to their own death in the concentration camps . . ." No matter how stifling or limiting of individual expression the life of suburbia, it ought not to be compared to Hitler's extermination camps! And even the most feminist reader might consider it an overstatement that educators discriminate against women as they segregate Negroes.

At times, the reader will be amused by the very feminine side of Mrs. Friedan, which comes to expression when she engages in succulent gossip about "a guilty woman college president—guilty personally of being college president." Whoever the unnamed and clearly unfortunate lady, she is thoroughly chastised by Mrs. Friedan for having introduced a course in the family, at a "famous woman's college."

But, offended or amused, the reader will no doubt learn from this book. He will learn about the pervasive discomfort of the women whom the author

has interviewed. He will be convinced that there is a problem, and that the role of women is changing. After all, this book testifies to the fact that preoccupation with the problem of the American woman has reached our shores. Over a year ago, an Italian author, Oriana Fallaci, presented the problem of femininity in America, in a much reprinted novel: *Penelope alla guerra* (Penelope in revolt).

Betty Friedan's book will teach the

reader that in the American mind the problem of femininity extends itself over the "home-career" or "where work?" axis, while in the European mind of Fallaci, the same problem is construed as a choice between "romantic" and "realistic" attitudes toward the opposite sex. About this the reader will not learn from Mrs. Friedan, because the entire laboriously compiled index to *The Feminine Mystique* lists not a single reference to "men."

## Au Sujet de la Mesure

J. M. Faverge, C. Flament, A. de Groot, L. Knops, M. Reuchlin and M. Yela

*Les Problèmes de la Mesure en Psychologie: Symposium de l'Association de Psychologie Scientifique de Langue Française.* Paris: Presses Universitaires de France, 1962. Pp. 185.

Maurice Reuchlin. Preface by Henri Piéron

*Les Méthodes Quantitatives en Psychologie.* Paris: Presses Universitaires de France, 1962. Pp. vii + 454. 20 NF.

Reviewed by JOHN B. CARROLL

The six authors of the first book are briefly identified in the review. Maurice Reuchlin, author of the second book, is presently Associate Director of L'École Pratique des Hautes Études, at the Sorbonne. He has written numerous articles on testing, factor analysis and applied psychology and has produced a book on the history of psychology. The reviewer, John B. Carroll, received his PhD from the University of Minnesota and taught at Mt. Holyoke College, Indiana University, and at the University of Chicago before moving to Harvard where he is now Professor and Director of the Laboratory for Research on Instruction, a part of the School of Education. He passed his foreign language requirements early in graduate school, and as a believer in maintaining language skills he accepted with relish the task of reviewing these books. He visited the Sorbonne recently and enjoyed several pleasant hours conversing with M.

Reuchlin. He is the author of *The Study of Language* (1953) and editor of *Whorf's Language, Thought and Reality* (1956).

THE APPEARANCE of two books on the subject of measurement within a short span of time from the principal scholarly publishing house in France would seem to signal an intense concentration of effort on this topic on the part of a number of contemporary European psychologists. From the standpoint of historical priority, first consideration ought to be given to the report of the 1961 Amsterdam symposium where psychologists (from a number of countries) who were able to communicate in French gathered to hear six major papers: Faverge (Brussels) on the fundamental logic of measurement, Reuchlin (Paris) on the meaning of measurement in psychology, Knops (Louvain) on measurement in psychophysics, deGroot



(Amsterdam) on general problems of psychometrics, Yela (Madrid) on the interpretation of factor analysis measures, and Flament (Sorbonne) on measurement in social psychology. More than a third of the book is devoted to transcriptions of the vigorous discussion which ensued. This portion of the book may, indeed, prove the most interesting, since it contains useful theoretical and empirical critiques of the positions and procedures espoused by the main speakers or by those whom they quote. For example, one discussant raises doubts about the universality of Stevens's power law of sensation and cites experimental evidence concerning it.

REUCHLIN's book is on the one hand an expansion at great length of the paper he gave at the Amsterdam symposium, and on the other, an extended treatise on the use of measurement in psychology. It invites comparison with a book of similar title published in the United States: Don Lewis's *Quantitative Methods in Psychology* (1960) (CP Dec. 1960, 5, 393). Major differences are immediately apparent; whereas Lewis's book is largely devoted to the exposition of mathematical methods for the empirical and rational analysis of experimental data in psychology, Reuchlin's is primarily devoted to what may be called the philosophy of measurement in psychology. The text is rarely broken by mathematical formulas or expressions; the concern is always with the logic or rationale of measurement. Indeed, at one point (p. 11) Reuchlin says that in order to treat the fundamentals of quantitative psychology, one must be at one and the same time psychologist, philosopher, and mathematician. He doubts that it is possible for anybody to attain this "triple culture," but insists that one must make his best efforts towards it nevertheless.

The scope of what Reuchlin attempts to cover is indeed very wide; besides consideration of the fundamental logic of measurement in whatever field it may be applied, there is treatment of measurement problems in both psychophysics (the measurement of sensation) and psychometrics (the measurement of mental capacities). The work of Lazarsfeld and Guttman on the measurement of atti-

tudes is also discussed, although it is not placed in any clear structuring of the whole field of measurement. Throughout, there is much reliance on work of American and British authors, including such names as Campbell, Stevens, Spearman, Thurstone, Coombs, Meehl, and Mosteller. Stevens's classification of scale types is accepted and elaborated with the help of some ideas from Coombs. The author appears to have completed his manuscript before Torgerson's treatise on scaling was available to him. It is easy to name major American works in psychometrics that he seems to have overlooked; for example, Horst's monograph, *The Prediction of Personal Adjustment* (1941).

In a chapter on the "things measured" (*les objets mesurés*), Reuchlin falls into the same linguistic trap that presents itself in English: the "thing measured" can be either an object as a member of a population of objects, or an attribute of a series of such objects. For the most part, however, he addresses himself to the specification of attributes, making reference to problems of defining the reliability of measurements, of identifying and interpreting the factors of factor analysis, and of selecting criteria for validation tests.

A large part of the book is devoted to the treatment of what are regarded as the two major functions of measurement: (1) the condensation or summarization of data, and (2) prediction. In discussing condensation, Reuchlin describes nearly the whole range of commonly used statistical methods; in connection with prediction, methods are classified under four headings: (a) prediction from "the nature of things," i. e., from attributes of classes; (b) prediction from empirical laws; (c) prediction based on rational constructions; and (d) prediction based on causal laws. What is most remarkable and unique in this treatment is the systematic coverage of measurements from such widely disparate contexts as psychophysics and vocational aptitude testing.

These books do not offer any major original contributions to the theory and practice of measurement in psychology. What novelty they have seems to reside in their presentation of detailed philosophical analyses of the foundations of

measurement and the application of this analysis in a variety of fields of psychometrics. They have values also as systematic, well-written treatises. For example, M. Yela's chapter in the Amsterdam Symposium volume on the interpretation of factor analysis results is in this reviewer's opinion one of the most sensible and straightforward statements he has seen on this subject. It is heartening, at any rate, to see the flowering of a European brand of quantitative psychology.

## Decisive Research

Orville G. Brim, Jr., David C. Glass, David E. Lavin & Norman Goodman

*Personality and Decision Processes: Studies in the Social Psychology of Thinking.* Stanford, Calif.: Stanford University Press, 1962. Pp. vii + 336. \$8.00.

Reviewed by HAROLD W. STEVENSON

The four authors of this volume, Orville G. Brim, Jr., David C. Glass, David E. Lavin, and Norman Goodman, are all sociologists, all have an interest in psychology, all are APA members. Brim did his graduate work at Yale and is now employed as a sociologist and as Assistant Secretary at the Russell Sage Foundation. Drs. Glass, Lavin and Goodman all received their PhDs at New York University and all are now teaching sociology. Glass at Ohio State University, Lavin at the University of Pennsylvania, Goodman at Queens College. Harold W. Stevenson, the reviewer, reports, but without expressing an attitude about it, that he is essentially the same individual he was when his biography last appeared in CP (CP, Nov. 1962, 7, 430). This assertion leads to the reasonable interpretation that he's still the very busy and equally effective Director of the Institute of Child Development at the University of Minnesota, that he is still involved in a wide assortment of research projects, a large percentage of



which will lead to significant publications, and that he's still unrepentant of the sin of leaving the University of Texas a few years ago to take up his present employment in Minneapolis.

THIS VOLUME, one of the Stanford Studies in Sociology, is an extensive empirical study of the personality correlates of the decision making process. The decisions that the authors deal with are related to child-rearing practices in the areas of masturbation, homework, obedience, and stealing. Approximately 200 parents, lower- and middle-class residents of New York City, volunteered to take part in the study. The parents were given tests and questionnaires, including a Decision Process Test, which yielded data on more than 50 variables. The goal of this enormous undertaking was to discover generalizations that might be used to describe the relation between decision making, personality, and social structure.

Trying to review all of this is much like trying to review a current issue of the *World Almanac*. There are so many data that even the authors chose to close the book with an epilogue, rather than with an attempt at a summary. In the 336 pages one can find an appendix of 108 tables, dozens of additional tables scattered throughout the text, and 34 pages of test materials and references. The book is scholarly and sophisticated, taxing but rewarding to read.

We can be glad that Brim's long-time interest in child rearing practices led to the choice of this as the focus for the study of decision making. As a consequence, the volume will interest child and personality psychologists, as well as sociologists and decision theorists. This is a significant volume, for it deals with little explored, but potentially very important, determinants of the ways adults make complex and difficult decisions. Although the introduction of such variables makes fantastically more difficult the currently popular goal of writing equations concerning decision making, it has the healthy effect of demonstrating that no discussion of decision making can be completely satisfactory until such variables are considered.

The Decision Process Test provides the core data. In this test parents were presented with four hypothetical problems dealing with children's behavior. Each problem was accompanied by six alternative parental actions. The parents were asked to write out at least one result which would occur for each of the alternative actions and to rate the results for desirability, probability of occurrence, and probable time of occurrence. The alternative actions were ranked for acceptability and the parent indicated the action that he would take. Scores were derived for each of the steps, culminating in an expected utility index, which combined the probability, desirability, and time scores.

The results from the Decision Process Test were then correlated among themselves and with scores from the MAS, CAVD, PARI, and other standard instruments, scales revealing belief in such things as fate, animism, and supernatural causes, and scales related to such factors as general satisfaction and desire for certainty. When all of these data are subdivided according to whether the parent answered alone or jointly with his mate, and according to the socioeconomic class and sex of the parent, the authors obviously had enough work to keep a computer humming for a good many hours. There are thousands of *r*'s, scores of *F*'s, and dozens of factors.

And what came out of all of this? An enormous number of interesting and provocative conclusions. Just tracing down the implications of the most salient conclusions could keep investigators hopping for the next decade. Social class and sex have interesting effects on decision processes. Lower class women, for example, differed from their husbands and from middle-class subjects in that they considered the more immediate and optimistic outcomes of their behavior, rather than the more distant and possibly less desirable outcomes. Differences in the characteristics of decision making by middle-class males and females were not found, but both tended to consider themselves more autonomous and masterful over nature than their lower-class counterparts. Among the most important conclusions derived from the study is that the parents' general values and orientations toward life,

together with their cultural backgrounds, seem to account for more of the differences among individuals in their decision making than did their performance on traditional personality or intelligence tests. The particular situations that the parents were asked to react to also had significant effects on their responses; for example, the masturbation situation produced quite different types of decision processes than the other situations.

This is an exploratory, loosely organized field study. Its goal of generating new concepts and hypotheses is successfully met. One could be critical of certain specific methodological and sampling procedures, but criticism at this time seems inappropriate. Early explorers often take less efficient routes than those discovered later. We can hope that Brim and others will continue this line of research, with other subjects and other problem situations. It will then be possible to see how representative the present results are. With the guidelines presented in this volume future researchers will have an easier time of selecting the most efficient and potentially productive variables, and bit by bit the complex domain of the social psychology of thinking should become clearer.



*According to my scientific friends, one of our greatest, and most glaring, deficiencies is the failure of us in this country to give high enough priority to scientific education and to the place of science in our national life. Of course, these scientists properly assume that we shall continue to acquire the most modern weapons in adequate numbers as fast as they are produced, but their conviction does expose one great future danger that no amount of money or resources currently devoted to it can meet. Education requires time, incentive and skilled teachers.*

—DWIGHT D. EISENHOWER







#### CP's LEADING READER WRITES

Two months ago in this space it was reported that E. G. Boring's distinction between a magazine and a journal had helped *CP* create what was felt to be a bit of cosmos out of some mild chaos. At that time and again a month later, a number of paragraphs were used to nudge and poke the distinction, so as to see it better and to appreciate more fully its meaning. Only after a good deal of nudging, poking and mulling did the nudger have the good sense to go to the files to reread the letter in which the slightly cosmic distinction was made. It turns out that the letter dealt not only with the matter of the differences between journals and magazines but with a number of other facets of *CP*'s form and substance. It was a response to *CP*'s annual report (*CP*, June 1963, 8, 233f.) and, as evidence below shows very clearly, should not be allowed to turn an unseen yellow in an untouched file. So with what seems to be still good if belated sense, *CP*, with its author's permission, presents the letter here.

—F.H.S.

THE Annual report for *CP* is interesting and moves me now to discuss the function of *CP*, or rather the poles of the continuum on which *CP* can be placed. It is called "a journal of book reviews," and it turned out to be a magazine. These are the two poles: journal vs. magazine.

A journal, it seems to me in the context in which I intend it in this letter, is primarily expository and gives information. *Psychological Abstracts* and *Psychological Reviews* are largely journals. There are not many value judgments in them, and the writers do not put in many new ideas. A magazine

gives information and ideas that arise out of the imagination and perceptivity of its writers, and it specializes in value judgments. There is a difference. The critics of *CP* complain if the values of *CP* are too idiosyncratic and also if there is too little descriptive information about the book reviewed, although they never complained about the limiting case where the book is omitted and the descriptive information is zero.

Now where does *CP* use its space and what purposes has this use? Or, to put it in another way, where could *CP* save space in order to cope with the increasing number of reviews that are called for by the increasing number of books. Let me list what takes space in *CP*.

1. *Reviews*. They tend to be long. Imagination and values require greater length. The writers like the longer reviews; they are frustrated less than they are accustomed to being. I think the readers like them longer. But you could get more information in less space, sacrificing values to information to some extent. When *CP* was bidding for support (it never got as many subscriptions as it should have), the length of reviews seemed to count as an asset.

2. *Books Received*. A very important section, it would seem, and almost a necessary one, for it is the pay that *CP* gives for receiving the book free, and it frees *CP* from reviewing the book in return for the gift of it. It is necessary for *CP* to be free, and you could hardly save space here.

3. *CP Speaks*. A luxury in ideas and values, but it personalizes *CP*. It is the first big step toward a magazine and away from a journal. It needs to be taken in connection with *On the Other Hand*, where the readers talk to the editor in order that the editor may talk back to them. There is nothing else

like this in the APA; there is not even a general business meeting any more and the Council takes over its functions. People meet and complain about the dull and deadly unEnglish of the JEP, but they practically never tell the editor that unless they happen to know him and be talking to him. Here anybody can say anything in OTOH, and the editor can talk back and does, within reason. But a journal would not have *CP Speaks*. It would be a dead organ rather than a live one, or might I say an automated organ?

4. *On the Other Hand*. Here all four parties talk to each other: the authors, reviewers, the editor, the readers. This represents the ideal of democracy at which the APA has always aimed, often failing because of its enormous, unwieldly size. *CP* is no solver of the APA's problem in this respect, but it is a contribution in the direction that the APA has been wanting to go ever since 1892—if I may dare interpret its mind so surely.

5. *Pictures*. You have cut them down to one-half and I hardly noticed it. They were an asset when *CP* was on the make, for people liked to have their pictures in. Some puritanical souls objected to wasting space in this fashion, and no psychosomatic believer could claim that a picture gives as much information per square centimeter as does the text. Still, they add a human quality to *CP*, keep it over on the side of being a magazine, personal and less automated. You could save space with still fewer pictures.

6. *Reviewer's biography*. The 1956 *CP* did not have them in it, but the demand forced it. People wanted to know who these strange reviewers were and to have some evidence of their competence. I felt that democracy made this a just demand, and we sought to meet it in 1957. I think that your biographies are a little longer than mine were, so perhaps there is a chance to save space here. You could go back to a single sentence, as does the *Saturday Review*. Something is needed if democracy is to be preserved, however.

7. *Author's biography*. I resisted this for four years, against great pressure to tell the reader just who the author is. I thought it involved us in more work



and research than we could undertake, but eventually we put most of this off on the reviewers and the author was added, as seemingly more important than the reviewer, although actually probably not. The pressure for authors' biographies did not arise until all the critics had forgotten that there used to be no reviewers' biographies. They took that for granted. So it seems to me that this needs to be kept, but it could be cut down, even to a single sentence as in the *Saturday Review*. The more you cut these things down, the more you regress toward the journal from the magazine.

8. *Citation*. The citation of the book and the by-line for the reviewer have a lot of white space mixed in with them. *Psychological Abstracts* does much better in this respect if saving space is the point. That is what Allen Edwards wanted. But this bit of glory belongs with the magazine. It has fewer bits per square centimeter than does the text, but it lets the reader relax and know what it is he is reading about.

9. *Title*. Is the title only sheer fun? It does not get cited properly when the review is cited. It catches the attention, it builds up interest, it keeps the magazine live. It also takes up a lot of space, its own area and the white around it. *CP* could be published without titles, but that would be regression toward a journal.

10. *Aphorisms*. Partly this waste space is a function of not carrying over reviews to back pages. *CP* did that in one or two early numbers, but readers hate it, and *CP* reformed and has not done it since. That is bound to leave blanks that have to be filled in, and what happier way can be found than to put in these aphoristic statements of all and various kinds? The magazines (cf. the *New Yorker*) that carry over to the back pages or run on and on a column a page are in a way trapping the reader into associating with the advertisements, but my instinct is against this in a scholarly journal. Still, you could save space and maybe get more advertisements. The change is not ruled out.

11. *Instructional Media*. I still feel that this is not a proper department in *CP*. By sheer chance, the early decision was to exclude tests and include in-

structional media. One took it for granted that textbooks got reviewed, so instructional media seemed to be films at the start. Now it's all kinds of automation. These practical matters seem to me to lie beneath the needs of the scholar, even though almost every American scholar in psychology who is drawing a salary has to deal with these practical concerns. *CP* would be itself better without applied departments, so there is a space that could be saved for more reviews. At once there is the question as to where this topic would be handled if not in *CP*, as there has been the question about tests.

AND that is that. Is there a defensible argument for the magazine as against the journal? Is there any defense of having scholarship good fun instead of hard work? Can operant conditioning make *Psychological Abstracts* gay and exciting without change of content or format? Or is the kingdom of God not wholly within you, but some of it out in the environment? Is not this paragraph itself more readable because I have not put it in strict, descriptive, puritanical English?

Or does the magazine have an educational value that the journal does not? Does not the scientist have an obligation to publish? The standard answer is Yes. But does he not also have an obligation to be read? The standard answer in America really ought to be Yes, although in Western Europe I sometimes think it is No. A man writes something univocally, something that is so dull that it is hard to work to get through it: has he not missed his responsibility to the science? The egoistic savant thinks not; he thinks it is the reader's job to work hard so as to understand him. I think that the reader is going to have enough hard work to do even after all the authors have worked hard, not only to be understood, but also to entice the reader into reading. All this gingerbread, where *CP* could use a square centimeter more efficiently for a few more bits of information, is devoted to education, to enticing the reader to think and have ideas. That's what education is, promoting motivation

for the acquisition of information and the stimulation of thinking. All the extras of *CP* can be counted up on this side. I like to think that the icing on the cake is not wholly unnecessary.

—E. G. BORING

## Catalogue of Theories

Rolf E. Muuss

*Theories of Adolescence*. New York: Random House, 1962. Pp. iii + 184. \$1.95.

Reviewed by RICHARD H. WALTERS

The author, Rolf E. Muuss, now Associate Professor in Education and Child Psychology at Goucher College, came to this country from Germany a decade ago. He studied at the University of Hamburg and at several North American institutions before obtaining his doctorate at the University of Illinois. An earlier book was *First Aid for Classroom Discipline Problems* (1962). The reviewer, Richard H. Walters, has recently become Professor of Psychology and Chairman of the Department at the newly established University of Waterloo, Ontario, Canada. He moved there from the University of Toronto. He received his education at Bristol, Oxford and Stanford Universities and has taught at the University of Auckland as well as in Canada. He is engaged now in developing instructional programs in psychology and is forwarding his research on the role of reinforcement patterns and of observational learning in the development of social behavior. He has co-authored, with Bandura, both *Adolescent Aggression* (1959) and *Social Learning and Personality Development* (1963).

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during adolescence. The coverage of authors is relatively satisfactory, since Muuss is well acquainted with Central European theorists, such as Spranger, Kretschmer, Jaensch, and Rempelin, whose contributions have been usually neglected in the North American literature on developmental psychology. On the other hand, Muuss's treatment of theoretical issues is too superficial to serve as more than a starting-point for further reading. Moreover, when dealing with controversial issues, the book often conveys the impression that theories can be evaluated without reference to their parsimony, consistency, or efficacy for generating research, and without taking into account the outcomes of relevant research studies.

Muuss's interests in education are reflected in his inclusion of sections on the educational implications of different theoretical approaches. His suggested applications of theory to practice are interesting and often provocative, though not always convincing. For example, Lewin's concept of the enlargement of time perspective during adolescence is said to justify the teaching of history. The historical events which are described and interpreted in history books do not ordinarily form a part of the "psychological past" of the adolescents; consequently, this application of field theory (like a number of other applications of theory suggested by Muuss) is so far-fetched that it constitutes a distortion of the theoretical framework on which it is supposedly based.

THERE are two glaring omissions in the book. In the first place, Muuss fails to provide a sound treatment of social-learning approaches to developmental psychology. Hollingworth is briefly mentioned, but only in respect to her emphasis on continuity of development and without reference to her occasional application of learning principles to specific problems. It is, indeed, somewhat surprising that Muuss does not point out that a social-learning theorist is more or less committed to an emphasis on continuities in development. Moreover, he leaves the impression that the only major contributions of social-learning theory to developmental problems are Allison Davis' concept of

socialization anxiety and Havighurst's account of developmental tasks. The second major omission consists of a total, and almost unbelievable, neglect of Piaget, whose contributions to developmental problems have many implications for educational psychology.

The author's theoretical preferences may perhaps be inferred from his final chapter on contemporary issues. Muuss seems to favor the view that adolescents are rebellious and confused, and that, owing to a rapidly increasing rate of social change, the current generation of adolescents constitutes a sub-culture, the values of which are fundamentally and necessarily in conflict with those of the current generation of parents. Although Muuss briefly refers to Friedenberg's "The Vanishing Adolescent," which in some respects emphasizes conformity and lack of dependency conflicts among adolescents, he seems unaware of the considerable body of psychological and sociological evidence in favor of the view that adolescents, at least those from middle-class homes, rarely suffer any serious conflicts of value or have problems in establishing their independence.

Muuss expresses the belief that some form of stage theory should be accepted, apparently because this kind of theory has been favored by the majority of the authors he discusses. Partial consensus is, however, not an adequate substitute for weight of empirical evidence.

As a synopsis for undergraduates or educationalists without thorough training in general and developmental psychology, this book could be misleading, mainly on account of the author's failure to provide illustrative research material tending to support or discredit differing theoretical positions. Consequently, instructors may prefer to recommend *Theories of Adolescence* only to more advanced students who wish to become familiar with theoretical approaches to developmental problems and require an initial overview of some of the relevant literature.



America is the only country in the world where one is ashamed of having nothing to do.

—SERGE WOLKONSKY



## Neither Watered Down Nor Tidied Up

Roger Brown, Eugene Galanter, Eckhard H. Hess and George Mandler. Foreword by Theodore M. Newcomb

*New Directions in Psychology*. New York: Holt, Rinehart & Winston, 1962. Pp. v + 353. \$2.50.

Reviewed by STEPHEN KAPLAN

Roger Brown, one of the four authors here, received his PhD from the University of Michigan in 1952 and has recently become Professor of Social Psychology in Harvard's Department of Social Relations. Eugene Galanter received a PhD from Pennsylvania in 1950 and stayed there until 1962, at which time he became chairman of the department at the University of Washington. Eckhard Hess received his 1948 PhD from the Johns Hopkins University and is now Professor and Chairman of the Department of Psychology at the University of Chicago and Director of the W. C. Allee Laboratory of Animal Behavior. George Mandler is a Yale PhD who is now at the University of Toronto as Professor of Psychology. The reviewer, Stephen Kaplan, is a PhD (1962) from the University of Michigan, and holds the position of Assistant Professor of Psychology there. He teaches and does research in collaboration with his wife, Rachel, also a Michigan PhD. He reports his enjoyment of undergraduate teaching, finding particular satisfaction in his work with the department's honors program. He also is involved in an unusual variety of research activities.

IN his sprightly Foreword, Newcomb indicates that the intention of this volume is to furnish the interested student and layman with a sample of recent developments in the field that is both "fresh" and "readable." Like *The Scientific American*, *New Directions* provides semi-popular presentations, but it is clear that the length of these articles permits a far more thorough treatment. Selections of the kind presented



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alize these values in his relationships to the community.

The therapeutic strategy based on these assumptions unfolds in three stages: 1) the child learns to give and receive love with a new mother, the therapist; 2) he learns to generalize his new attitudes in a sheltered environment, the day-care program; and 3) he learns, gradually and with continued support, to extend his new learnings into his natural environment.

Dr. Riese has liberally laced her writing with accounts of the behavior of her child-patients as they progress through treatment. The resentful, suspicious, withdrawn, or aggressive youngster, each in his own way, works toward his goal "to be somebody." The emphasis in the anecdotal material is on interaction between child and therapist, illustrative of therapeutic concepts like establishment of the transference relationship, dealing with negative transference and resistance, and working through new insights. By implication, parent counseling and educational programming are auxiliary services, supporting the gains made in therapy rather than making a major contribution on their own. The difficulty in achieving major changes in parents' behavior in this sort of patient-population is obvious, but it seems equally obvious that an energetic and imaginative school program could provide important benefits to culturally deprived children. That it does not do so in the Educational Therapy Center may be an inaccurate conclusion, but the relative emphasis, compared to the elaboration of therapeutic concepts in Dr. Riese's writing, suggests that the educational program is not regarded as a primary agent in influencing children's behavior. A unique educational challenge is presented by this population of children, not only in remedial approaches to learning basic language skills, but in testing strategies for transmission of cultural concepts across the boundaries of a deviant sub-culture.

Dr. Riese paints a convincing picture of the efficacy of the Center's methods in treating neglected and rejected children, and suggests adopting the pattern as part of the general array of services for disturbed children. The suggestion would be more compelling if the descriptive material had been supported by follow-up data on outcomes. While the purpose of the Center is service rather than research, the advocacy

of a new pattern of treatment would seem to require some simple documentation of its effectiveness. It would be possible, without an inordinate investment of resources, to match individuals or groups to the patient population in order to compare broad social consequences of treatment vs. no treatment. In the Cambridge-Somerville Youth Study, for example, records of arrests and convictions over a period of ten years were used to assess the effectiveness of delinquency prevention measures. For the Educational Therapy Center's population, information on school dropouts, commitments to training schools, and treatment by other

mental health facilities would also be helpful in comparing the social development of children who have received treatment and children who have not.

Even without data on effectiveness, the method described by Dr. Riese is a clear invitation to re-examine a prognostic concept that has become part of the folk-lore of child-clinical psychology. Whatever the validity of Dr. Riese's method, the question she poses is quite clear: "Are there learning experiences that will allow children from an emotionally impoverished background to move into the developmental stream of mutually satisfying interactions with society?"

## Menstrual Taboos and Chi-Square

William N. Stephens

*The Oedipus Complex: Cross-Cultural Evidence: With a Chapter on Kin-Avoidances in Collaboration with Roy G. D'Andrade.* New York: Free Press of Glencoe, 1962, Pp. v + 273. \$6.00.

Reviewed by GEORGE DeVOS

*The author, William N. Stephens, received his PhD with John Whiting at Harvard and then wrote the present book during the following two years while holding a post doctoral fellowship in Harvard's Department of Social Relations. He is now at The University of Kansas, where he holds a multifaceted appointment and where he has recently completed work on another book; The Family in Cross-Cultural Perspective. The reviewer, George DeVos, is an anthropologist turned psychologist; he took his MA in anthropology and his PhD in psychology, both at the University of Chicago, where he worked under David Shakow, Lloyd Warner and Donald Fiske. His varied experience includes a stint as Research Associate in Psychology at Michael Reese Hospital and another as Chief Psychologist at Elgin State Hospital in Illinois. Since then he has spent two years as a Fulbright Research Grantee in Japan at Nagoya National University*

*and has taught at the University of Michigan. Since 1957 he has been at the University of California at Berkeley, where he is an associate professor with multiple roles.*

THE SYSTEMATIC compilation and categorization of human behavior derived from numerous ethnographic studies comprising the "Human Relations Area File" is now found at a number of universities. The data in these files have provided strong impetus to cross-cultural comparisons of both psychological and social-structural variables. Recent studies, many of them under the guidance of John and Beatrice Whiting of Harvard University, have been attempting to use individual cultures quantitatively as separate cases. In these studies tests are made of possible correlative relationships between specific cultural-social and legal institutions, on the one hand, and, on the other, behavioral traits inferred from a study of



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By **EGERTON L. BALLACHEY**, *University of California, Berkeley*. \$2.95

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childhood experiences and patterns of family life. The present work is a carefully described example of one of these studies. As such, it suggests both their value and their limitations.

The title, *The Oedipus Complex: Cross-Cultural Evidence*, is somewhat misleading. One might expect, perhaps, a detailed, comparative, qualitative examination of formative experiences, psychosexual vicissitudes, and the like. This is not the case. The author's data, method, and testing of evidence are quantitative throughout. Qualitative analysis or description is found only as an adjunct. The author assumes that the reader has a familiarity with the complexity of psychoanalytic theory and has done at least some previous reading of ethnographic descriptions.

Stephens states carefully that he is concerned only with that limited portion of the psychoanalytic theory dealing with early sexual involvement between mother and son. Specific about what he seeks to affirm step-by-step, the author sets up a series of discrete hypotheses to be tested by chi-square. He seeks to establish possible correlations between (a) particular culturally prescribed ideas and practices concerning female functions (b) indirect measures of what he assumes to be the intensity of sexual attraction felt by the son toward the mother. The cause and effect chain of assumptions and hypotheses which he seeks to test at certain points are as follows: a long post-partum sexual taboo and other intercourse taboos found most characteristically in polygynous cultures indicate a dilution of the husband-wife sexual marital relationship and consequently an intensification of the mother's covert sexual interest in her child. In turn, this pattern results in an intensification of the growing child's sexual interest in the mother. This mutual intensification leads to a consequent continuing involvement into adulthood of the entire emotional patterning termed the Oedipus complex. The more pronounced the dilution of the marital relationship and the intensification of the mother's sexual interest in the child, the more manifest in the particular culture will be signs of unresolved anxiety related to Oedipal feelings.

Seen in the model of a psychological experiment, what the author seeks to do is to measure, on the one hand, the manifest institutionalized cultural behavior related to post-partum sexual taboos and marital sex diffusiveness, and then, on the other hand, to establish the ratings of the concomitant anxiety that is assumed to result from the continuing operation of the Oedipus complex. This anxiety, the thinking goes, causes the establishment of or helps maintain menstrual taboos and other restrictive practices related to female sexuality. In this test design, the hypothesized mutual mother-child intensity of covert sexual interest becomes the intervening variable. Unresolved sexual anxiety results in a continuing unconscious confounding of mother and sexual object. Such anxiety in male adults leads to the establishment and continuation of specific prohibitive practices, which will vary from culture to culture in severity, depending upon the degree of anxiety.

**M**ENSTRUAL taboos are world-wide and are a central index of Oedipal anxiety in adults, according to the author's thinking. He therefore fashioned a Guttman-type scale for measuring the severity of menstrual taboos, ranging from concern with the "dangerous" or "harmful" potentials of menstrual blood as the weakest indicator, to extremes found in severe taboos involving the establishment of special menstrual huts. He then scaled a number of indicators of sexual anxiety, rating these plus or minus when mentioned as specifically present or absent in ethnographic reports. Arriving at a score for each group, he compared it with the menstrual taboo scale and obtained a correlation. The groups clustered either characteristically high on both scales or low on both, so that an extensiveness of menstrual taboos relates, at better than the one percent level of significance, to other measures of sexual anxiety.

He is not able to rule out the possibility, as he himself says, that the positive correlation between post-partum sexual taboo and consequent variables is merely an artifact of the effect

of polygyny per se or of the resulting mother-child households.

As is true in all quantitative approaches, one must over-simplify theory in order to manipulate discrete variables. Dynamics other than those espoused by the author may explain his results. Variables not considered in the study may in actuality contribute more to the behavior than those considered. For example, one must consider the relative legal and political power of the male in overtly polygynous societies. The legality that sanctions multiple wives may also afford the males the power to enforce restrictive practices toward females. These restrictions themselves may exacerbate underlying fears. Analogously, in a state discriminating between different groups, such as South Africa, increasing fears resulting from progressively restrictive practices may cause further fear to arise in regard to the submerged. It may well be that an increasing exercise of power and dominance itself induces a consequent institutionalized fear of the restricted group. On the other hand, it does not follow necessarily that men in societies who have less complete dominance are any less fearful or that they manifest in lesser form the particular Oedipal traits that the author seeks to establish. Much as Stephens's theory helps explain the statistical appearance of certain forms of correlations between polygyny and later negative restrictions on sexual functions of women, it does not necessarily preclude the possibility that equally intense continuities of Oedipal feelings would occur in certain monogamous societies where maintaining a double standard for men amounts to the practice of informal polygyny. In other words, monogamous societies themselves differ in the degree to which these are intensive or diluted marriage relationships.

In nineteenth century Western Europe, one saw a double standard in which the sexual lives of women were much more severely circumscribed and inhibited than those of men. In that society, the one that Freud observed and from which he generalized the universal functioning of the Oedipus complex, women de facto were very often



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without orgasmic sexual gratification. There was a great prevalence of frigidity and a lack of intimacy between husband and wife. The sexual act itself, for a large number of women, was not gratifying; and women in such a society characteristically turn to their children for substitute forms of gratification. The sexuality was often not overt but symbolically disguised in other maternal concerns. Such societies certainly produced a large number of individuals who could be said to have severe anxiety concerning the sexual functioning of women.

THE REVIEWER is saying, in other words, that Stephens's interpretation would be in line with Freud's; certain primitive societies, according to the nature of the relationship between the sexes, overtly manifest, in their ritual and in their institutions, what was found in relatively covert neurotic behavior within nineteenth century Western society. Looked at cross-culturally, it is obvious that some societies go to great extremes to institutionalize avoidance techniques and taboos, while in others the anxiety inferred to act as motivation for such restrictions appear only in more subtle non-institutionalized forms. The author's contention is that the Oedipal situation is universal; nevertheless it can, depending on the culture, take on it particular forms of institutionalized intensity which relate to institutionalized dilution of the marriage relationship itself.

This necessarily brief and partial description of the argument, design, and method used by Stephens in his book may be sufficient to indicate that the use of cross-cultural methods following the work of John and Beatrice Whiting is concerned with controlling and testing data in a manner more in harmony with standards established in psychological research than with those found in the more non-quantitative approaches characteristic of the anthropological tradition. Such increasing concern with method within anthropology cannot but have an effect on standards of reporting ethnographic data.

CRITICS of Whiting and others sometimes emphasize the questionable nature

of the ethnographic reports on which their quantitative comparisons are based. There is no doubt, for example, that an assessment of the "severity" of puberty rituals based on written evidence is open to varying judgments. Some critics point out that even in the more recent ethnographic studies initiated by the Whitings themselves there is difficulty in classifying traits as mild or severe, and that, in setting up a classification system within the area files, judgment actually was easier in the past since scanty evidence made a quick, although possibly questionable, judgment more possible.

There is a missing of the point in such criticism of the attempt to set up the area files and to establish researches based on comparative ethnographic data. What is being established is a potentially fruitful scientific method for assessing behavioral dynamics cross-culturally. It is a spur to present day ethnography to produce more careful documentation related to formulated problems. Comparisons between cultures must be made on the basis of adequate evidence. It is essential to the course of science for theory to offer a challenge to method, and for method, in turn, to challenge the standards of recording and observation that were considered sufficient in the past. What is necessary to training within anthropology today is a raising of standards of observation by developing for field workers a more disciplined sense of problem. Once the challenge is issued, subsequent work, to be considered scientific, cannot be performed below standard. While criticism may be directed to present imperfections and limitations, the attempt of Stephens deserves commendation and further emulation.

Hopefully, books of this kind may also act as a stimulation for the psychologist to greater involvement in cross-cultural work. This work written in a familiar conceptual methodological framework may act as an inducement to younger psychologists to seek answers to problems of human behavior outside the secure, if not somewhat stifling, confines of the laboratory.



## How to Get and Use Knowledge

Claire Alice Selltiz, Marie Jahoda, Morton Deutsch and Stuart W. Cook. Editorial - readers Isidor Chein and Harold M. Proshansky

*Research Methods in Social Relations*. Revised one-volume edition. New York: Holt, Rinehart and Winston, 1961. Pp. v + 622.

Reviewed by JEROME E. SINGER

The first author, Claire Alice Selltiz, is a research Associate in NYU's Research Center in Human Relations. She received her BA from the New School and her MA from NYU. Marie Jahoda, Morton Deutsch and Stuart Cook are, only more so, the same well known social psychologists they were when they produced the original editions of the present book. Stuart Cook has just moved to the University of Colorado. The reviewer, Jerome Singer, spent three years at the University of Michigan as a chemical engineer and then worked for a while as a naval architect before taking a BA in Sociology and Anthropology. He then went to the University of Minnesota and after four years in the Social Relations Laboratory received the PhD. Then there was a post doctoral year in the Histochemical Laboratories, before he moved, in 1961, to Pennsylvania State University as Assistant Professor of Psychology. He does research in affiliation, conformity and social psychopharmacology.

IN THEIR preface to the one volume revision of the earlier two volume work, the authors are quite explicit about their goals. They seek to provide a text for undergraduate courses which can initiate students to research methods in sociology and social psychology while avoiding the Scylla of technical detail and the Charybdis of patronization. That they were only partially successful in achieving their aims is in good part due to the impossibility of the task and does not detract from the



utility and contribution of their work. They have developed a book which will take students interested in survey research from "hunch" to "handout" in the preparation of a study and have done so without being too abstract and without creating a "cookbook."

It would seem almost a truism that any attempt to discuss in one volume topics as diverse as research design, measurement, sociometry, and the relationship of research to theory will be uneven in quality. Yet, the work is not overly spotty. There is a failure to cover all relevant methods and strategies in social research, but the fault is emphasis of selection rather than inadequacy of treatment. Chapter 13 is prefaced by a quotation from Whitehead, "unapplied knowledge is knowledge shorn of its meaning." Although placed physically near the end, the quote is the keystone of the work, for most of the examples and techniques are drawn from studies with a direct practical application: interracial contact in public housing projects, racial discrimination in Manhattan restaurants, the effects of a summer orientation program on the attitudes of foreign students. While the discussion of these topics is cogent, the emphasis on questions centering around social problems results in a neglect of those techniques and methods utilized in research of less immediate application. Experimentation as a technique in social relations receives favorable mention but relatively little discussion or explication. Casual inference, randomization, matching, designs, controls, interactions, and generalizations are all dealt with in one 33 page section; in contrast, over 13 pages of the first chapter are illustrative material drawn from a single monograph of an interracial housing study.

In the course of the book, Selltitz, et al raise and discuss a surprising number of important technical points. They do not gloss over them and, though they refer to other sources, they reserve to themselves the burden of explaining these issues. Although in places the book seems padded with an excess of anecdote, the authors are shown to best advantage by the clear,

jargon-free prose and clarity of organization which they employ. A case in point is the brief but lucid treatment of rating and attitude scales.

The existing biases of topic selection,

which are rather limiting, reflect the authors' own research interests. As a text for courses with similar foci, the book should have widespread utility in several disciplines.

## The Development of The Psychoanalytic Child

Ruth S. Eissler, Anna Freud, Heinz Hartmann and Marianne Kris (Managing Eds.)

*The Psychoanalytic Study of the Child, Vol. XVII.* New York: International Universities Press, 1962. Pp. 493. \$8.50.

Reviewed by LOIS B. MURPHY

*The managing editors of this volume, Ruth S. Eissler, Anna Freud, Heinz Hartmann and Marianne Kris, are all well known leaders in psychoanalysis and have been for many years. The papers are predominantly by child analysts and psychologists involved in psychoanalytic research with children. The reviewer, Lois Barclay Murphy, is the wife of one psychologist, the mother of two children, and grandmother of four others. She absorbed William James with her milk from a mother who had studied at the University of Iowa with and greatly admired two of James' students, Carl Seashore and G. W. H. Patrick. Her father studied with Dewey at the University of Chicago. She herself studied at Vassar with Margaret Floy Washburn and Miriam Gould and at Columbia with Jersild and Lois Stolz, receiving her PhD at the latter institution in 1937. She taught at Sarah Lawrence from 1928 until 1952 when she came to Topika and the Menninger Foundation where she is now a research psychologist and a therapist. Her most recent book, The Widening World of Childhood, will soon be reviewed in CP.*

foreshadowed new developments in psychoanalysis in several directions: he brought aggression into drive theory; he formulated the stimulus-barrier role of the ego; he emphasized the unique importance of the mother in the pre-oedipal phase; and he re-emphasized the role of mastery through fantasy and play in normal childhood. Implicit in the thinking reflected in the "stimulus-barrier" was the assumption of the central problem of organism-environment interactions in an open system, interactions both reflecting and contributing to the structuring of the ego. In subsequent years creative developments in psychoanalysis centered on further understanding of the drive and interaction sources of aggression; structure, functions and early development of aspects of the ego and related structures such as the superego and ego-ideal; processes involved in the development of object relationships, and the planned use of play in child analysis. Our review of a notable volume of new psychoanalytic writing can gain one perspective by looking for the extent to which these major avenues of understanding are explored further.

Readers of a publication concerned, as CP is, with all of the sub-disciplines of psychology and many neighboring disciplines will also be interested in such questions as these: on what kind

**M**ORE than forty years ago Freud presented in *Beyond the Pleasure Principle* hypotheses and concepts that



of evidence are conclusions based, what processes of correction and revision of formulations are available, how much flexibility is there in the interaction between data and theory? Is theory capable of being expanded to allow room for new observations or is it so rigid that new facts are not allowed recognition?

Answers to the latter questions come from a review of the variety of papers offered here, their sources, the author's attitudes toward their own contributions. Here we note first the wide range of kinds of empirical findings—from rich short-term and longitudinal direct observations of individual children to a statistical study of many children; and a study of a group of cases with a common syndrome, at one extreme, to a discussion, at the other extreme, based on the parallel analysis of a mother and child. Beyond these is the use of excerpts from the unique mine of data available in indexed weekly reports of therapy at the Hampstead Clinic—the product of a heroic effort to learn from the organized findings of a large collection of clinic cases. A predominant impression is that this volume is inductively oriented to a large extent, and that the authors are concerned with the careful use of data available in both long-term treatment cases and intensive direct observations. While the psychoanalytic orientation helps to organize observations it does not straitjacket them here, much less does it selectively read out of court specific items which do not neatly fit theoretical assumptions. In general the authors show both the spontaneity and the empirical conscience of the scientist concerned with careful study of data and sound progress of his science.

IF now we return to trends in the topics presented, we see that a number of papers deal with effects of physical damage or defect and the effects of traumatic life experience. Over half of the total number deal with or throw light on mother-child relationships. A small but solid group is concerned with theory of the ego ideal and of the superego. Anna Freud's condensed formulation of an approach to the as-

essment of childhood disturbances not only organizes the relevant structural concepts, etc. but emphasizes the central issue of the child's growth capacity, and the need for contributions from varied disciplines such as descriptive and dynamic psychiatry, psychology and the social sciences to provide a comprehensive metapsychological picture of the child.

This range of subject matter invites some reflection on current trends in psychoanalytic thought here, as they are influenced by the focus on early factors in normal and in pathological development. The concern with defect and damage implies an increasing awareness of the adaptive problems the idiosyncrasies of the organism present to itself and to the environment, while the interests in subtle inadequacies in mothering, separation, in operations such as tonsillectomy, imply the counterpart—a concern with different impacts of the environment in early sensitive phases of development. Whether the focus is on organism or environment the central concern—equally important for psychoanalysis and for general psychology—is on the resultants for psychic structure and growth of the deprivations, overstimulation, traumata, ongoing stress or developmental disturbances as well as conflicts seen in the experience of an infant.

For example, George Klein comments on Omwake and Solnit's observations of a congenitally blind child that interpersonal isolation *without* sensory interference can produce symptoms; but that sensory deficit alone, without affective and interpersonal isolation, need not. What is at stake in the more critical deprivation involved in interpersonal isolation, is the integrity of organizing rules or "transforms" that make it possible both to accommodate and "conserve the redundant structure of the environment" and contribute to the intrapsychic structure. It is through the "continuing feedback-evaluation process by means of which the child develops ideas of an anticipatable and predictable reality" that distinctions between self and other and between self and the environment are developed. A related point of basic importance is that the information from components of affect experience—its "signal" aspect—are critically important to drive differentiation and drive control.

"It is through affective reverberations that drive tension is known, and it is through affects produced by drive generated contact with the environment that the consequences of such contacts are in part recorded in structures of action and thought." Here data from an experiment of nature are used to clarify the currently living issues of development of a differentiated self and object, issues also dealt with in Rubenfine's article on "Maternal Stimulation, Psychic Structure, and Early Object Relations." He briefly reviews the behavior of two mothers reported on by Escalona and contrasts the interactions between these mothers and babies with reports of autistic or atypical children. He comments that the autistic child may be concentrating on toys and other objects because these have clearcut stable boundaries, are unable to initiate action, and are thus under control of the child. That is, the pathology need not be a matter purely of failure of distinction of self and non-self; it may have arisen from a much too early perception of the separateness of the object, due to a high level of need tension, pain, etc. from which the mother cannot or does not protect the child. This premature awareness of the object may be associated with "the premature differentiation of aggression with which such primordial precepts are then invested." In essence here, he describes consequences that arise when the earliest relationships originate in an atmosphere of pain or conflict rather than in one of need satisfaction and tension discharge.

Another extension of understanding of environmental influences is provided by Marjorie Sprince's article on "The Development of the Pre-Oedipal Partnership Between an Adolescent Girl and her Mother," a report uniquely possible in simultaneous analyses, of the impact on the third generation of disturbances in grandparents which in turn distorted the development of the mother of the child patient. There was evidence that very early ego disturbance led to probably permanent ego damage to the child.

I have selected for illustration contributions of obvious importance for a general theory of development. Equally broad in its implications is Ernst Kris's article on "Decline and Recovery in a Three-Year-Old," which is also a model of luminous writing. With vivid strokes he paints a "picture that mirrors the conflict of the age between growing independence and old attachment" in a nursery school group, and he continues with incredible conciseness to suggest the in-



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finite variations in the ways in which different children experience this clearly structured situation. The central question with which his study deals is: how have decline and recovery come about, and how lasting does the recovery promise to be? Ernst Kris did not live to reach the final goal he set for himself in this paper but the formulation of the problem of decline and recovery processes, and the account of the developmental experience of the child—at times evocatively concrete, while at other times succinct in its summarizing of many repeated sequences—provides both a model and inspiration for further efforts to probe the sources of resilience in childhood—a crucial area for understanding development.

Another discussion concerned with a topic of broad interest is Sandler and Rosenblatt's "The Concept of the Representational World." "In all our considerations we have made use of a notion which seems to us to be a central one in psychoanalysis, that of the child's subjective *world*, a world which is only gradually differentiated in the course of development as a consequence of processes of biological and psychological adaptation. As we have used it, it includes Freud's *Internal World* (1938) and Hartmann's *Inner World* (1939) . . . and is related to the concepts of the child's world described by Piaget (1937) and Werner (1940) as well as to the work of Head (1926) and Schilder (1935) on the body schema or image."

This approach to the subjective image which represents the child's internalizations of and distortions of reality must not only include the concepts of Piaget and the others mentioned; logically it cannot avoid dealing in some way with inner representations of earliest "objects" as Sullivan, Fairbairn and the Kleinians have tried to describe these, and also the representations of the self as foreshadowed by Schilder and later by Jacobson in psychoanalysis and by Gardner Murphy and others in psychology.

Interestingly enough, while it is apparent that many lines for rapprochement between psychoanalysis and psychology, and also other disciplines, can be seen in these studies, the emphasis on ego psychology as such is less in this volume than in many of the preceding ones. An important group of theoretical articles deals however

with refinements of the concepts of superego and ego ideal—concepts which will doubtless undergo still more revision after they are seen in relation to the inner objects of the child's representational world.

ONE way of describing trends in psychoanalysis as glimpsed through the window offered by this volume then, is to outline those stimuli to revision and development of old concepts and of new formulations which are provided by a) response to a broader range of empirical data from both psychoanalytically oriented observations and results of research in adjacent disciplines, especially those concerned with development, normal and abnormal, and b) refinements required by the impact of such revisions and new concepts upon previous formulations.

The history of psychoanalysis up to now may be broadly seen in terms of a) the clarification of the role and development of drives and conflict, b) the psychoanalytic view of the ego and its mastery problems, c) the development of object relations. We can expect the current developments to contribute a more differentiated view of both organism and environment, their mutual influences and contributions to the psychic life of the growing person and to the total functioning self which develops from both the conflicts and the nourishing encounters between itself and the environment, as well as between drives and ego, and among drives themselves, and among the various ego-functions.

Psychoanalysis has never reached a phase where it could be called static. Even though it has lost more than one of its early creative thinkers, it continues to evolve through the force of its own needs for coherence, and for new clarifications, and the generative power of impacts from the realities of human development and of exchanges with intellectual neighbors, as well as the need to resolve conflicts between different viewpoints even within its own fences.



## First, Addition

Gunnar Goude

*On Fundamental Measurement in Psychology.* Stockholm: Almqvist & Wiksell, 1962. Pp. 176. Sw. Kr. 18: -

Reviewed by ANDREW COMREY

*The author, Gunnar Goude, is associated with the Psychological Laboratory of the University of Stockholm. The reviewer, Andrew Comrey, is Professor in the Department of Psychology at the University of California at Los Angeles. He received his PhD from the University of Southern California where he worked with J. P. Guilford, and taught at the University of Illinois and then at the University of Southern California before moving over a few blocks to UCLA. Psychometricians and others know his numerous articles, most of which deal with one facet or another of psychological measurement.*

WRITERS on the theory of measurement have frequently insisted that no measurement of a fundamental kind is possible without a physical interpretation of the operation of addition which meets certain basic algebraic properties. It has been insisted further that psychological properties have no physically defined operation of addition and hence are not measurable except in some more primitive sense, e.g., rank-order measurement. Goude advances the thesis that this is much too pessimistic a view of psychological measurement. Not only do certain "direct" and "indirect" measurement procedures seem to come very close to fundamental measurement, according to him, but it is even possible to devise ways of interpreting the operation of addition for certain psychological properties. Several experiments are described and the results offered in support of the author's thesis.

The book is a small volume translated from Swedish. The treatment of the theory of measurement is brief, based primarily, according to the au-



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some new data, there is, much of the time, a scrupulous effort to avoid easy generalizations. The emphasis throughout is on the complexity of the concept, and on the many different antecedent and concomitant variables associated with maternal deprivation.

Ainsworth gives by far the most comprehensive coverage of the literature and the issues. In the preface her paper is represented as an authoritative statement of Bowlby's present position. Many points in her reassessment of the literature are similar to those made in the other recent reviews. She also analyzes maternal deprivation in terms of a) "insufficiency," a quantitative deprivation of maternal care; b) "discontinuity" resulting from separation, and c) distortions in mothering. Her analysis of the methodological deficiencies of many studies indicates the limitations of the evidence, and points to the kinds of research designs needed to handle more adequately the complex research questions.

Several of the other contributors, Andry, Lebovici, and Wootton, especially, emphasize the crudeness of the concept of maternal deprivation and point to the difficulties in testing a theory in which the basic terms lack empirical referents. Although somewhat less critical of the research, Lebovici also underlines the danger of using a single concept to explain the genesis of psychopathological disorders whose etiologies are obviously very complex. From the viewpoint of the therapist, he makes the interesting point that the handling of object relationships during psychotherapy does not offer confirmation of the historical value of reconstructions of traumatic events related to separation and privation during the first months of life. He suggests that external criteria are needed.

Analysis of a concept often results not only in simplification, but also can suggest new dimensions. In a detailed discussion of the effects of "masked deprivation"—the subtle and covert distortions in mothering which occur in intact families, Prugh and Harlow emphasize that psychological separation may be as serious as physical separation from the mother. Andry points out that the exclusive concern with maternal depriva-

tion overlooks an important aspect of the child's experience—"the subtle and basic triangularity which exists between a child and both his parents."

MEAD's and Wootton's papers are most stimulating, and succeed in giving fresh perspectives on the issues. From a cultural anthropological perspective, Mead raises many questions about the generalizations concerning the effects of maternal care practices which are deviations only from a Western norm. She develops a case for the cultural anthropologist's orientation . . . "as a corrective to the present tendency to overattribute certain consequences to single causes or sequences of events, such as breast-feeding or its absence, separation from a mother-figure, institutionalization, early or late toilet training, swaddling, etc., which has been characteristic of attempts to apply clinical insight to the establishment of viable theory and to the development of comprehensive recommendations for changes in social practice." She notes that separation trauma is much more common and more serious in cultures which foster an exclusive mother-child relationship than in cultures where the child learns to adapt to several loving mother-figures. In a strong indictment of institutional care which she sees as a civilized refinement of infanticide, she states . . . "present institutional practices are only a prolonged, ritualized method of disposing of the infant for whom no one wishes to care."

Wootton deplors the prevalent use of maternal separation as an all-embracing cliché to explain the etiology of delinquency and a variety of personality disorders. She concludes that the new evidence since the Bowlby report raises even more questions about the validity of attributing permanent damage to maternal deprivation, and suggests the need for longitudinal and follow-up studies.

Maternal deprivation is an area of research in which there has been an intimate interweaving of clinical concerns and basic theoretical issues. During the past ten years there have been important theoretical developments on a number of issues—the development of object relationships in infancy, the rel-

evance of critical period concepts to human behavior, the effects of sensory stimulation and deprivation on development, neurophysiological processes in stress. There have also been many significant animal studies that have stimulated new insights and hypotheses for research on human maternal care. It would have added much to the significance of this volume had there been a chapter on the theoretical advances and some attempt to evaluate the significance of the comparative animal data.

While each individual chapter makes its own distinctive contribution, the volume as a whole suffers from the lack of an editor. Each chapter stands by itself, with only occasional cross-references. As a result, the volume lacks coherence, and there is a repetitious recital of the same studies. This repetition is not always without interest, since it illustrates the divergent interpretations of the same data that are possible by people with different theoretical commitments.

The criticisms of the concept and the critical evaluations of the research on maternal deprivation in this monograph are not likely to activate a complete swing of the pendulum toward a negation of the importance of early maternal care. Rather these reassessments should help in achieving a more balanced orientation regarding the effects of deprivation in early maternal care, and should provide a clearer perspective for the formulation of research problems. The complexity of the concept becomes evident after analysis of the varieties of conditions of maternal deprivation and of the modifying variables. Perhaps we can look forward to some reassessments of these reevaluations ten years from now.

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# On Metonyms and Polysemy

Stephen Ullmann

*Semantics: An Introduction to the Science of Meaning.* New York: Barnes and Noble, 1962. Pp. 278. \$6.50.

Reviewed by DAVID McNEILL

The author, Stephen Ullmann is Professor of Romance Philology in the University of Leeds. His earlier publications include the book, *Principles of Semantics* (second edition 1957). The reviewer is David McNeill, who received his 1962 PhD from the University of California (Berkeley) under Postman, Susan Ervin and Mark Rosenzweig and has spent the last year as an NSF Postdoctoral Fellow at the Center for Cognitive Studies at Harvard. One might judge that he has done well there for in September he became a member of the regular staff of the Center with the position of Research Fellow in Cognitive Studies. He has been working with Roger Brown and has been concerned, among other things, with experimental studies of word association and with semantics in child language.

THE ACTIVITY of European philologists has burgeoned greatly in recent years. This book, like its predecessor by the same author (*Principles of Semantics*, CP, Jan. 1959, 4, 14), is written partly as a summary and partly as an interpretation of the philologists' scholarly output. Compared to the earlier book, the present one summarizes more than it interprets; theory is not neglected, but the book is written more as a progress report than a theoretical pronouncement. Ullmann's aim is to integrate recent research on semantics with the material covered in the 1951 edition of *Principles*. (A second edition of *Principles* came out in 1957; except for one supplementary chapter on recent developments, how-

ever, it was a reprint of the first.) Fully 50% of the references in the present book are to publications since 1950, a fact that demonstrates the need for a work of this sort, and its appearance will be welcomed by all those concerned with semantics.

Psychologists will find much here to fascinate them. The psychological reality of the word is an example. Phonemes, morphemes, phrases, and sentences are the standard units of language description. The word does not fit into this scheme. But, to quote Ullmann, "There can be no doubt that awareness of words, as distinct from other linguistic units, lies at the very root of man's whole conception of language." There is good reason to believe that the learning of language by children is accomplished through an intuitive analysis identical to the explicit analyses of linguists. The infant and the linguist are each faced with the same task. They both need to discover, on the basis of a limited corpus of utterances, the structural relationships that will produce all and only the utterances of a language—a goal that characterizes both full fluency and adequate description. We would expect the child and the linguist to discover the same units in their progress to this common goal. Perhaps they do, but words are prominent to speakers and unessential to linguists. It is this special psychological status of words that needs to be explained. Literacy evidently cannot account for the phenomenon, since literate and nonliterate peoples alike assign a central role to words.

A different and very intriguing problem is onomatopoeism. These are

words that attempt to name non-vocal sounds through mimicry. *Buzz*, *tic-toc*, and *meow* are some English onomatopoeisms. They appear to offer no special problem. However, an unsuspected dependence of onomatopoeic effect on meaning appears when certain homonyms are considered. For example, *peeling* potatoes is not onomatopoeic, but the *pealing* of bells is. Similar pairs are *ring* on the finger vs. *ring* the bell, and a *great* laugh vs. a laugh that *grates*. Clearly, the meaning and the sound of a word somehow interact to create an effect of special force. Sound symbolism cannot explain these homonymic pairs. Ullmann speaks of "resonance," but this is only a descriptive metaphor; the problem, which is a psychological one, is to understand such resonance.

THE idea that seems to be richest in implications for psycholinguistics and psychology generally is Ullmann's suggestion that there is such a thing as semantic productivity. This is a novel idea, since productivity is usually thought of in connection with grammar. Grammatical productivity is the capacity to produce new but comprehensible expressions. It is a feature of great utility in language, for it allows users to escape the bounds of rote repetition. Semantic productivity is the same phenomenon in the lexicon. It provides language users with the means of enlarging their lexicon by principle rather than convention, so that the store of lexical information can be increased without further encumbrance of memory.

A major result of semantic productivity is "motivation" in language. Examples of motivated expressions are compounds, metaphors, and metonyms, such as *penholder*, the *hood* of a car, and *gown* when it is academic. They are the fruits of past semantic productivity, now frozen into the language. Another major result of semantic productivity is polysemy, the existence of multiple meaning. Again, metaphor and metonym provide examples: the *eye* of the storm was the chairman of the board. The psychological implication of these examples is that the number of





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motivated and polysemantic expressions frozen into a language can be an index of individual users' semantic productivity. Languages differ in the number of motivated words contained in their lexicons. English, for example, has relatively few motivated words compared to German. If the hypothesis is correct, the resources for semantic productivity should differ between the two languages, with German having the richer system. Speakers of German should coin more words, discover more metaphors and metonyms, and in general manipulate their lexicon more freely than English speakers. Daily experience suggests this may be so, although the hypothesis has not been studied.

rules of semantic productivity would be more psychological, i.e., ramify more than syntactic rules into other cognitive areas than language processing. Indeed, if there is anything to the notion of semantic fields, a set of generative rules for semantic productivity would be a model for thinking as well as for enrichment of the lexicon, and so would have very strong implications for the psychology of cognition.

## Psychology Bland

Louis S. Levine

*Personal and Social Development: the Psychology of Effective Behavior.* New York: Holt, Rinehart and Winston, 1963. Pp. vii + 510. \$6.50.

Reviewed by GOODWIN WATSON

*Louis S. Levine, the author, attended a State Teachers College in New Jersey before going West to Stanford University for his graduate training. He has served as a psychologist in the VA at Palo Alto and with the California Bureau of Vocational Rehabilitation. He now teaches courses in personal and social adjustment at San Francisco State College. Goodwin Watson, the reviewer, has taught psychology of personality and social psychology at Teachers College, Columbia University, since 1925 and with great distinction. Now Emeritus on the rolls of Columbia he serves as Distinguished Professor at Newark State College in New Jersey.*

THE MAJOR problem set before psychologists by semantic productivity is to discover the mechanisms of enrichment. In part, this is a question of structure, and Ullmann refers here to Jost Trier's theory of semantic fields. Semantic fields are units of organization in the vocabulary; they are formed of words that occupy the same "conceptual spheres." How conceptual spheres are recognized independently of semantic fields, itself is an unresolved question, but Ullmann's examples of color and kinship, both of which are omnipresent and partitioned differently in different cultures, will suggest the nature of the distinction. While questions of structure are essential to understand semantic productivity, they are only part of the problem. Structure and production are not uniquely related. The processes of semantic production operate on semantic structure, but additional hypotheses are needed to account for the processes themselves.

Linguists have stated grammatical productivity in terms of sets of rules which can be applied to generate the grammatical sentences of English. It has been argued that these generative rules also describe the cognitive processes of English speakers in producing speech. Perhaps semantic productivity can be stated in a similar way. Of course, semantic and grammatical rules are likely to show fundamental differences. One optimistic view is that the

brotherly love and a peaceful world. It touches on the themes embroidered in the popular magazines for women, but never says anything that would offend an apprehensive college president, a devoted parent, or a parish priest. It is eclectic, wholesome, superficial, and blandly for the general welfare. It is very American undergraduate.

Psychologists may wish to know that students will find, here and there, brief accounts of the familiar classical experiments from Albert's scary rabbit to Harlow's cloth-clinging apes. Freud will be summarized in a few pages which raise no deep or disturbing questions. All three learning theories (operant conditioning, trial and error, cognitive expectancy) are seen as equally acceptable in understanding the complex process. Non-directive counseling excerpts are reported. The reviewer found no substantial attention to dreams, to Jungian concepts, or to the existentialist ideas in contemporary psychotherapy.

The Levine text has many excellent qualities. It expresses a real concern for the problems of individuals and for the reduction of international tensions (Osgood's GRIT is commended.) Most of the writing is easy to read. The positions taken are those which would be supported by substantial majorities in any poll of the APA. There are some excellent full-page photographs; a number of case illustrations of problems of normal people; bits of popular anthropology; and a few, if any, statistical tables.

An instructor who tends to get a bit bored taking students over the very familiar material in psychological approaches to problems of everyday life might want to assign Professor Levine's book for students to read outside of class and to be examined on. He would thus make sure that they have all been exposed to a good, easily understood version of the commonplaces. He would then be free to use his class time to pursue any interesting and provocative ideas he or his students may propose.

The instructor who leans heavily on the textbook may be assured that Levine's would be a competent, not-too-demanding, fairly interesting, safe, and essentially non-controversial, choice.



# Philosophical Perspectives

John Herman Randall, Jr.

*The Career of Philosophy: From the Middle Ages to the Enlightenment.*  
New York: Columbia University Press, 1962. Pp xi + 993. \$13.95.

Reviewed by BENJAMIN B. WOLMAN

*The author, John Herman Randall Jr. is Frederick J. E. Woodbridge Professor of Philosophy at Columbia University. His main interest has been the history of thought in relationship to the central currents in the history of science. He is the author of Nature and Historical Experience (1958) and of Aristotle (1960). The reviewer, Benjamin Wolman, received his PhD from Warsaw University in 1935. He has been Assistant Director of the Centos Heilpädagogic Institute in Poland, Director of the Mental Clinic in Tel-Aviv, Israel, and Director of Educational Services for families of Jewish servicemen and Supervisor of Guidance Systems for them. He has taught at Teachers College, Tel-Aviv, at Teachers College, Columbia University, at CCNY, at Queens College and Albert Einstein College of Medicine. He is currently teaching at Hunter College. He is the author of Contemporary Theories and Systems in Psychology (1960) and editor with Ernest Nagel of Psychology and the Philosophy of Science (in press, Basic Books). He is also editor of the Handbook of Clinical Psychology, also in press (McGraw-Hill).*

It was thirty years ago when I had to take my orals in the history of modern philosophy. (Philosophy played at that time an important role in the training of psychologists in Central Europe.) I had to struggle through piles and piles of monographs and original works of the various philosophers. Whenever in despair, I turned to the two great histories of modern philosophy, Windelband and Hoffding. Wind-

elband was never translated into English, Hoffding was in 1900.

Randall's book is the first comprehensive history of modern philosophy in English. It is excellent English: profound yet easy to read, precise yet simple, scholarly yet elegant, complex yet lucid.

Randall's book conveys the irresistible feeling of historical continuity and relatedness of the various systems. There is logic in the controversies and good sense in his discussion. Randall's main message is that philosophy has its own career among sciences. In a way, philosophy binds them together. The book emphasizes the changing yet persistent development of the Western thought from the Greek tradition toward our age of atoms and space. There is the delightful discovery that Reformation, Renaissance, and modern science did not spring in full armor like Pallas Athena out of Zeus' head, but that there was a slow development in Medieval Times and a gradual transition and shift of values in Modern Times. For example the science of Galileo "springs primarily from the re-establishment of direct contact with the mathematicians and physicists of the Greek Hellenistic Age" (p. 271). Or, "the empiricism of the English Franciscans at Oxford, of Ockham and his followers, has descended in a direct line through Hobbs, Locke, Hume, and the Mills, to the modern scientific empiricists of Cambridge and Vienna" (p. 20). The connections are profuse and their implications convincing. Prof. Randall's profound knowledge of the Greek tradition and of modern history of civilization adds depth to his analysis of the various schools and theories, and

makes history of philosophy make sense.

As any other book on history of human thought, *The Career of Philosophy* raises questions, evokes controversies, and stirs old and never-ending discussions. I could not agree with Prof. Randall that Locke was Cartesian except in his early years but I am impressed by his emphasis on Newton's role in the history of scientific thought. I believe Randall has omitted Rousseau's complete reversals in social philosophy and theory of education. The first came about in Rousseau's suggestions to the Polish political reformers, the second in Rousseau's book on education of women.

RANDALL's volume has its weak and its strong points. The chapters on Bruno, Galileo, Newton and Spinoza are written in the best tradition of the great art of historical writing. It was Spinoza whom Unamuno has called the "tragic, sorrowful Jew of Amsterdam." Randall sees him in a different light. "For under a rather drab exterior there burned the inward glory, the calm clear light of a mind that has looked upon the very face of God, and in the knowledge and the intellectual love of God found peace and blessedness" (p. 435).

To Spinoza the logical order of nature was the moral order of nature; Spinoza's *Ethics* had to be evolved "more geometrico." In his times mathematics was "the only safe science," but it was Kant who ascribed infallibility to mathematics. Randall counterposes the English empiricism to the German rationalism. "The empirical tradition has proved ineradicable in British thought; the rationalistic tradition, with its emphasis on relations, concepts and systems of laws, has been characteristically German . . . In the present state of physical science this British preference for the substantial and concrete is having a hard time of it" (p. 584). One may add—it has a hard time also in psychology.

STUDENTS of psychology who have some interest in historical and philosophical foundations, can't help marveling at the miraculous development of modern psychology. On one hand contempo-



rary psychology is a direct and legitimate heir of the French sensationalists, such as Cabanis, the physician interested in physiological psychology, La Mettrie, the man to whom men were machines, and Holbach, who applied the laws of mechanics to men. The impact of English and French associationists such as James Mill, D. Hartley, E. B. Condillac, and others is easily discernible in the works of Thorndike and Bechterev, Hull and Pavlov, Skinner and Bykov. Yet the Kantian admiration for mathematics and the neopositivistic love for conceptualization inspired K. Lewin and C. L. Hull. The impact of Kant and Wittgenstein is felt in practically every aspect of psychological research and experimentation.

It seems as though psychologists have been mostly influenced by Hume's radical empiricism and his love for mathematics. I could hardly agree with Prof. Randall that "Hume stands for all time as the antithesis of Spinoza in his thoughts" (p. 630). It seems to me that Kant represents a strange combination of Spinoza's ideas with Hume's radical empiricism. (An excellent criticism of Hume's method was written by Randall in *Nature and Historical Experience* and by MacIver in *Social Causation*.)

For a while psychology was historical and antiphilosophical; it is no more. Presently we are witnessing a most encouraging and rapidly growing interest in historical and philosophical roots of psychology. As a young science psychology was perhaps self-centered and narcissistic. As a mature science psychology looks around in search of its own historical and philosophical origins, and tries to establish a relationship with other disciplines and with the body of human thought in general.

Randall's book, as a profound contribution to the history of science and the history of human thought, is of great value to whoever seeks to comprehend the ways sciences grow and interrelate.



*Historical continuity with the past is not a duty; it is only a necessity.*

—OLIVER WENDELL HOLMES, JR.



## Fine Boat, No Rhoin

Gerhard Nielsen

*Studies in Self Confrontation.* Copenhagen: Munksgaard, 1962. Pp. 221.

Reviewed by LEONARD D. ERON

*The author, Gerhard Nielson, works at the Psychological Laboratory at the University of Copenhagen. The review further identifies him. The reviewer, Leonard Eron, is Professor of Psychology and Director of Clinical Training at the University of Iowa. After receiving his Wisconsin degree, he taught at Yale University and in the Smith College School for Social Work. Then, from 1955-1962, he was Director of Research at Rip van Winkle Foundation, where he conducted research on the assessment of children and their parents, concerning himself especially with aggression in children and the ways in which it is related to parental behavior. He is currently completing a book with Joseph Zubin and Florence Schumer on assessment techniques in abnormal psychology.*

THIS slim, paper back volume (including a 20 page summary in Danish) is a description of the ingenious assessment procedures devised by the Danish psychologist, Gerhard Nielsen, when he was a visiting scientist at the Harvard Psychological Clinic in 1957 and 1958 and again in 1961. Indeed it is a charming account, spiked throughout with references to the Old Testament, Cicero, Ovid, Darwin, James, Freud and Robert Benchley. Included is a theoretical section presenting Nielsen's orientation to the phenomenology of the self. However, one need not subscribe to the tenets of the "emergent self" to appreciate the descriptions of the behavioral reactions of the Ss to the dramatic self confrontations. Most delightful and informative are the verbatim excerpts from interviews between the Ss and E

subsequent to their viewing sound movies of themselves in stressful interaction with a stooge. This was a part of a more elaborate investigation of dyadic interviews reported in the address by Henry Murray on receipt of the 1961 APA Distinguished Scientific Contribution Award. For this specific study each of the Ss (22 normal Harvard undergraduates being studied intensively at the Clinic) spent several weeks writing an essay on his personal philosophy of life. He was then told he would meet another S for a discussion in which they would challenge and defend their respective philosophies. The other S was actually a skilled lawyer who met with each of the Ss under special instructions to attack S, challenge his philosophy, point out inconsistencies, and make him alter or withdraw his statements. Twelve minutes of this dyad were recorded on sound movies which constituted the self confrontation when S viewed the playback; once ostensibly by himself (he was being observed all the time); once with E who interrupted the film at critical points, asking S the significance of certain movements, gestures and speech habits, how he actually felt at that moment, and having him free associate to similar events in his childhood history; and once, a year and a half later, when he tried to recapture his feelings at the time of the stressful interchange.

FOUR analyses of these data are reported. The first had to do with the proportion of time each S spent looking at himself during the first viewing of the movie, which was recorded by an observer looking through a peep-



hole in the screen. This behavioral measure, as close to the classical concept of narcissism as you can get without a pool of water, was correlated with two ratings of narcissism made by *E* on the basis of interviews about the experience and one made independently on the basis of clinical interviews and psychological tests. Rank order correlations between the criterion and the first two were significant beyond the .01, not significant for the third. These results were taken to indicate that predictions are more valid if based on knowledge of the situation in which the predicted behavior occurs and that the criterion was a particular kind of narcissism, called "cognitive narcissism" (self introspection or self concern) rather than "behavioral-material" narcissism which the clinical evaluation was more likely to pick up. Although no value of *rho* is actually mentioned, the scatter plots of all three correlations are presented. This is the first and last time (p. 56) that there is any indication of a correlation, significance test, or any of the other trappings one usually expects to accompany a presentation of data in a report of a psychological study. Unfortunately, there is no indication of the reliability of the criterion measure (which would seem highly susceptible to error) nor is there any indication of the reliability, or independence of the ratings of narcissism made on the basis either of the interviews or the clinical material. Although one cannot argue with the strategic decision of the investigators to eschew large scale (in terms of *N* only) studies and the use of electronic computers, the reader should be furnished some measure of confidence in the veridicality of whatever data are reported.

The other analyses deal with (1) the particularities of bodily movements, i.e., the idiosyncratic gestures *S* made when under stress, which were inexorably in each case traced back to childhood traumatic antecedents, although no other possibility was entertained for their origin; (2) the visual behavior of *S* during the interchange, how often he looked at his antagonist and how often he looked away during the discussion; and (3)

response of *S* to a confrontation with the film a year and a half later, at which time he indicated the degree to which he recalled feeling angry with his antagonist and the degree to which he felt he manifested this anger. The latter was the only one of the four studies which yielded any generalization or classification of *Ss*. Four types of "angry men" were formulated in terms of expressing and controlling anger. An example of each type is given but there is no indication of how inclusive and discriminating this classification is, e.g., could each of the 22 *Ss* be placed unequivocally in one and only one of the four categories and how reliable would the classification be?

In general this is a stimulating little book which demonstrates a speeded up method for obtaining retrospective data from *Ss*, similar in some respects to what could be obtained in more lengthy, analytic type contacts. It should also serve as a goldmine for testable hypotheses. The phenomenological freight with which it is laden, and the cavalier disregard for the usual canons of scientific verification of the hypothetical statements that are made, need not obfuscate its potentialities for behavioral research. This is another instance of the valuable methodological contributions stemming from the Harvard Psychological Clinic.

## Eyes on Eggheads

Abraham J. Tannenbaum

*Adolescent Attitudes toward Academic Brilliance*. New York: Teachers College, Columbia University, 1962. Pp. v + 100. \$3.50.

Reviewed by ALEXANDER W. ASTIN

The author, Abraham J. Tannenbaum, is a Columbia-bred social psychologist who is now Associate Dean, Graduate School of Education, Yeshiva University. He has recently been on leave from

that position to serve as Coordinator of Education for Mobilization for Youth, a delinquency control project in New York City. The reviewer, Alexander W. Astin, is a product of the University of Maryland, having received his PhD there in 1957. He spent two years in research on narcotic drug addiction at the USPHS Hospital in Lexington, Kentucky, then moved to the VA Hospital in Baltimore for a while before coming to his present position as Research Associate for the National Merit Scholarship Corporation. Both his history and his self concept make him out to be a research psychologist with a strong orientation toward applied problems.

THIS monograph is one of a series of studies being conducted at the Horace Mann-Lincoln Institute of School Experimentation; the series is concerned with educational problems in the public schools. The major goals of this particular study were to obtain high school students' impressions of the academically brilliant student, and to determine if these impressions are affected by either the "studiousness" or the "athleticism" of the student.

Verbal descriptions of the "brilliant," "average," "studious," "nonstudious," "athletic," and "nonathletic" student were combined to yield eight imaginary "stimulus characters" representing all possible permutations of brilliance, studiousness, and athleticism. Using a list of personal traits which had been judged previously as having either high or low social desirability, students from several high schools rated each of these characters. Mean global social desirability scores provided the main bases for comparing the students' impressions of the eight stimulus characters.

The relationships between the three attributes and the students' ratings were evaluated by means of a 2 x 2 x 2 factorial design, in which each of the eight cells represented a stimulus character who was brilliant or average, studious or nonstudious, and athletic or nonathletic. Because the design did not include a "subjects" dimension (even though all subjects rated each of the eight characters), the number of



significant results is probably underestimated.

Athleticism, which was consistently seen as a positive attribute, accounted for the largest proportion of variance in the ratings. Academic brilliance as a main effect turned out to have little relationship to the students' global ratings. The author devotes most of the discussion to the results from one high school, where brilliance resulted in lower ratings when combined with either studiousness or nonathleticism. However, interactions involving brilliance were very different in the other high school samples.

The rationale given for using imaginary stimulus characters, rather than real students who possessed the required attributes, is that the obtained results would be less ambiguous: "There is less mystery as to what, precisely, is triggering off reactions [to the imaginary character] . . . than in the case of a live encounter between two people" (p. 20). Unfortunately, the verbal descriptions of two of the three traits do not satisfy this desire for unambiguous stimuli. The "brilliant" character, for example, is a "brilliant high school student who is also among the highest in all academic subjects." Thus, the construct "brilliance" is confounded with high academic performance. Similarly, the "athletic" student is "sports-minded and participates in many athletic activities in school." Here, a high degree of interest is confused with participation and (by implication) proficiency in sports. The description of the "studious" student is perhaps the least ambiguous: he "spends more time at home studying school subjects and doing homework than do most students."

Since the general purpose of this study was to learn more about the social status of the academically talented student, one wonders if there was any particular advantage to using imaginary characters instead of real people. Even if one grants that real people constitute more ambiguous stimuli than verbal stereotypes, it is at least possible, if real people are used, to check on alternative interpretations by securing a variety of measures of individual differences in addition to the ones under investigation.

The book is well-organized and clearly written. In his design and implementation of the project, the author runs a refreshing middle course between the blindly empirical 'shotgun' approach and the ritualistic 'hypothesis testing' which characterizes so many

studies of this type. If the reader is willing to assume that the data do provide some information about high school students' attitudes toward their peers, he is likely to find the results of this study both stimulating and provocative.

## Another In-Basket Case

John K. Hemphill, Daniel E. Griffiths and Norman Frederiksen. With the assistance of Glen Stice, Laurence Iannaccone, William Coffield and Sydel Carlton

*Administrative Performance and Personality: A Study of the Principal in a Simulated Elementary School.* New York: Teachers College, Columbia University, 1962. Pp. vii + 432. \$8.00.

Reviewed by WILLIAM E. KENDALL

Two of the three authors, John Hemphill and Norman Frederiksen, are now associated with the Educational Testing Service in Princeton, Hemphill as supervisor on its Leadership Study, and Frederiksen as Director of Research. Daniel Griffiths, the third author, is now Associate Dean of the School of Education at NYU. The reviewer, William E. Kendall, received the PhD from the University of Minnesota and, after a short period at Syracuse University, became Director of Personnel Research for the Chesapeake and Ohio Railroad. Later, as Coordinator of Personnel Studies, he worked for some years for the Standard Oil Company (NJ) at their Aruba Refinery. In 1959 he joined The Psychological Corporation where he is now Director of the Personnel and Market Research Division. He is the author with C. H. Stone of Effective Personnel Selection Procedures.

FOR the uninitiated it should be explained that an in-basket test is a situational test which seeks to simulate aspects of the task of the administrator. The examinee is presented with a collection of items which have presumably accumulated in the admin-

istrator's in-basket and are awaiting his attention. The examinee is given necessary background information and is told that he is the new incumbent of the administrative position and is to deal with the material in the in-basket. The items in the basket are the test items and the actions taken by the examinee with respect to the items in the basket are his responses to the test. Originally, the in-basket test was intended for use in the evaluation of Air Force training programs. The research reported in *Administrative Performance and Personality* represents an attempt to utilize the in-basket test as a device for making observations of administrative behavior, specifically, the administrative performance of the elementary school principal.

The major objectives of the research (pp. 7-8) are stated as: "1. To determine dimensions of performance in the elementary school principalship and thus to develop a better understanding of the nature of the job of the school administrator." "2. To provide information helpful in the solution of the problem of selecting school administrators." "3. To provide materials and instruments for the study and teaching of school administration." In view of these major objectives and in



the light of certain findings to be discussed below, the title chosen by the authors, namely, *Administrative Performance and Personality* is, in the opinion of this reviewer, inappropriate.

The data reported were collected in 1958-59 on 232 elementary school principals (137 men and 95 women) brought together in small groups for a one week testing session. In addition to three in-basket tests developed for this study, participants were also administered the Bureau of Business In-Basket Test, were required to complete such tasks as the preparation and delivery of a ten-minute speech (taped), and to serve as a committee member assigned to the solution of a problem in educational administration. Prior to the experimental test week, each participant completed the Strong Vocational Interest Blank for Men, the Symonds Educational Interest Inventory, the Sixteen Personality Factor Questionnaire, four measures of professional and general knowledge, and tests purporting to measure thirteen "basic mental ability" factors, e.g., deduction, flexibility of closure, ideational fluency, associative memory. Tests for the four fluency factors were from Guilford, while the tests for the remaining nine factors were taken from those assembled by J. W. French.

It would have been of interest to this reviewer and, probably, to all who have access to *Buros' Mental Measurements Yearbook*, if the authors had explained why, other than for reasons of 'factorial purity,' they had chosen the particular tests and questionnaires employed. The authors (p. 80) concede, insofar as the basic mental ability battery is concerned, that meaningful normative data obtained from comparable groups under similar conditions are not available. The descriptions given by Cattell for factor scores of the 16 PF Questionnaire were adopted without qualification. It is relevant here to note that the "appearance of many orderly relationships between personality factors and performance of the principals" on the in-basket test was, in the words of the authors, "somewhat unexpected." There is no mention of treatment of

the Symonds Interest Inventory data. The Strong VIB Scores were handled in the same manner as those of the 16 PF Questionnaire, that is, scores of men and of women are not separated in the analysis. This leads to some dubious interpretations of relationships of SVIB Scales, e.g., (p. 271) with respect to in-basket composite Score A: Exchanging Information; high communication principals are said to "tend toward the feminine side of the masculinity-femininity continuum" ( $r$  with M-F Scale =  $-.20$ ).

After scoring the in-baskets using 68 scoring categories developed for the purpose, intercorrelations were computed among the 40 highest frequency in-basket scoring categories and used for purposes of factor analysis. Eight first-order factors and two second-order factors were identified and "interpreted as basic concepts of administrative performance in the in-basket test situation" (p. 147). The first-order factors were labeled: A. Exchanging Information; B. Discussing With Others Before Acting; C. Complying With Suggestions Made by Others; D. Analyzing the Situation; E. Maintaining Organizational Relationships; F. Organizing Work; G. Responding to Outsiders; H. Directing the Work of Others. The second-order factors were called: X. Preparation for Decision vs Taking Final Action and Y: Amount of Work Expended in Handling the Item. Having identified and labeled these factors the authors treat them throughout the balance of the report as though they were criterion measures, that is, correlations with test scores and personal and demographic data were computed, analyzed and interpreted.

In the final chapter the authors discuss the implications of their research for selection of administrators. They observe, for example, (p.337) that correlations between ratings of principals and personality questionnaire scores were "too low to be of much value in selection." However, when the "unique components of the eight factors were used as criterion measures . . . very interesting patterns of relationships with personality test scores were obtained for

each of the eight factors." The authors illustrate how these interesting patterns of relationships might be utilized by a school board in selecting principals. Thus, if it were desirable to employ principals who "are proficient at maintaining organizational relationships" 16 PF Scores should be such that the principals are: "1. Friendly, socially responsive; 2. Lively and enthusiastic; 3. Bold, warm-hearted, and spontaneous; 4. Self-confident and accepting; 5. Free from worry and anxiety." If on the other hand, the school board desired principals "whose forte was responding to outsiders" the 16 PF attributes should include "1. submissiveness, modesty, and obedience; 2. simple sentimental naivete; 3. lack of enthusiasm; 4. shyness and timidity; 5. persistence and stability; 6. lack of anxiety."

Similar suggestions are made for the use of the mental ability tests and for the SVIB. The authors tell us, for example, "Principals of high general mental ability are characterized more by preparation for decision than by taking terminal action" and that high work output is "typical of high mental ability principals." They suggest, "If a district determines the factors it wants in a principal," SVIB "scores might be of some help in making a choice." Thus, interest scores could help to pick a principal high on Factor A; Exchanging Information, since persons high on Factor A "have interests unlike those keyed for policemen, but positively related to the interests of city school superintendent, lawyer, and psychologist."

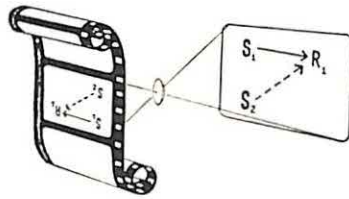
In the opinion of this reviewer, while the in-basket test may have a place as a research device, e.g., in training and training evaluation, the case for the use of in-basket test scores as criterion measures of on-the-job performance is not convincing. This opinion plus a negative reaction to the proposed use of mental ability, personality, and interest scores, leads this reviewer to the conclusion that this volume should never be allowed to fall into the hands of a school board. For the psychologist who is involved in the selection of administrators, this volume, despite its title, will be disappointing.





# INSTRUCTIONAL MEDIA

Edited by A. A. Lumsdaine



## Byways through a FORTRAN Maze

Harry L. Colman and Clarence P. Smallwood

*Computer Language: An Autoinstructional Introduction to FORTRAN.*

New York: McGraw-Hill, 1962. Pp. 160. \$3.95 Paperback, \$5.95 Hard Cover.

Reviewed by CHARLES DENOVA

Colman, the senior author, is a philosopher-turned-computer-man who developed this programmed (?) text—see review—in connection with his responsibilities for training computer-program users during a several-year period spent at the IBM-sponsored Western Data Processing Center, located on the Los Angeles campus of the University of California. Something over a year ago he moved on from UCLA to further responsibilities in the field of computer use at the Armour Research Foundation in Chicago. Smallwood, the junior author, collaborated with Colman while serving as a consultant to W.D.P.C. Denova, the reviewer, is Programmed Instruction Administrator at North American Aviation's Space and Information Systems Division. Earlier he spent six years at General Dynamics where, among other things, he had responsibilities for developing training curricula for the Atlas missile. With a BS from Louisiana State University (1951) and a MA from San Diego State College (1958), he is, in addition to his duties at North American, currently working toward a doctorate in the field of programmed learning at UCLA.

PROGRAMMED instruction has introduced to the publishing world startling new formats, books with zebra stripes, left-hand pages printed upside down, and even, to the horror of some, half-empty pages. This book, an introduction to IBM's algebra-like language for programming digital computers,

boasts a format as unfamiliar to most autoinstructional programmers as it is to most publishers. In the layout designed by Elizabeth Paine, the text information is printed inside blocks or frames and presented as a data flow-chart. Frames are coded according to the kind of information contained inside. (See Figures 1 and 2.)

The stated intent of each page design was "to attract attention" and "to enhance retention." The reader follows the frames and arrows in a variety of eye patterns—vertical patterns, horizontal patterns, diagonal patterns, O-shaped patterns, U-shaped patterns, Z's, N's, S patterns, and pain patterns. (Some readers may find this text helpful for eye muscle exercise, acquisition of FORTRAN becoming incidental.)

The reader receives information on FORTRAN programming in small chunks. This presentation format, small bits of information, may have encouraged the authors to state that "it is based on B. F. Skinner's reinforcement theory of learning." While this may be the vogue in the writing and marketing of programs, any similarity to standard linear programs is, in this book, hard to see. Skinner has never had a no-response mode.

Fig. 1. An Introductory Page on FORTRAN

### To the Reader

This text is an autoinstructional program. It has been written and designed to provide an efficient, self-contained system for learning the basic concepts of Fortran programming.

The subject matter is presented in short, concise increments printed inside a frame. The type of frame denotes the kind of information inside it.

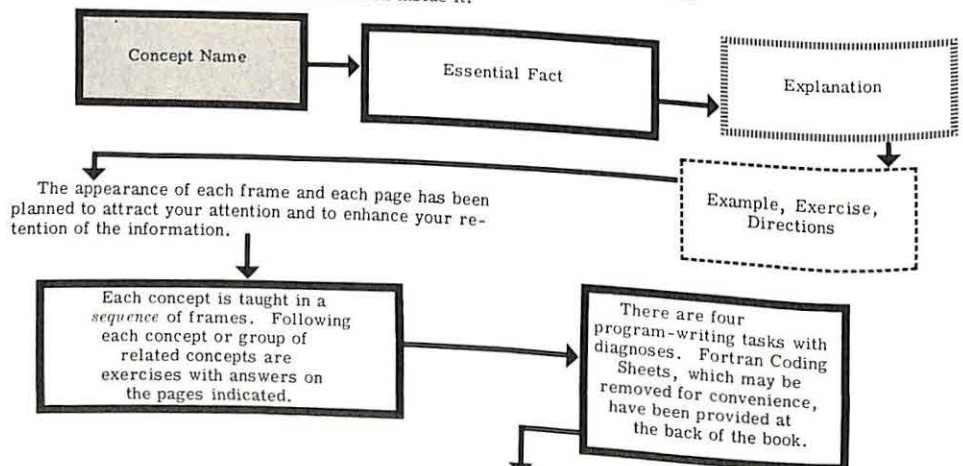
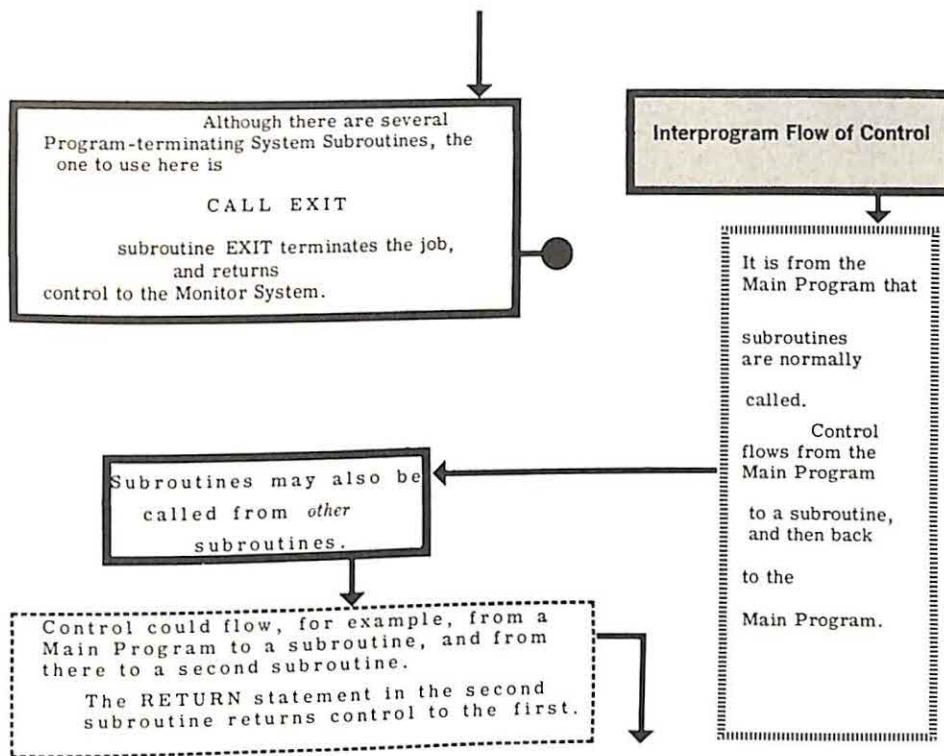




Fig. 2. Sequence on FORTRAN Subroutines



The authors' statement that their text is autoinstructional would imply that the text is designed to provide an efficient, self-contained system for learning. As commonly understood, it would also imply that the reader is required to respond, either overtly or covertly, to each step forward and, upon responding, receive immediate knowledge of results. There are, indeed, questions and exercises at the end of each sequence (sometimes a complete chapter in length), with confirmation provided by hunting for the answer in a randomly-scrambled confirmation section in the back of the text. According to the authors, "the exercises were added, not as instructional, but as a means of building confidence." The authors admit these differences from common practice and cite "experiments, particularly by Arnold Roe at UCLA [which] indicate that under some circumstances, if the overt response and verification are omitted, the result is a program of instruction that is equally as effective and significantly more efficient." The question remains whether this is a program or whether it belongs in the realm of textbooks.

The objectives of the program, as stated in the preface, include "an introduction, not only to FORTRAN, but also to the art of computer programming," both for those who intend to use a computer and for those who merely wish to familiarize themselves with the rudiments thereof. The target population is broadly defined as "managers and administrators who need a basic acquaintance with the concepts and terminology of computer programming and high school and college students in mathematics, statistics, engineering, and business administration." The authors admit that "the program has never been experimental," but report that informal trials of early drafts led to extensive revision. Time to complete the program is given as two to ten hours. Students are assured that the program requires no special background knowledge and are advised to "concentrate on the Essential Facts and skim over the Explanations and Examples" if the subject matter is easy to understand.

A SMALL SCALE evaluation of the program's effectiveness was conducted using three groups, each consisting of two

individuals. Each pair comprised one member who was an electrical/electronics technician with computer experience but no experience in FORTRAN; the other member was an electrical/electronics technician with no computer experience. These individuals were selected on the basis of a review of their background resumés. Two learners were instructed to read only the Essential Fact frames, which constitute approximately 20% of the material. Two other learners were instructed to read those frames that would be of benefit to them and to skip subordinate frames when they felt confident. The remaining two learners were instructed to read every frame. No additional information or instruction was given. The measure of the effectiveness of the program was the students' performance on the exercises included at the end of the text.

The two learners who read only the Essential Frames took less than two hours; one took one hour and forty minutes, the other took one hour and forty-five minutes to complete the program. However, they were unable to perform any of the exercises at the end of each unit. This fact leads one to conclude that the Essential Fact frames are misnamed. They do not include all the essential information necessary to solve the exercises. One member of the pair who read all the frames took eleven hours and fifty-seven minutes; the other took twelve hours and ten minutes. One of the optional-frames learners, who reported he could program a computer, took eleven hours and fifteen minutes to complete the instructional program, while the other took eleven hours and thirty minutes. The latter two students read the less important frames to determine if they should read them, and discovered that some of the less important frames were in fact less important—alas, too late, they were already read. This accounts for the close proximity of time taken by the second and third sets of students. Although the third group read all of the material, they could only work an average of 73% of the exercises correctly. The self-programmed group performed correctly on 71% of the exercises. According to reports from these students, the first design ob-



jective, that of "attracting attention," was reached, but they found the patterns of frames and arrows confusing. They also reported difficulty in remembering the meaning of the different frame outlines. The second objective, that of "enhancing retention," cannot be said to have been attained.

The title of the text indicates that the program material is autoinstructional. With students such as the ones used here, the book is not autoinstructional, in the sense that it can not stand alone. There is a need for outside instruction if students of comparable or lesser background are to use this FORTRAN material.

## I. M. Research Reports

WHEN experimentation on instructional films in the nineteen fifties represented the main stream of instructional-media research—in the largely pre-teaching machine, pre-N.D.E.A. days of *CP's* first four volumes—the *CP* department on *Films* presented summary reviews of several dozen reports of experimental studies on that evidently no longer instructionally pre-eminent medium. Like the current surge of programmed-instruction researches, and the sizable sequence of recent reports on instructional-TV experiments, these film-research studies were mostly based on government-sponsored research projects, with the lion's share of those reviewed emanating from C. R. Carpenter's program of instructional film research at Pennsylvania State University. Now, as then, these contract research reports represented something of a dilemma with respect to candidacy for *CP* review.

In format they are reports, mostly mimeographed or multilithed, mostly staple or spiral bound, some of them of book or monograph thickness, most of them shorter. Some of these reports are essentially surrogates for journal articles, which *CP* does not ordinarily review as such (with rare exceptions for special reasons: e.g., Glaser's review of the Pressey, Skinner, and Ramo papers in the January, 1960 issue).

Some of the research reports are clearly published material, in terms of referencibility if not always of continued ready availability; some are for sale, at the Department of Commerce's Office of Technical Services or elsewhere. Others fail to clearly meet full publication status; and the line is hard to draw (see the notes in the appendix to the Lumsdaine-Glaser source book, pp. 574-579, concerning related problems with respect to availability, and difficulties in the definition of publication status, for many government and foundation sponsored research reports).

Should *CP* review such instructional-media research reports, as was the custom with the earlier film-research reports, or not? The answer is affected by the availability of other journal-publication outlets. At the present time, there are several other sources, in which such research reports are abstracted or summarized, that are rather readily available to psychologists interested in instructional-media research—as well as the sources of fuller reports on instructional-media research mentioned in the October issue of this department. In addition to the information provided by *Psychological Abstracts*, these sources include *Programed Instruction*, published bimonthly by the Center for Programed Instruction (New York City); *AID* (now combined with the *NSPI Journal*, published in San Antonio by the National Society for Programed Instruction, and the NEA-published *Audio-Visual Communication Review*).

Regular issues of *AVCR* contain abstracts of studies on programmed instruction and other instructional media, in addition to periodic batches of abstracts of instructional-media studies conducted under Title VII of the National Defense Education Act, prepared for the Educational Media Branch of the U.S. Office of Education. Five such collections of U.S.O.E. abstracts have thus far appeared in *AVCR*; they are also available as separate booklets from the U.S. Office of Education. Issues 1-5 contain abstracts of 16, 4, 10, 12, and 18 completed USOE-sponsored research studies, respectively, of which 5 deal with programmed instruction and teaching machines, 25 with instructional television, 13 with films, and 18 with other

aspects of instructional media. Not all of the studies abstracted are, of course, experiments in which specifiable variables were experimentally manipulated. In the field of programmed instruction, Wilbur Schramm has recently completed a volume of abstracts of experimental studies, from a variety of sources, which is also being published by the U.S. Office of Education. This volume will be reviewed in a subsequent issue of *CP*.

In addition, summarization of many soft-publication reports (mimeographed, etc.) is being augmented by several of the papers to be included in a second Teaching-Machines-and-Programmed-Learning volume soon to be published by the National Education Association's Department of Audiovisual Instruction, under Robert Glaser's editorship. Also, a greater proportion of completed reports of research are now being published, with less lag, in such journals as *Psychological Reports*, *The Journal of Programed Instruction*, and *The Journal of Educational Psychology*, with corresponding reduction, as compared with prior years, in the proportion of difficult-to-obtain research papers available only as University- or sponsor-issued mimeographed reports.

All of these factors combine to make it less useful than formerly, in the judgment of *IM's* editor, to use *CP's* pages for summary reviews of individual instructional-media research reports, as was done in *CP's* earlier years. Accordingly, with occasional exceptions in the case of substantial book-length research-report collections, *IM* will not resume the earlier practice of publishing instructional media research summaries. Comment on problems and plans in reviewing self-instructional or auto-instructional programs (e.g., see above) will appear in an early subsequent issue.

—A.A.L.



. . . he's often slain that wants to slay!

HOMER  
*The Iliad*





# ON THE OTHER HAND



*In this Department CP invites discussion of reviews and of books reviewed. Here is the place for that kind of intellectual dissent that promotes progress in understanding. Let your criticism be ad verbum, not ad hominem. Seldom does a criticism merit more than half the space of the text criticized—never more than equal space and then only when the letter is interesting and well written. CP edits letters when it thinks they should be. Single-spaced letters will be returned for double-spacing.*

*In this space last month the letter by Witkin et al. was longer than CP, except in very rare circumstances, allows. The present response by Zigler is also much longer than can ordinarily be permitted. In neither case, however, could CP find what it regarded as appropriate paths to brevity. CP takes the position now, however, that justice will not be greatly advanced by further airing of the Witkin-Zigler issues, and that the box appearing at the head of this section contains rules that already have snapped back from their severe bending and will govern future editorial procedures for this section of the journal.*

—F. H. S.

## ZIGLER STANDS FIRM

The letters of Proshansky, Witkin et al., and Korchin (CP, Sept. 1963, 8, 362f.) mistakenly suggest that a basically unsympathetic attitude towards the work of Witkin et al., as well as personal biases, led to my presenting a distorted overview of their book. As indicated in my review, I have long felt that cognitive-style research could profitably be incorporated within a developmental framework. I therefore looked forward with considerable anticipation to reading *Psychological Differentiation*, which repre-

sents just such an effort. In my review, Witkin's thinking was evaluated solely from the vantage point of his own theoretical aspirations. It was my opinion that a considerable gap existed between aspiration and attainment.

The writers of all three letters take some satisfaction in noting that Werner and I disagree about the value of the Witkin volume. That we disagree is relatively unimportant. What may be important is the relationship of Witkin's theoretical efforts to those of Werner. One is free to deduce this relationship by reading Werner's foreword to Witkin's book or by reading the bulk of Werner's theoretical statements and experimental efforts. I preferred to pursue the latter course.

The charge that I presented a distorted picture of the book by not discussing enough of its contents raises a thorny issue. Unquestionably, different reviewers would choose to focus on different aspects of the book. In the limited space available, it seemed infeasible to present a complete summary of the sort Witkin et al. and Korchin apparently would have wished. Let us turn our attention to some specific criticisms of my review.

To the example I gave of a contaminated measure Witkin et al. responded by describing several stages of research on the mother-child interaction. The fact remains that the major tests of certain hypotheses concerning characteristics of mothers and their children were dependent upon global ratings from interviews with the mothers. Such ratings unfortunately may not be independent of the interviewer's knowledge concerning certain relationships between children's general behavior and field dependence. Had this been the only instance of a possibly contaminated score, I would not have bothered to note it. It is surprising that the authors take such offense at my notation since they themselves point out that the global nature of certain of their measures lend themselves to such contamination. In this instance it would appear that I was simply concurring with the authors' own honest appraisal.

Witkin et al. are mistaken if they believe that they can escape the criticisms raised concerning the validity of their measures by invoking the concept of construct validity. There is little evidence in *Psychological Differentiation* that the authors intended to meet the stringent demands imposed on the theoretician when he makes the decision to utilize construct validity. An important requirement of construct validation is that "A test should not be used to measure a trait until its proponent establishes that predictions made from such measures are consistent with the best available theory of the trait" (Cronbach & Meehl, 1956). I continue to feel that many of Witkin's et al.'s measures were not adequate indicators of the traits being investigated. The problem of interpreting the correlations presented by Witkin is such that I am unable to agree that "the question of validity . . . is not particularly at issue".

The issue of construct validity is also pertinent to Witkin et al.'s indignation at my pointing out certain significant correlations between field-dependence measures and Witkin's verbal measures, even though insignificant relationships are also reported. I am thinking here of Cronbach and Meehl's statement that "Just one finding contrary to expectation, based on sound research, is sufficient to wash a whole theoretical structure away" (1956). Witkin et al. gave no indication in their book that the significant correlations cited in my review were in any way spurious.

Another pertinent aspect of the construct validity issue, and one that appears to have gone unnoticed by Witkin et al., is that "A matrix of intercorrelations often points out profitable ways of dividing the construct into more meaningful parts, factor analysis being a useful computational method in such studies" (Cronbach & Meehl, 1956). It is surprising that Witkin et al. did not factor analyze the measures found to be related to their perceptual index. Such an analysis would have informed the reader whether it is appropriate to attribute the interrelationships between the measures to a single factor interpreted in a particular way. Without this analysis it is difficult to evaluate Witkin et al.'s conceptualization that the covariance existing between measures primarily reflects the cognitive process of decontextualization. Other investigators, who have reported many of these same relationships, have interpreted them quite differently (c.f. Gardner, Holzman, Klein, Linton, &



Spence, 1959; Gardner, Jackson, & Mesick, 1960).

Finally, I would like to draw attention to the statement that "it should be particularly noted that rejecting the null hypothesis does not finish the job of construct validation. The problem is not to conclude that the test 'is valid' for measuring the construct variable. The task is to state as definitely as possible the degree of validity the test is presumed to have" (Cronbach & Meehl, 1956). This point is quite relevant to the general intelligence issue. Adherence to this dictum would have led Witkin to partial out the covariance between his tasks and general intelligence. Witkin *et al.* have attributed to me the view that all of the correlations they reported are due to "nothing but" the common variance that their measures have with general intelligence. This is not the position advanced in my review. There is no argument that over the years Witkin and his co-workers have isolated a factor which has some variance independent of general intellectual functioning. However, the question remains just how much variance on any measure is related to this specific factor and how much to general intelligence. The matter is a simple one. Witkin *et al.* inform us (p. 60) that their "anchor" variable, i.e., the perceptual scores, is related to general intelligence. They then go on to argue that the plethora of relationships discovered between their "anchor" variable and other measures is not due to general intelligence. However, the nature of certain of their dependent measures makes this an extremely likely possibility. The position implied in my review was that if the general intelligence variable were partialled out, the magnitude of certain of the significant relationships reported would remain relatively undisturbed, others would be substantially reduced, while still others would vanish completely.

It is difficult to understand Witkin *et al.*'s sensitivity on this point since it is a basically empirical issue that has frequently been raised by those working in the area of cognitive-style research. For instance Broverman (1962) has argued convincingly that ipsative rather than normative scores be employed in such research. Such a procedure would partial out the general ability factor. While research strategy may have made it inappropriate for Witkin *et al.* to avail themselves of this technique, they have certainly not handled the general intelligence issue by constructing intellectual (Block Design, Picture Completion, and

Object Assembly) and verbal (Vocabulary, Information, and Comprehension) indices and then demonstrating that many of their measures are more related to the intellectual than to the verbal index. Although Witkin *et al.* note Cohen's work (1959) in both their book and their letter, they seem to be completely unimpressed with his findings. For instance, Cohen found that for ten-year olds (the age of Witkin's subjects) the Comprehension and Picture Completion subtests were both loaded on a common factor labeled "Verbal Comprehension II." More important to the general intelligence issue was Cohen's finding that in ten-year olds the three subtests comprising Witkin's intellectual index had correlations with G of .58, .42, and .43, while the three subtests of the verbal index were correlated with G .86, .79, and .74. It thus appears that both of Witkin's indices are significantly loaded on G, the verbal index more so than the intellectual index. The general intelligence issue, therefore, cannot be handled by showing that a score is more related to the intellectual than to the verbal index. The question would remain as to how much of the relationship was due to the G variance and how much to the primary-specific variance of the intellectual index.

In lieu of using ipsative scores, an acceptable strategy would be to employ total IQ as an indicator of G and to include all relationships with IQ in the correlational matrices. If this were done, the readers of *Psychological Differentiation* could assess for themselves the role being played by general intelligence. I was referring to the correlational matrices rather than each individual measure presented in *Psychological Differentiation* when I made the statement that not once was the relationship with overall intelligence scores presented. It is amusing that Witkin *et al.* and Korchin have decided to refute this statement by pointing out that a relationship between Witkin's "anchor" variable, i.e., his perceptual index, and general intelligence was presented on page 60. It was exactly this information, rather than any preformed bias, that led me to make the general intelligence criticism. Actually, a relationship between one of their dependent measures and general intelligence is presented on pages 126 and 127, where we are informed that the measure is significantly related to both the full-scale WISC score and to the Goodenough intelligence scale. Again Witkin *et al.* feel that they have handled the implications of these

relationships by pointing out that the measure in question is more related to their intellectual than to their verbal index. Considering the vast number of correlations reported, anyone interested in the general intelligence issue will be little consoled by this instance in which the relationship of a dependent variable to general intelligence is presented.

Witkin *et al.* have characterized my criticisms of their views concerning their verbal index as being "absurd," and have offered a short quotation in support of this characterization. In defense of my position, readers are referred to Chapter 11, *Verbal Skills*, in its entirety. Throughout this chapter the reader is confronted with the surprising view that the Vocabulary, Information, and Comprehension Subtests of the WISC reflect little more than verbal skills or fluency which are minimally related to the child's level of psychological differentiation. Again Witkin *et al.* should note Cohen's findings which clearly indicate that their verbal index is a better indicator of a child's general intellectual level than is Witkin's intellectual index. It is just this general intellectual or cognitive level that most investigators have used as the major indicator of an individual's differentiation. Lewin and his students, as well as others, have used MA as the overall measure of differentiation. Piaget, too, has been content to use the MA as a rough indicator of a child's level of cognitive development. What should be noted is that in a study like Witkin's, in which all of the Ss are of approximately the same CA, MA and IQ covary almost perfectly. Thus, the best indicators of general psychological differentiation would be those measures most highly loaded on G, i.e., Witkin's verbal index. With respect to the quotation in Witkin *et al.*'s letter, do these investigators really believe that the cognitive processes identified by other investigators as being reflected in Witkin's verbal index do not follow the principles of psychological differentiation as defined in Witkin's opening chapter? If they do, then, as noted in my review, their position runs counter to that of certain of psychology's most respected development theorists.

It is Witkin's inadequate theorizing that has led him to this untenable position. Witkin begins with the classic position that development proceeds from the global to the differentiated and that an individual's degree of differentiation will be manifest consistently across a number of important areas of psychological functioning. He then made a decision to use



the field-dependence scores as his anchor measures of the degree of differentiation. What then can Witkin do when he discovers that these measures are unrelated to his verbal index? Witkin *et al.* chose to disclaim that the verbal index reflects an important component of psychological differentiation. Other alternatives were open to Witkin. He could have asserted that his perceptual index was not an adequate measure of a child's degree of differentiation. In view of Witkin's commitment to the field-dependence dimension, it is understandable why this alternative was not selected. Witkin might also have stated that he had driven the differentiation notion to the point where it broke down and had simply abandoned it, a perfectly respectable procedure. He could then have used the decontextualization construct as the focal point of his interpretations. This would have kept Witkin close to his empirical evidence and would have resulted in a generally satisfactory miniature theory. Very little would be lost since, as noted in my review, decontextualization is the only construct clearly emanating from the differentiation concept. The other constructs mentioned in Witkin *et al.*'s letter are derived from the decontextualization and not from the differentiation concept. A possible exception might be the constructs involved in the individual's controls and defense system. Given the various interpretations of the control and defense constructs, one could hardly argue that they flow unerringly from the differentiation concept.

If Witkin did not wish to settle for a miniature theory, he could have decided to employ a broader measure, e.g., MA, as his major indicator of differentiation and then have examined the relationship to this broader measure of the clusters of variables that both did and did not load on the field-dependence dimension. An examination of these relationships and their changes in the course of development could well result in the construction of a nomological network of considerable sophistication and power. Any of these latter alternatives would mean that Witkin would have to begin the task of refining his theoretical framework.

Concerning theory construction, Witkin seems little aware of the pitfalls inherent in calling the differentiation postulate or axiom an "hypothesis." It is an hypothesis only in the loosest usage of the word since a prerequisite of an hypothesis is that it be as open to disproof as to proof. What possible evidence would be accepted as disproof of such

an accepted principle? Indeed, the differentiation concept is amenable to any and all evidence. What Witkin does not fully appreciate is that the differentiation concept as he employs it is a pre-theoretic assumption. Its utility is to be assessed by that network of constructs which emanates from it. In Witkin's hands it becomes a vacuous concept employed simply as a vehicle for vague metaphorical thinking. Let me make my criticism explicit. In the early stages of theory construction many of the theoretician's constructs tend to be vacuous and much of his thinking metaphorical. The error lies in not realizing that one's theoretical efforts are at this stage, and even worse in allowing them to remain there. The disappointment expressed in my review was in response to the fact that after 20 years of effort, Witkin has been unable to incorporate his field-dependence dimension into any more satisfactory theoretical framework than the loose development approach presented in *Psychological Differentiation*.

It is surprising that Witkin chose to employ a development approach at all since it is largely alien to his commitment to the view that perceptual or cognitive styles remain stable over time. In their letter, Witkin *et al.* refer to their evidence on stability as important data for development thinkers. However, they use the concept of stability in an extremely strange way. What they have actually found is not that a child's perceptual style remains constant over time, but rather that there is some stability with respect to the child's relative position in a group over time. Readers are referred to the figures on pages 128 and 129 of *Personality Through Perception* (Witkin, Lewis, Hertzman, Machover, Meissner, & Wapner, 1954), where we clearly see a significant decrease in the tendency to be field dependent between the ages of 8 and 17. These figures also reveal that, contrary to Witkin's basic position concerning the relationship between psychological differentiation and his perceptual measures, individuals tend to reverse this pattern and become more field dependent in early adulthood. Witkin's findings are of considerable interest to development thinkers; however, as noted at the outset, a considerable gap exists between Witkin's findings in themselves and any adequate conceptualization of them.

One final note is in order. Witkin *et al.* were clearly displeased with my review. This displeasure has apparently motivated them to attribute to me a

much more negative attitude towards their book than I actually possess. I stated in my review, and would like to emphasize again, my feeling that this is an effort of considerable consequence. I do not feel that *Psychological Differentiation* is as methodologically sound and as theoretically sophisticated as the authors do. Perhaps this is the state of affairs that normally exists between authors and reviewers.

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EDWARD ZIGLER  
Yale University

#### CP ERRS ON TV

I have just read in the last number of *Contemporary Psychology*, Miss Margaret Mead's review of a study which my colleagues, Dr. Oppenheim, Miss Vince, and I carried out some four years ago. The review is based entirely on the first four chapters of a book of which the U.S. Television Information Service chose to reprint 3,000 copies to be distributed free of charge. These are summary chapters written for the interested lay public.

When my colleagues and I were asked by the Oxford University Press to give permission for the reprinting of these first four chapters, we did so not knowing at the time that this Television Informa-



tion Office is an information office financed by the commercial television companies; that it aims at explaining and defending the industry; this is probably why the fifth chapter entitled "Suggestions to television producers," a chapter which is critical of the assumptions and practices of programme planners, was not reprinted with the others. My colleagues and I thought that this service was a branch of the United States Information Service; otherwise we might not have given permission for such selected reprinting.

I should like to protest to the Editors that they should have asked Professor Mead for a long review, based on a summary of 52 pages, when the entire book of pp. 513 is readily available and has already been favourably reviewed in *Contemporary Psychology* by E. Maccoby. To cap it all, the journal has given more space to a review of the first four summary chapters than to that of the entire book.

The review contains errors of fact; it also requests additional information (and regrets that this is not available) even though, only a cursory study of the book itself would have shown that we have been very much at pains to provide just that kind of data.

To mention but a few:—

1. Gorer's study, which consisted of a brief survey of adults carried out by a market research organisation, was not a more intensive enquiry than ours, which took four years in all and in which each of several thousand children involved kept a diary for a week and were then questioned and tested for up to seven hours. In addition, we had information about their home background and their school behaviour from the teachers, together with data about their IQ and social background.
2. The study was based on two age groups; but the book also refers to the results of some eleven separate studies.
3. A fact which Professor Mead does not mention is that it is a genuine before-and-after study. All children of two age groups were tested (3,000 in all) in one city before the advent of television, and then all those who had since acquired television were retested a year later, together with a matched control group.
4. We have stressed the child's emotional adjustment as important in determining children's reactions to television. Professor Mead regrets that we

did not consider the psychological dynamics of television addiction—in fact, a special chapter is devoted to a comparative study of addicts and other viewers.

5. Professor Mead criticises the exclusive concentration in our study on one medium. Had she read the book, she would have discovered that our study was designed precisely to avoid this. The children's use of and reaction to television was set in the context of their use and of reaction to other mass media and indeed to other ways of spending their free time. For this reason, children were not told that we were interested in television, but in the way they spent their free time; for this reason, too, the diary preceded the questionnaire and test sessions.

6. The same is true for our study of the arousal of fear and anxiety.

*Contemporary Psychology* is there so that the experienced reviewer reads a book, evaluates its usefulness, also by reference to other studies, and then reports his views to other psychologists. To do so, using only a summary, and an incomplete summary at that, written for a lay audience, seems somewhat out of keeping with the traditions of the journal.

Some blame must attach to your office for considering that these chapters should be reviewed altogether, for not sending Professor Mead the book, and not drawing attention to Dr. Maccoby's review in your journal some years back.

HILDE T. HIMMELWEIT,

*The London School of Economics  
and Political Science*

*Hilde Himmelweit is very correct in her judgment that some blame must attach to the CP office. In fact, a lot of blame must attach. A total of about 500 pounds of editor erred here, so perhaps it is to be expected that the blunder is a large one. Margaret Mead may also regard herself as an innocent victim of CP's fallibility. We gird ourselves for correspondence from her and express the hope that neither of these ladies can stand the sight of blood.*

F.H.S.



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- ALBRIGHT, LEWIS E., J. R. GLENNON & WALLACE J. SMITH. *The use of psychological tests in industry*. Cleveland: Howard Allen, 1963. Pp. 196. \$4.25.
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A quarterly published by UNESCO, the  
UNITED NATIONS Educational, Scientific  
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**Vol. XV, No. 3, 1963**

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*I never come across one of Laplace's  
"Thus it plainly appears" without feeling  
sure that I have hours of hard work be-  
fore me to fill up the chasm and find out  
and show how it plainly appears.*

—NATHANIEL BOWDITCH





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A JOURNAL OF REVIEWS

December, 1963  
VOLUME VIII NUMBER 12

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Contemporary Psychology: A Journal of Reviews is published monthly. The subscription per year is \$10.00, foreign \$10.50, single number \$1.00.

Published by the American Psychological Association at 2210 W. Dallas, Houston, Texas and 1333 Sixteenth St., N.W., Washington 6, D. C.

Second-class postage paid at Houston, Texas and at additional mailing offices. Accepted for mailing at special postage rate stated in section 132.122, Postal Manual.

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# Contemporary Psychology

A JOURNAL OF REVIEWS

*Edited by*

FILLMORE H. SANFORD, *University of Texas*

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**VOLUME 8, 1963**

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# Contemporary Psychology

A JOURNAL OF REVIEWS

VOLUME VIII

December, 1963

NUMBER 12

## A Psychologist Copes with Coping

Lois Barclay Murphy

*The Widening World of Childhood.* New York: Basic Books, 1962. Pp. xvi + 399. 10.00.

Reviewed by ROBERT W. WHITE

Lois Barclay Murphy, here an author, is essentially the same Murphy she was in the November issue of CP where she was a reviewer. She is still at the Menninger Foundation, still the wife of Gardner Murphy, still a productive psychologist, still mother of two and grandmother of more. The reviewer, Robert W. White is well known to CP readers. What may not be well known is the fact that after an AM from Harvard he started his teaching career in 1926 as an instructor in history and government at the University of Maine, and then taught for a while at Rutgers before returning to Harvard for his 1937 PhD. Since 1937 he has stayed mainly in Cambridge and at Harvard, busily at work either in the Psychological Clinic or in the Department of Social Relations of which he was chairman from 1957 to 1962. He is the author of *The Abnormal Personality* (1948, 2nd ed. 1956) (CP, March 1957, 2, 61), *Lives in Progress* (1952) and editor of *The Study of Lives* (1963).

**B**REAKING a false image that impedes further thought can be a major service to science. This book shatters with finality the image, still widespread among psychologists, that there is just

one healthy way for children to behave—unafraid, outgoing, flexibly realistic—and that all departures from this hygienic ideal belong in the inferior category of defense mechanisms. Lois Murphy accomplishes her valuable task by examining the behavior of ordinary children in a variety of situations involving some degree of newness, situations such as visiting the study center or going to a party. When this is done with an eye as sharp as the one usually reserved for sick children, normal adaptive problems come to view in all their variety and complexity; the more so when the observer is sensitive to the child's limited experience and outlook, in contrast to the adult's understanding of the world.

Take for example the preschool child's first encounter with the psychologist and her tests. Three-year-old Brennie fulfills the hygienic ideal: he comes confidently to the study center, talks freely, charms everyone with his smile, and addresses himself zestfully to the tests. Other children are more deliberate, keeping a certain distance while they survey the scene, responding cautiously and selectively to the proffered tasks. Still others require the supporting presence of their mothers and venture little outside this haven

of security. Obviously Brennie is the healthy boy—but is it so obvious? Perhaps he is only the convenient boy who fits the adult's purposes and makes it easy for her to get a "representative score." The child of three cannot know beforehand that the psychologist's intentions are benign, that she will do her best not to bewilder, frighten, or humiliate him; is he not wise to be guarded and to be slow in relinquishing maternal support? Brennie, like a genial cocker spaniel who welcomes friend and burglar with equal joy, may be in for trouble when he meets a storekeeper who is not above short-changing him, an older boy bent on taking his marbles, or a stranger who offers him a ride. Then perhaps he will be thought to need help in overcoming his gullibility, and the theorist can point out that he has overgeneralized the trust response.

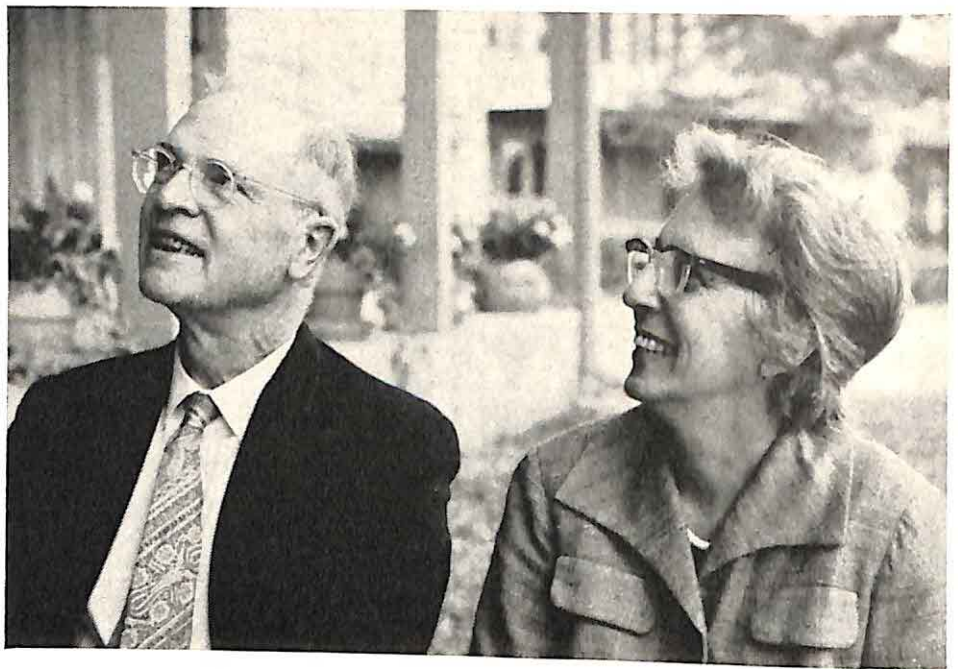
**W**HEN a preschool child confronts a new situation, he requires time to orient himself and become familiar with what is before him. He may stand still, even retreat a bit, while he looks things over. This detached scrutiny keeps him from being confused and allows him to "define a safe area" within which greater activity can be risked. Prominent at this age is the child's need to maintain autonomy and to move only at his own pace. By selecting and rejecting, by assertion here and withdrawal there, by strategic patterns of resistance, avoidance, approach, action, persistence, and the acceptance of help, the child does what he can to reconcile his desire for new experience with his sense of limitation. By means of a rich description of these coping processes we are led to see many facets of the child's adaptive problems; we appreciate as never before the full extent of his effort to



avoid being overwhelmed by the unknown, to preserve pride and freedom of movement, to meet expectations, to satisfy curiosity, and to increase his mastery of the surrounding world. Coping turns out to be far more complex than the use of defense mechanisms, which are, after all, scared short-cuts to safety. The children in this study are not heavily freighted with anxiety. Each in his own way reaches out with eagerness toward the widening world.

Particularly valuable are the studies of coping over the course of time. Here we witness spontaneous progressions from shy hesitancy, avoidance, and self-doubt to a confident and gratifying participation in what is going on. It becomes clear that although adult permissiveness and encouragement can play a helpful part the real crux of this progress is the child's own urge to explore and master his environment and the feelings of efficacy that result from doing so. Most predictive studies of children have erred on the side of pessimism. This may well have happened because inhibitions and avoidances were interpreted as fixed traits rather than temporary maneuvers that would presently yield to a desire for competence.

THE BOOK in which these valuable insights are brought forward is one of a series on the "Coping Project" at the Menninger Foundation. When Mrs. Murphy went to Topeka in 1952 she fell heir to a group of children already studied in great detail as normal infants by Escalona and Leitch (1). The "Coping Project" has continued with 32 of these young Kansans, now approaching adolescence; the present report, however, deals with them almost entirely at preschool age. The chief innovation with respect to method is the presence at each session of a "parallel observer" charged with getting down a running record of "the child's own manner of dealing with pressures and threats, potential or actual." The material here presented is drawn mainly from these "natural histories" of coping. Intended for "a varied audience," aimed to "make available a range of data in its natural richness" while "remaining cautious



LOIS MURPHY AND FRIEND, CHILD WATCHING

about broad conclusions," the book nevertheless has forceful implications for the theory of personality. In the background stand 640 rated variables from the infant and preschool periods of study. Part of this material has been used by Escalona and Heider<sup>2</sup>; more will be analyzed in a future volume. The intercorrelations are here used especially in the last two chapters, where, as we shall see, they strike hard blows at certain aspects of current theory.

First let it be said that a book so heavily descriptive will not suit everybody's taste. The pace is slow, the examples very numerous, the discussions quite loosely organized. Perhaps because of our long neglect of coping behavior we deserve to be drenched in it, but some will feel that a quicker bath would have sufficed. Lois Murphy does not have the quarrelsome, legalistic traits that most psychologists prize; as a result, her hospitality to the ideas of other people at times carries permissiveness to a fault. As presented here, her concepts certainly need more restrictive definitions. Her use of "autonomy," for instance, does not sufficiently exclude general activity, assertiveness, initiative, and even trust, and her conception of "coping" covers all adaptive behavior except the innately auto-

matic and the deeply habituated. The book does not qualify as a tightly argued defense of a thesis, but it was not so intended. Its strength lies in suggestiveness and insight with respect to a neglected aspect of children's lives.

THE theoretical yield, nevertheless, should not be overlooked. In her quiet way Mrs. Murphy has caused a peck of trouble for psychoanalytic theory. She shows how inept it is to interpret separation anxiety as loss of a love object when the anxiety so often turns on feelings of inefficacy without the mother's help. She discloses grave error in the traditional association of passivity, orality, and receptiveness. Active children prove to be zestful feeders; general high drive is likely to include high oral drive. Receptivity, represented in this research by low thresholds of sensory responsiveness, also appears often enough in highly active children. Clearly the activity-passivity variable is in need of much sharper analytic breakdown. The concept of aggression turns out to be in similar hot water. The more active children are not highly aggressive in the sense of being destructive. Those few cases in the series who show persistent destructiveness have histories that include insuperable frustrations.



It seems likely that good coping and well-developed competence tend to make destructiveness less necessary. The study throws doubt on the hypothesized relation between autonomy and the anal stage. Preschool children who are strongly autonomous have already shown the same trait in infancy in relation to feeding; they are the children who as infants most firmly refused the nipple or stopped feeding when they wanted to, and who were given the most freedom along these lines by their mothers. It also comes out that the most successful copers at preschool age are in good tune with the people around them while exhibiting a strong naive sense of pride—a "healthy narcissism." Omnipotence, reality testing, self-esteem and identity all come up for reconsideration in the light of Mrs. Murphy's belief that an urge toward mastery must be counted as an independent motivating force, creating interests of its own and contributing importantly to the growth and integration of personality.

The introduction of vigorous Kansas children into the dark intense atmosphere that sprang from Freud's consulting room can hardly be expected to leave psychoanalytic theory intact. For, as Mrs. Murphy points out, a major difference between these children and clinical cases lies in their tendency "to explore new opportunities for gratification and to use them for growth." This is precisely the aspect of behavior that is not well seen in clinical study and that has been neglected in psychoanalytic theory.

Our author has the great gift of looking at children without letting conceptual black spots dance prematurely before her eyes. She keeps on observing long after other workers, their gaze gone glassy, have succumbed to thoughts about tension reduction and psychosexual stages. Much as this play with concepts may sound like science, it is not *good* science until the observational function has been performed fully, faithfully, searchingly, delightedly, as Lois Murphy performs it.

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## Anxiety and Neurosis: Fact or Fiction

Raymond B. Cattell and Ivan H. Scheier

*The Meaning and Measurement of Neuroticism and Anxiety.* New York: Ronald Press, 1961. Pp. viii + 535. \$12.00.

Reviewed by SANFORD J. DEAN

*The first author, Raymond B. Cattell, received his PhD from the University of London and for six years directed a guidance clinic at Leicester, England, before coming to the United States and to Teachers College, Columbia, where he was a research Associate under Thorndike. Presently he is Research Professor of Psychology and Director of the Laboratory of Personality Assessment and Group Behavior at the University of Illinois. He is the author of Personality: A Systematic Theoretical and Factual Study (1950) and Personality and Motivation Structure and Measurement (1957), (CP, Nov. 1958, 3, 323 f.). The second author, Ivan H. Scheier, took his PhD at McGill University, then taught at McGill and Sir George William College before coming to this country. He joined Dr. Cattell's laboratory as a Research Associate in 1955. The reviewer, Sanford J. Dean, is an Ohio State PhD who taught at Stanford for three years before moving east to Syracuse University where he is presently Associate Professor. His research interests keep him studying personality, mediational processes and verbal learning.*

FOR a number of years Raymond Cattell has been pleading with psychologists to cast off methodological chains and adopt factor analysis as the most appropriate method for probing the complexities of personality. His impatience is aimed not only at the

clinician who never proceeds beyond the vague intuitive hunch based on uncontrolled observation but also at the experimentalist who contents himself with easily controlled but frequently unlikelike, situations. The answer, he believes, lies in the devising of precise tests and the application of multivariate statistical techniques. The factors which emerge from such analyses are the variables with which psychology should concern itself, for they are based on precise, replicable operations and represent actual rather than contrived regularities in behavior. Following his own dictum, Cattell has produced an impressive flow of research data at his University of Illinois laboratory, data which he feels clearly support the utility of his approach.

In his book Cattell and his co-author Scheier report a series of studies designed to give precise definition and meaning to the clinical concepts of anxiety and neurosis through the application of factor analysis. This work is an extension of earlier research on normal personalities and employs the same basic instruments and methods. Considerable new data on pathological populations are reported and integrated with previous findings whenever appropriate. Anxiety and neurosis are examined both as characterological traits and as fluctuating moods or states. Additional chapters are devoted to exploratory work on somatic and physiological interactions, a multifactor theory of anxiety and neurosis, im-



plications for a calculus of adjustment and, finally, the clinical applications of the measuring instruments.

This volume, taken together with previous work by the authors, is the most sophisticated and far reaching attempt yet reported to apply the factor analytic method to the study of human behavior. It is a contribution deserving the serious attention of all psychologists. The case for factor analysis is forcefully and convincingly presented and the results it has yielded are presented in detail.

THIS reviewer's main criticism is that the authors are carried away by their own enthusiasm for and commitment to their method. Perhaps this is as it should be and authors should leave to others the adding of the grain of salt. The fact that factors only reflect relationships among responses causes little concern. For these enthusiasts, factors exist, they are real, and they operate as determinants of personality and of behavior pathology. Some factors are inherited, others are acquired; some are conscious, others are unconscious. It is not always clear which of two related factors causes the other, but further factor analysis will tell. While the desirability of employing factorially defined variables in experiments is recognized, few if any, of the interpretations are based on experimental results.

Some idea of the authors' unbounded faith in their measures may be conveyed by the following example. In discussing the factor profiles that differentiate neurotics from normals, the authors conclude that neuroticism is distinct from psychoticism because the same profile also differentiates neurotics from psychotics. However, psychotics do not differ from normals. "Practically every questionnaire and objective test dimension measured thus far shows psychotics at about the same level as normals. This suggests, for further study, the hypothesis that psychoticism is in many respects functionally closer to normalcy than neuroticism is (p. 114-115). Researchers have been finding no differences between psychotics and normals on many measures for many years. The usual interpretation, however, is that the measures are not adequate

or appropriate, not that no differences exist. The authors recognize that certain factors may have operated with the psychotics which would result in random responding an artifactual flattening of the profile, but this is not seen as bearing on the validity of the first conclusion. However, if the measures on the psychotics are not valid, the comparisons with the neurotics are as questionable as the comparisons with the normals and neuroticism and psychoticism may not be separate and distinct processes.

Considering the authors' critical treatment of single tests developed as operational definitions of concepts without benefit of factor analysis, the correlation between the anxiety factor and the Taylor Manifest Anxiety Scale is of some interest. The correlations of .82 and .85 are the highest reported for the anxiety factor—much higher than the correlations, ranging from .20 to .42, with clinically judged anxiety. Researchers who have been using the Taylor scale now can feel either less guilty because it is factorially respectable, or more guilty because it has little to do with clinically judged anxiety.

THIS is not an easy book to read. Not only is a large array of data presented in a very compact style, but the uninitiated will find that a whole new vocabulary is required. At times the reader will be slowed to a crawl while he backtracks to identify and reidentify variables. While the authors' advance a defensible rationale—that a science of behavior cannot be expected to be simple—it is equally true that complexity and validity are not synonymous and it is unfortunate that many potential readers may be lost when a more fluent style would have served equally well.

In summary, this report of a systematic, continuing attempt to explore personality through factor analysis is highly recommended for all those involved in personality theory and research. It is hoped that some of the variables defined factorially by the authors will be taken up and investigated experimentally. The authors' suggestion that their measures be adopted by prac-

tioners in the clinical setting is likely to go unheeded, at least for some time. The approach is an alien one to most clinicians, many of whom have heavy investments in other methods offering greater intuitive freedom, if not so much objectivity and validity.

## On the Plenitude of Jung

Avis M. Dry

*The Psychology of Jung: A Critical Interpretation.* New York: Wiley, 1961. Pp. ix + 329. \$6.00.

Reviewed by HENRY A. MURRAY

The author is Avis M. Dry, whose interest in Jung was generated more than ten years ago at the University of New England, Armidale, Australia, and pursued during the subsequent phases of her professional training and career at Leeds University, at Bedford College, London, at Glasgow University and at the C. G. Jung Institute in Zurich. The reviewer, Henry A. Murray, is Emeritus Professor of Clinical Psychology, Harvard University. He reports that in the nineteen twenties it was Jung's writings and a few weeks spent with Jung in Zurich that influenced him, as much as any one thing, to change his professional allegiance from medicine and bio-chemistry to psychology and psychoanalysis. It is his feeling, however, that he has been nourished by Jung less in his strictly vocational endeavors than in his life and in the pursuit of his several avocations. His main sidelines, as many psychologists know, involve him intimately with literature, mythology and the present plight of man.

THIS BOOK consists of a remarkably accurate and lucid historical exposition coupled with well-balanced, temperate evaluations of the principal



features of Jung's evolving universe of thought and discourse. Assuming some familiarity with Jung's ideas on the part of her readers, the author has been able to achieve a critical purpose with unequaled specificity and clarity by focusing on each selected feature at a time, taken as it emerged in the course of Jung's incessant flow of published works, and then, after illustrating this feature with ample, apt quotations, by pointing out in what respects it differed from a comparable feature either of Freud's universe or of some other—maybe academic or theological—universe of thought.

The features of Jung's mental produce that the author chooses to distinguish and weigh in the scales of rationality are of various sorts: a stressed aspect of development (e.g. the primary (good and bad) mother-son relationship as compared to the later father-son relationship emphasized by Freud, the hazardous passage of a child from dependency to independency, the value of a *Weltanschauung*), or a concept (e.g. complex, anima, collective unconscious, archetype), or a theory (e.g. the compensatory, prospective, or creative functions of unconscious processes, epigenetic self-actualization extending through adulthood), or a practical differentiation (e.g. between the general statistical knowledge gained by science and the necessity of understanding the unique nature of each individual patient), or a mode of speech (e.g. metaphorical, excessively molar, hazy) and so forth. In choosing among such features, the author seems to have been especially attracted to whatever was most distinctive (in relation to other theories of personality), most indicative of background influences (social, intellectual, theological), most consequential (in determining Jung's drawing power for certain types of people), or most criticizable (from a leniently scientific point of view).

ALTHOUGH she refers to Meier's statement regarding the close connection between Jung's personal life and his ideas, the author refrains from fishing in those teeming waters without the license that she might have gained had she known the Wise Old Man of

Zurich or become a fully initiated Jungian. There are no indications that the author pretends to be anything else than a judicious and disciplined outsider who deliberately and scrupulously made her way through the fiery furnace of Jung's imaginations, and through the broil of the Jungian-Freudian schism, without involvement of her hypothalamus or limbic system, and, like Shadrach, Meshach, and Abednego, came out unsinged, to perform, at a head temperature of about 97.6°F, as nice and even-handed an anatomical dissection as one can find of the complex convolutions of a highly productive intellect.

The distribution curve of the multiple written appraisals of Jung's ideas is certainly bi-modal, with the negative left-hand pole of the continuum marked, say, by the attempted theory-and-character assassination of Edward Glover (e.g. "a mish-mash of Oriental philosophy with a bowdlerised psychobiology"), and the right-hand pole by the genuine homage of Erich Neumann (e.g. "the grandest attempt yet made to construct a theory of the psyche"). Although these extreme judgments and a long array of others, pro and con, are candidly presented to us, the author herself cleaves to an as-yet unoccupied position at the center of the continuum, or more exactly—since there is no word of praise for any part or quality of the twenty or more substantial volumes put into circulation by the cortex she dissected—somewhat to the left of center. About the furthest to the right that this cautious assessor allowed herself to inch is represented by her saying: "Being *ourselves fairly* favourable in our *attitude* to Jungian therapy, we may in fairness add a less (actually far less) favourable interpretation by a neo-analytical writer, Clara Thompson" (italics and interpolation mine.)

In summary I would say that of all the available critiques of Jung's work, this one is the most satisfying to rationality and the least provocative of extraneous affects, and so perhaps the fittest for American psychologists, students and practitioners, the majority of whom, proud as they may be of their standing on the F scale, are, for one reason or another, profoundly preju-

diced against Jung. An inexhaustible horn of plenty, a trough at which unconscionable plagiarists are wont to feed, Jung remains an isolate outside the pale of the Establishment, as Melville did for long enough, and hence his name is seldom mentioned nowadays or seldom without a bite. This book may make a difference.

## Performance and Potential

H. Alan Robinson (Compiled & Ed.)

*The Underachiever in Reading: Proceedings of the Annual Conference on Reading held at The University of Chicago, 1962.* Chicago: University of Chicago Press, 1962. Pp. v + 198. \$3.50.

Reviewed by ROY A. KRESS

The author, H. Alan Robinson, received his EdD from NYU, has served as Reading Supervisor in various high schools, then taught at NYU and at Hofstra College before going to the University of Chicago where he is now Assistant Professor in the School of Education and Director of its Annual Reading Conference and Workshop. The reviewer, Roy A. Kress, a Temple University PhD, has taught school, served a term in the Marine Corps, done a stint as training officer in the VA program, taught for a while at Temple, served as Educational Director at the Shady Brook Schools in Richardson, Texas and worked as Director of the Children's Research Foundation at the same place. In 1958 he went to Syracuse University as Director of the Diagnostic and Remedial Services at its Reading Center but in September, 1963, went back to Temple as Professor of Psychology and Director of The Reading Clinic.

THIS PAPER-BOUND volume, at very reasonable cost, will enlighten many a psychologist who has found himself



embroiled in the current controversy about reading. Offering no panacea for the solution of reading problems or even a suggestion as to any one group or technique which might have been to blame for the numbers of children retarded in reading, the book attempts to place the responsibility squarely where it belongs: on accurate identification of *all* of the factors which have contributed to the *individual* child's present inadequate performance in reading.

The contributors to this volume include such outstanding educators—some of whom are psychologists—as Constance M. McCullough, San Francisco State College; Helen M. Robinson, University of Chicago; Mary C. Austin, Harvard University; Samuel A. Kirk, University of Illinois; and Gertrude Whipple, Detroit Public Schools. Their papers present a realistic discussion of the characteristics of the underachiever in reading, suggest techniques for a differential diagnosis of individual cases and practical recommendations for programming. Most refreshing is the specific definition of the underachiever in terms of performance *vs* potential and the classification of these children into five well-defined categories: (1) retarded reader, (2) slow learner, (3) bright underachiever, (4) reluctant reader, (5) socially and culturally deprived reader. The chapters are sectioned within these categories for discussion of the classification, diagnosis and treatment of each of these identifiable groups. The book suffers from the usual redundancy found in symposia and proceedings when there is over-lap of causal, testing and treatment factors among the contributing authors. However, this does not detract appreciably from the solidity of its content and at times the differing modes of presentation serve to clarify and reinforce ideas.

**I**N ADDITION to their interest in the emphasis upon sound clinical techniques and practices recommended throughout the volume, psychologists will be particularly intrigued with the potential use of *The Illinois Test of Psycholinguistic Abilities* for differentiating "the slow-learning child whose retardation in intelligence and in reading can be

ascribed to a subcultural factor *from* (italics mine) the child whose retardation is the result of special psychological deficiencies" as suggested by Samuel A. Kirk. Also psychologists will be interested in the scattergram technique employing measures of reading achievement and scholastic aptitude, a technique suggested as an objective screening device for identifying the underachievers most in need of further diagnosis and treatment. This is contained in the last section of the volume which is devoted exclusively to suggestions to administrators for in-service training and for the mobilization of a total school effort toward prevention and remediation. School psychologists will find suggested here many sound practices that can aid in structuring recommendations to their

administrative officers and/or Boards of Education. There is value also in the list of standards, prepared by the Writer's Committee of the Detroit project, for the writing of Pre-Primers to be used with culturally disadvantaged children.

Finally, the volume is well indexed, both by author and by subject, to aid the hurried reader, and includes an appendix which contains a comprehensive source list of tests and instructional materials.

Robinson's effort is a valuable contribution to the fields of both Reading and Psychology. It could well serve as a basic text for an introductory course in the analysis of reading difficulties or for supplemental reading in the clinical orientation of school psychologists.

## Rituals or Explorations?

Warner Muensterberger and Sidney Axelrad (Eds.)

*The Psychoanalytic Study of Society: Volume II.* New York: International Universities Press, 1962. Pp. 317. \$7.50.

Reviewed by DAVID GUTMANN

Warner Muensterberger, one of the editors here, received his PhD in 1938 from the University of Basle, then went to the Royal Institute for the Indies in Amsterdam where he was a Research Associate. Later he became lecturer in Anthropology at Columbia University and is presently Clinical Assistant Professor in the Department of Psychiatry at the University of New York's Downstate Medical Center. Sidney Axelrad, the other editor, received a DSSc from CCNY. He was formerly a Research Associate with the New York Training School and did supervisory work for the New York Bureau of Child Welfare. Since 1950 he has served as Chairman of the Department of Anthropology and Sociology at Queens College. David Gutmann, the reviewer, has a PhD from the University of Chicago's Committee

on Human Development, and has held internships at the Illinois Neuropsychiatric Institute and at Michael Reese Hospital Psychiatric Institute. He spent two years in the Boston area as a staff psychologist at the Massachusetts Mental Health Center and taught, with David Riesman and Eric Erikson, at Harvard. Since 1962 he has been Assistant Professor of Psychology and Senior Staff Psychologist at the Psychological Clinic at the University of Michigan. His major research has been concerned with the psychology of the aging process.

**T**HIS BOOK is a rather loose aggregate of articles by psychoanalysts and Freudian psychologists, the second in a series of such volumes. The contents reflect a fairly orthodox psy-



choanalytic approach to such issues as the social development of the individual, the nature of the creative process, and thought and behavior in various cultural settings. As such, it is another statement in a continuing dialogue between theoretical psychoanalysis and theoretical anthropology. The conflicting positions are neither as simple nor as polarized as I shall present them, but I suggest that sociologists and anthropologists are wont to see the individual as a rather passive extension of society, as culture writ small, while the conservative Freudians would see the social realm as the psyche writ large. Each "side" claims primacy and dynamism for its favorite construct: thus, for some anthropologists—notably the British—personality is not a system in its own right, but only the concrete metaphor of an overarching cultural reality—one which structures the inner and outer universe, telling the individual what to value, how to value it, assigning the very categories in which he thinks and feels. In their turn, die-hard Freudians see social institutions—such as church, government, the law—as collective representations of intrapsychic structures (the Id, Ego and Superego) and of parental figures: the Good and Bad mother, the arbitrary father.

Interprofessional rivalries are reproduced theoretically: for the sociologist, the individual is refractory stuff to be "socialized," e.g., coerced into social collaboration through shaming and sanctions. And Freud, spokesman for the Id, saw society, with its impersonal purposes and collective necessities, as the antagonist of man's spontaneous nature.

There are, of course, notable exceptions on both sides: Margaret Mead, the anthropologist, has detailed with great psychological acumen the ways in which cultures communicate basic themes to the infant via its "somatic understanding"—through its skin, mouth and eyes as it is handled and fed by the mother. And Erik Erikson, a psychoanalyst, sees ego structure as partially reflecting the socio-cultural configurations that confront the ego at various developmental crisis points.

These are hopeful ventures; perhaps some day the personal-social dichotomy will be broken down, and both will be viewed from a new theoretical perspective, as part of a superordinate reality.

If such be the way of progress, then the work reviewed here represents a "reactionary" or counter-revolutionary step, for, in most of the included articles, the traditional psychoanalytic approach to phenomena is restated, and the dialogue between the two major approaches to Man is not much advanced. True, the interest has shifted to topics of current interest, such as "creativity," and the development of the self, but these are dealt with from the familiar, "Id-centered" standpoint. Schmale, for example, discusses maturation of structures, both psychic and organic, from a "deprivation" perspective: . . . "when the organism outgrows its sources of immediate supply, it is forced to change in organization in order to continue functioning with and in the immediate environment" . . . and Muensterberger relates creativity to fetishism and the denial of castration. For both these theorists the organism, no matter how integrated and complex its activities, is ultimately seeking surcease from painful tensions; the possibility that the creative person—and the maturing organism—might be seeking optimal forms of stimulation (the view advanced by psychoanalytic ego psychologists) rather than complete surcease, is not much considered.

The same Id perspective is explicit in many of the discussions of cross-cultural phenomena: Geza Roheim, a noted psychoanalyst, demonstrates the open emergence of oedipal, incestuous themes in the play of children from the primitive bands of Central Australia; Abel describes the emergence of a parallel range of instinctual themes in the dreams of a Chinese analysand; and Boyer deduces the oral personality of shamans from their ritual practices.

These are interesting findings and speculations, but they sometimes left me feeling that I had been participant in a ritual, rather than an exploration—a ritual wherein the correctness of

Freud's vision of universal libidinal themes and symbols was once again demonstrated. Culture becomes a complex code to be cracked, but always for the same monotonous message—infantile deprivation, the phallic Mother, the Castrating Father. The psychological exploration of society is finished once the local form of the oedipus has been unearthed, when the familiar ikons are glimpsed through the mist of alien tongues, rituals and myths. Thus, I was excited by the "aliveness" and richness of the Roheim data (and I am grateful for this introduction to his work), but the treatment of Shamanism left me with a strong "so what?" feeling. For purposes of psychotherapy it is important to establish the patient's level of psychosexual fixation, but Boyer has not demonstrated the value of this approach in cross-cultural analysis.

In this devoted attempt to relate peoples in terms of their shared Id interests, much sense of their uniqueness is unavoidably lost. What is worse, the hypothesis of universal strivings leads to a fixed interpretive set, of the "snake equals phallus" variety, so that any hypothesis in this area can easily become self-confirming. This is not a sentimental plea for the preservation of cultural uniqueness, but a methodological criticism. The reality of the human unconscious and its reservoir of instinctual strivings has been amply demonstrated. But the orthodox Freudians, perhaps out of nostalgia for the rebellious youth of Psychoanalysis, continue to fight a battle already won. Perhaps the task is not to demonstrate the unconscious as a cross-cultural fact, but to become—as Freud was—naturalists again: to study the unconscious as a system, maintaining itself in contact with other systems within ever unique and specific habitats. Only thus can we establish the evolutionary and adaptive meaning of the unconscious as a psychic "organ."



*History is a graveyard of aristocracies.*

VILFREDO PARETO





# Growth Through Work

George W. Burchill

*Work-Study Programs for Alienated Youth: A Casebook.* Chicago: Science Research Associates, Inc., 1962. Pp. vii + 265.

Reviewed by PAUL BOWMAN

The author, George W. Burchill, received his graduate degrees in education from Pennsylvania State University. For five years he was teacher and counselor in the San Diego City Schools and then, after a period in education at the University of Wisconsin, assumed his present position, in 1962, as Assistant Professor of Education, University of Redlands. Paul Bowman, the reviewer, worked up an interest in psychotherapy during his graduate work at the University of Chicago and then practiced and taught psychotherapy at Chicago and at the University of Louisville before returning to Chicago to direct a ten year community study of youth development. The first of two volumes to come out of that study, *Growing Up in River City*, was recently published by Wiley and was reviewed in October (CP Oct. 1963, 8, 410). Another volume from the same study is on the way. Since the summer of 1962, he has served as Director of the Department of Prevention of the Greater Kansas City Mental Health Foundation. In his present job and in various sub-studies of the River City project he has gathered experience of many kinds with disturbed and delinquent youth.

THE SIGNIFICANCE of this book lies as much in its assumptions and its reflections of historical trends as in its actual content. Its major concern is the development of delinquency prevention programs among youth of high school age. A hundred years ago American society probably placed higher value on work than on education. The young nation needed manpower of all kinds, and there were adequate opportunities for all who could work. Children were often recruited for the work force and not in-

frequently were exploited for profit. This led to legislation protecting the child from too much and too heavy work.

We now have come almost full circle to a point where at least some children are being damaged by the lack of opportunity to work. At least this is one of the assumptions on which this volume is based. A second assumption is that the experience of work under proper educational supervision is of itself a maturing and stabilizing experience that can aid in personal growth and reduce alienation from society.

This volume has resulted from the initiative of the Phi Delta Kappa Commission on the Role of the School in the Prevention of Delinquency and has been underwritten by the Ford Foundation. It is called a casebook. It presents descriptions of work study programs for alienated youth in nine different American communities, large and small, and these nine chapters constitute the body of the book. One third of the book is given over to appendices that provide more detailed information about the organization of the various programs. One section reprints an excellent statement on the plight of alienated youth in our society by two members of the Phi Delta Kappa Commission.

There is a stimulating variety of experience reported. One program is in a private farm-home institution, but all others are in public schools. One is operated in conjunction with a hospital. One program involves only 12 to 20 students while two are county-wide, involving 10 to 15 percent of the total school enrollment. Several programs are limited to boys in trouble, several to pre-delinquents or potential drop outs, one to non-college-bound girls, and several to 'slow learners.'

The case descriptions vary considerably in detail and completeness of treatment. Not all of the programs are specifically aimed at alienated youth, but the spotlight is kept on those that do relate to delinquency. In each case there is a very lucid presentation of how and why programs were started, for what students they are intended, and descriptions of actual operation of programs.

THE AUTHOR obviously hopes to interest his readers in attempting work programs in their communities, but he does not resort to a selling line. The reporting is sympathetic but objective—and quite readable. An all too brief Summary chapter is devoted to the implications of these cases for school programs. The author states that "elements are transferable, total structures are not," and utilizes his own personal knowledge of the cases to recommend specific methods of procedure. Two themes are dominant, one the integration of work programs with the regular academic program of the school, and the other the necessity of utilizing people and agencies outside the school in both planning and operation of work programs.

The reader will probably wish for a more coordinated view than he will find here of work experience and its place in education. Even a casebook might well have attempted a more systematic comparison of the different approaches than can be contained in a 6 page summary. The reader will also wish for more research and evaluation of programs. One program has been evaluated by survey methods, and another, just started last year, has a control group design in its evaluation procedure. Since these programs have all been developed in the last ten years and have resulted from the pressure of community need, it is understandable that systematic evaluation has been slighted.

This reviewer heartily concurs in the judgment that school programs for these students need major revision. This volume states the problem clearly and offers specific ideas for a type of curricular modification. The school psychologist, school consultant or concerned citizen should find good use for this source-book in bringing this problem to the attention of his community.





#### BOOKS TO COME

**C**P likes to talk about forthcoming books and about both the ideas and the people involved in bringing books forth. The putting together of paragraphs on bookish events not yet happened gives CP—and maybe even some of its readers—the feeling that it is on top of its job. In such reporting there can be the appearance of great alertness and, if one wants to move a bit further toward delusion, even a currency. Closer to the pleasantly actual, however, is the fact that it is both easier and more proper to deal with the human side of the scholarly enterprise before an author's humanity has been committed to print; after a book is published, it must be dealt with in terms of what is explicitly in it; then the author's motives, frustrations, exaltations, his personal aspirations and attributes all become secondary or irrelevant matters.

CP likes to report on books to come. But it has a hard time gathering the requisite facts. It succeeds in learning in advance about only a small percentage of the books eventually seen as they flow in from publishers, pass across CP's mail table and go out to reviewers. Authors themselves are apparently hesitant to volunteer information about their own future productions. Perhaps they are too modest. Or maybe they suspect themselves of immodesty and do not trust either CP or themselves to represent them as they hope they deserve. Or, more parsimoniously, maybe they are too busy contending with proofs and figures and bibliographies and the normal neuroticisms of authorship. Publishers, one might think, would happily furnish information about books that are maturing within their own systems. Actually, publishers do happily furnish such information, but they seem to pre-

fer to furnish it in meticulously prepared and carefully presented items in catalogues and advertisements. And they prefer to make their announcements after they are absolutely sure that there will be an actual book of a specified size available on a definite date at a certain price. Such announcements are too little, too late, too sere, too safe to serve CP's functions.

**T**HE PROCEDURE for getting information on books to come is to receive from publishers, six months or more before publication, the names of authors who seem certain to produce. CP then writes to expectant authors to get from them the kind of response it thinks it wants. This apparently unworkable procedure has functioned in recent months only in the case of one publisher, four authors (two of whom are paired), and two editors. The publisher is Harper and Row. The authors are Harry Helson, Ledford Bischof, and two Sherifs, Muzafer and Carolyn. The editors are Bert Kaplan and Robert Isaacson.

Harry Helson's book, as anyone might well surmise who heard him in September in Philadelphia or who has read him in the role of recipient of an APA Distinguished Scientific Achievement Award, deals with adaptation level theory, and will appear in the spring of 1964. This book was begun in 1956 but its completion was delayed by such developments as its author's acceptance of the editorship of the Psychological Bulletin, and by his apparent inability to stop doing research. He reports himself as already unhappy with the final manuscript; it's a fair bet that by May, 1964, he will have thought and researched into obsolescence his own brand new book. But only he will know it.

The Sherif and Sherif book, due to appear in February, 1964, is another product of the Sherifs' program of research on the group phenomena of adolescent life. This book, *Reference Groups: Exploration into Conformity and Deviation of Adolescents*, will not only report the accumulating wealth of concrete data on the groups, neighborhoods, attitudes and goals of adolescents seen in their native habitat, but will also aim both at an integration of psychological and social approaches to adolescents and at the achieving of a balanced presentation in which there is a concern for a spectrum of adolescent behavior, rather than a preoccupation with misbehavior. Their friends and acquaintances will bet that between the two of them the Sherifs will carry it all off very well indeed.

Ledford Bischof's book, scheduled by Harper and Row for mid-March, will be a textbook for undergraduate courses in the psychology of personality. But, if its author has his way with it, it will not be just another personality text. Bischof intends this one to be a teaching instrument rather than a source book or compendium. And for undergraduate teaching as well as in other contexts Bischof subscribes to Dorpfeld's dictum "there is nothing more practical than a good theory," for he intends to put strong emphasis on the various personality theories.

The book that Bert Kaplan put together shortly before his recent move to Rice University will contain a series of first person accounts of mental disturbances. Kaplan is convinced that mental "illnesses" are profitably viewed not solely as varieties of awfulness but, more positively and more wholly, as meaningful, organized experiences. And he feels that the disturbed individual himself, although his conceptions may not be always in line with this year's psychiatry, has a vantage point and an intimacy of experience that, reported, can increase understanding of mental disorders. The book, due to appear early in 1964, will contain more than thirty accounts of various kinds of mental disorders, each viewed from within.

Robert Isaacson has put together a somewhat more conventional book of readings. At least its general format is



conventional in that it will contain articles from the literature. But its substance is new. It will concern itself with neuropsychology and will contain those articles which Isaacson is convinced all physiological psychologists should read, reread and know. While he realizes that some of his wheat may be another man's chaff, he feels that he has collected articles, some from relatively inaccessible sources, of genuine significance. And like many another editor of collections of readings, he finds himself greatly surprised if not downright shocked at the amount of work involved in such an editorial role.

For the moment, that empties the folder on books to come. *CP* hopes the folder will fill up again soon. Come to think of it, there is one vastly fascinating item that might now be placed in the folder and taken out immediately to be reported upon: Fillmore Sanford and John Capaldi have put together, for Wadsworth publication in early 1964, three little books of readings in general psychology. This obviously is an enterprise of such great pith and moment that it deserves thousands of words of attention all to itself. Here and now is not the place and time.

—F.H.S.

## Dreams of Utility

Walter Bonime. Foreword by Montague Ullman

*The Clinical Use of Dreams*. New York: Basic Books, 1962. Pp. vii + 343. \$8.50.

Reviewed by BRUNO KLOPPER

*The author, Walter Bonime, is currently Associate Psychiatrist at the New York Medical College, Metropolitan Hospital, New York. He follows the tradition of Karen Horney and he received his training analysis from Bernard S. Robbins. This is his first book. Bruno Klopfer, the reviewer, is in most respects the same fellow CP said he was in September of this year (CP, Sept. 1963, 8, 327). He is a Jungian psychologist who in his varied career had two years of thoroughly Freudian psychoanalysis and a year of work with Jung. In this country since 1934, he has taught at assorted institutions of higher learning and has practiced psychotherapy.*

**CP** printed reviews of two books about dreams in 1959 (*CP*, June 1959, 4, 164; *CP* Nov. 1959, 4, 154) by one of the specialists in the field (Calvin S. Hall), and once more a rather critical review (*CP* February, 1961, 6, 44) under the title, "Dream Kaleidoscope." In comparing the information

in these three reviews with the unusually careful analysis of the present research situation and the background of Walter Bonime, as given in the twelve-page Foreword by Montague Ullman, one gets immediately the impression that this book has the right background. Still, the claims near the end of the Foreword: "Dream analysis becomes an integral part not only of the therapeutic situation but of the daily life of the patient. He catches on to his own style of dreaming, enriching his grasp of the forces at work in his day-to-day living. It is amazing how real and credible the patients are as they emerge from what are in fact no more than brief, sharply etched vignettes of moments in their lives," raise a high expectation. It is, however, amply fulfilled. One paragraph further Ullman says: "Little is assumed or left to the imagination in Dr. Bonime's handling of clinical material. It is as if the analyst were in effect always saying: 'Here is what I do. Here is what the

patient does in return. This is the result which ensues.' In this manner the clinical material speaks for itself."

This statement characterizes Bonime's basically phenomenological approach which establishes the extraordinary value of the book beyond any theoretical differences of opinion. Bonime shares this characteristic with Medard Boss without sharing his theoretical argumentativeness.

**T**HE ingenious organization of the book uses as its background over one hundred dreams of more than a dozen patients (listed in the Dream Index under rather fetching titles), which are used over and over in the carefully organized text (from short references in footnotes to about a dozen different places throughout the book). The text itself has a twenty-eight page Introduction and eleven chapters, which are arranged partly according to theoretical concerns (symbolism in dreams, feeling in dreams, sexuality in dreams, and anxiety in dreams), but mostly along temporal phases of the analytic work (for instance, introductory, resistance, terminal dreams).

In spite of the always present clinical aliveness, Bonime does not avoid critical references wherever they offer themselves. For instance, on page 108: "He had performed what is so often falsely taken for interpretation—the mere identification of individuals or actions, mere symbol-labeling (the onlooker identified as analyst, the action as keeping sober)," or on page 155: "Naturally the presence in the dream of the adult patient in bed with both of his parents in a sexual context, and at a time in his life when he is disturbed by the prospect of marriage, offers rich speculative possibilities for Oedipal interpretation. Such an interpretation would, however, be arbitrary. It would be the application of a construct from outside of this patient's biography, and would result merely in substituting one set of symbols for another. There was a real and immediate problem to tackle—the problem of his unwillingness to fructify a relationship, to give himself to the enhancement of another," or, finally, on page 270: "If one says to the patient



# Summaries of Selected Psychology Titles . . .

## **Pattern and Growth in Personality**

Gordon Allport, Harvard University

"... a profound and stirring commentary on the psychological nature of man, a book of scholarship, compassion, belief, and hope—a volume worthy of the distinguished author and the profession to which he has contributed so much."—from a review in *Contemporary Psychology* 1961, 517 pp., \$7.50.

## **Social Learning and Personality Development**

Albert Bandura, Stanford University  
Richard H. Walters, University of Toronto

A comprehensive text for courses in developmental psychology, personality theory, social learning, and the like, the authors highlight social variables that account for the development and modification of social behavior. *Sept.*, 1963, 344 pp., \$7.00.

## **Theory and Practice of Psychological Testing** 3rd Ed.

Frank S. Freeman, Cornell University

"Excellent text incorporating the latest findings in the field. Presentation of various points of view judiciously balanced."—Professor Boris Levinson, Yeshiva University 1962, 715 pp., \$7.95.

## **Statistics for Psychologists**

William L. Hays, University of Michigan

Geared to the undergraduate student with an interest in psychological research, this important new text requires minimal training in mathematics yet is mathematically modern and rigorous. 1963, 736 pp., \$10.75.

## **Man: A General Psychology**

Clarence Leuba, Antioch College; in association with  
William John, Antioch College

"A thorough and well written text for any beginning student. The integration of biological data into the entire text is most refreshing."—Wm. D. Thompson, Baylor University 1961, 686 pp., \$7.95.

## **Personal and Social Development:**

### **The Psychology of Effective Behavior**

Louis S. Levine, San Francisco State College

Combining relevance to the student's immediate concerns with a valid cognitive frame of reference, Dr. Levine organizes his discussion of the fundamental issues of life around a developmental theory of personality that harmonizes with the sequence set by the life cycle. 1963, 522 pp., \$6.00.

## **Research in Personality**

Edited by Martha T. Mednick and Sarnoff A. Mednick,  
both of the University of Michigan

Articles by some sixty-five specialists offer an overview of leading theoretical positions. Includes a number of significant controversies and a section on approaches to understanding the creative personality. 1963, 672 pp., \$6.50.

## **Children and Adolescents: Behavior and Development**

Boyd R. McCandless, State University of Iowa

Stresses the general principles—the theory—and the research necessary for an understanding of children and adolescents in today's society. 1961, 530 pp., \$6.50.

## **Research Readings in Child Psychology**

Edited by David S. Palermo, University of Minnesota,  
and Lewis P. Lipsitt, Brown University

This rich collection of articles emphasizes experimental studies of child behavior. Methodological problems and approaches are presented in their theoretical and historical context; then the various approaches are related to a wide variety of research areas. 1963, 585 pp., \$6.75.

## **Psychology**

### **Revised Edition**

Delos D. Wickens and Donald R. Meyer, both of the  
Ohio State University

Conveys to the reader an appreciation of psychology as a growing science whose concepts and methods can be applied to practical human problems. 1961, 784 pp., \$7.75.



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that he is really angry at his father, not at the analyst, one deflects his attention from an intensive examination of his activity and feeling in response to the current situation, one comforts him by assigning blame to the past, and relaxes the urgency of his responsibility for the present. The patient must deal with himself as a creature of the present in the life of the present."

IN the Introduction, Bonime distinguishes the elements of the dream (action, individuals, and surroundings) and confronts them with the associative and interpretive activity of both the analyst and the patient, without going any further into theory. His phenomenological emphasis is formulated on page 32: "*Dream symbols arise out of the specific life history of each individual, and it is only from the individual's life history that we can derive the meaning of his dream symbols*"; as self-evident as this statement appears and as carefully as he defends it, his claim on page 37: "... dream symbols, as I view them, neither derive from a collective unconscious nor conform to the requirements of a theory of a universal pattern of psychosexual ontology," is only substantiated in the latter direction. This leads, occasionally, to semantic troubles. The term 'self-concept' is used for the 'role-concept' of the individual (for which Jung chose, with an allusion to antique actors, the term 'persona'). The ego awareness, the integrity of which is so heatedly defended by most patients, and the self-awareness, or self-actualization, which Bonime himself describes so vividly in the dream "Dying and Being Reborn" (page 296 f).

The most valuable contributions can be found in Chapters 6, 8, 9, and 11. In Chapter 6, on pages 218-225, we have the most detailed description of a short period of a few therapeutic hours and the role played by dreams, associations, and interpretation. This yields, incidentally, one of the examples where Bonime's humbleness and honesty come out in an aside "... (sarcasm is not advocated as a standard part of the psychiatric armamentarium) ...". Following this episode is an interesting discussion of the problem of 'falling in love

with the analyst,' (page 227).

In Chapter 9, we can choose the discussion on page 275ff., under the heading "You Hold Me Close," as an excellent example of Bonime's thoroughness. Finally, all of the thirty-two pages

of Chapter 11, "Working with Dreams in the Therapeutic Situation," can serve as an example of Bonime's way of working, which certainly does not need any additional comment to recommend it.

## Poet on the Couch

K. R. Eissler

*Goethe: A Psychoanalytic Study 1775-1786.* Vols. I and II. Detroit: Wayne State University Press, 1963. Pp. vii + 1538. \$35.00.

Reviewed by HANS A. ILLING

*The author, Kurt Eissler, is a practicing psychoanalyst in New York City. He is the author of Searchlights on Delinquency: New Psychoanalytic Studies, Leonard da Vinci, On Hamlet, and Psychoanalytic Notes on the Enigma. The reviewer, Hans Illing, is presently psychologist at the Hacker Clinic in Beverly Hills, Calif., and Psychological Social Worker with the Parole Adult Clinic of Los Angeles. In connection with his graduate work at the Friedrich Wilhelm Universitaet in Berlin he did dissertation research on the psychological premises of Goethe's Wilhelm Meister. His numerous subsequent writings have included several earlier reviews for CP (Mar. 1961, 6, 80; Sept. 1963, 8, 352.).*

IT IS NOT difficult to see at first glance that Eissler has produced his magnum opus in *Goethe*. No matter what a reader's differences with Eissler may be, he will have to accept the author's claim that he has worked on this "Study" since 1948, and that he had "to step into the role of a *Goetheforscher*." Eissler is aware "that in going beyond my function of psychoanalyst I am liable to the criticism of the experts in the field."

This expression of difference leads Eissler (and the reviewer) to the important question: does a psychoanalyst have to be a literary critic or *Goetheforscher* in order to write about this man of letters? Eissler served his ap-

prenticeship (*Gesellenstueck*) in this special field when he wrote a psychoanalytic study of Leonard da Vinci. Admittedly, compared with Goethe, little is known about the Renaissance artist and his life; some of his works have been lost or are moldering away (like his "Last Supper"). However, the very fact that Eissler was able to write a masterly study of Leonardo indicates that the analyst can deduce or "interpret" from "between the lines" what the layman cannot. (Eissler himself makes reference to his one and only famous predecessor in such analytical studies, Marie Bonaparte, the author of *The Life and Works of Edgar Allan Poe*). Therefore, it seems to me that Eissler does not have to be defensive (as he admits to be) about his limited knowledge of the *Goetheforschung*. If anything at all, Eissler is overly modest, when he states that he does not "feel responsible for the proper handling of the Goethe-literature." His 562 item bibliography, however, suggests a vast, encyclopedic knowledge, in which few can match Eissler. In going through the bibliography, it would seem to me that most of the important biographers and literary critics of Goethe are included, and Eissler makes frequent use of these sources in his text.

IN ADDITION Eissler had to include material which the literary critic almost never is called on to handle: namely,



# FORTHCOMING



# NEW

## THEORY OF PSYCHOLOGICAL MEASUREMENTS

By **EDWIN GHISELLI**, *University of California, Berkeley*, McGraw-Hill Series in Psychology. Available in January, 1964.

Provides knowledge, at the level of an elementary course, of the problems, statistical techniques, and theoretical concerns basic to psychological testing and measurement of mental traits. In this text almost every formula developed is presented fully, and as simply as possible, to aid the student in understanding. The treatment of norms and standardization of scores, correlation, reliability and validity of measurement are presented comprehensively and emphasis is given to mathematical models and their uses. Integrated illustrative problems are included.

## PSYCHOLOGY IN MANAGEMENT, Second Edition

By **MASON HAIRE**, *University of California, Berkeley*. Available in January, 1964.

This thorough revision of a highly successful, readable book for the student of business and management deals with basic psychological issues in basic business problems. The Second Edition takes into account new fields, such as organization theory—and new thinking about business and psychology. An excellent book which may be used for courses or leisure-time reading with equal satisfaction.

## CREATIVITY: PROGRESS AND POTENTIAL

By **CALVIN W. TAYLOR**, *University of Utah*. Available in January, 1964.

An integrated, critical review to which the researcher, educator, or layman can turn for a useful, succinct overview of the field of creativity. The book summarizes the most important and current research findings in creativity and indicates further areas of study urgently in need of research.

## HANDBOOK OF MENTAL DEFICIENCY: Psychological Theory and Research

Edited by **NORMAN R. ELLIS**, *George Peabody College for Teachers*. McGraw-Hill Series in Psychology. 830 pages, \$14.50.

The first definitive presentation of all the significant theoretical approaches to the study of mental deficiency. Its purpose is to assess the status of behavioral research and theory in the field. The material is divided into two parts: Part I is devoted to the exposition and evaluation of theories of defectives' behavior. Part II summarizes the literature pertaining to the area of mental deficiency and evaluates the available data, pointing out the relevant aspects as well as the shortcomings.

## FIELDS OF APPLIED PSYCHOLOGY

By **ANNE ANASTASI**, *Fordham University*. Available in January, 1964.

A comprehensive and integrated picture of the professional activities of psychologists in business, industry, advertising and marketing, education, clinical practice, law, government, and the military. The approach emphasizes methodology, how psychologists operate in the various fields of applied psychology. Each topic is discussed and illustrated by relevant applied research.

## SYSTEMS AND THEORIES IN PSYCHOLOGY

By **MELVIN H. MARX**, *University of Missouri*; and **WILLIAM A. HILLIX**, *Navy Electronics Laboratory, San Diego*. McGraw-Hill Series in Psychology. 489 pages, \$8.95.

The primary purpose of this book is to provide the advanced undergraduate and the beginning graduate student in psychology with a single, up-to-date source containing the basic information about systematic and theoretical problems in psychology. The approach is scientific rather than subjective or clinical. The authors provide not only the basic tenets of various classical and contemporary viewpoints in psychology but also a philosophical framework within which the tenets can be evaluated.

## LABORATORY STUDIES IN OPERANT BEHAVIOR

By **JACK MICHAEL**, *Arizona State University*. 80 pages, \$2.95.

A laboratory manual describing exercises in operant behavior. Although designed to accompany *The Analysis of Behavior* by James Holland and B. F. Skinner with references to that text, the manual may be used with other textbooks. The exercises are planned so that they may be carried out by a single student working with one animal. The aim is to introduce psychology majors to laboratory techniques and to bring students into contact with behavior as an orderly experimental subject-field.

## EDUCATIONAL MEASUREMENT

By **JUM C. NUNNALLY**, *Vanderbilt University*. 472 pages, \$7.50.

A comprehensive treatment of tests and educational measurements, this new text is designed for use in basic undergraduate courses in teachers' colleges and departments of education. Although the book presents material on all phases of measurement, it is particularly slanted toward the needs of elementary and secondary classroom teachers. The text is founded on the premise that tests are used only if they are helpful in making educational decisions.

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analytical psychological studies of Goethe, which, while relatively rare, still are of utmost importance in so comprehensive a study as Eissler's.

Of the titles which Eissler omitted, the most important is the only pseudo-analytical study of Goethe by Felix A. Theilhaber, *Goethe*, sub-titled: *Sexus und Eros*, published in Berlin in 1929. Theilhaber has never gained favor with contemporary writers because of his one-sided approach of treating the Olympian, Goethe, from a psycho-sexual angle only; but then, Eissler would never do this, if his Study had been aimed at German readers: Germans never did embrace Freud. Eissler knew this, and hence his choice to address an English-speaking audience. The question, however, arises: since Eissler's book makes it mandatory for the reader to be well acquainted with both Goethe's life and works and the *Goetheforschung*, will the American reader be able to use this Study profitably? I doubt it.

In reacting to Eissler's book, one must bear with the analyst's whims. This is another way of saying that the writer has the privilege of letting the patient on the couch engage in free associations as much as the patient needs to and desires. Analyst Eissler can do so only by assuming that Goethe might have talked to him about certain incidents and experiences of his life. Thus Eissler chose the period from 1775 to 1786, i.e. from the 26th to the 37th year of Goethe's life, which included Goethe's *Italienreise* (Italian Journey), an experience that the latter generally regarded as starting a new stage of creativity.

The book is divided into four parts, which exemplify Eissler's practice in analyzing his patient Goethe. Part I, only three chapters, apparently is meant as a general commentary on individual and widely different episodes in Goethe's life. Part II deals with Goethe's arrival in Weimar, his two journeys to Switzerland, the death of Goethe's father in 1782 (it should be noted here that Eissler is probably the first writer to analyze the meaning of the father's death for Goethe, an important link hitherto almost completely lacking in contemporary *Goetheforschung*), and ends with Goethe's preparations for the Italian Journey. Part III, entitled "Solutions and New Problems," deals only with the Italian Journey. Part IV, finally, lets the reader look

behind the scenes, and, to my mind, constitutes the highlight of the entire study. Simply entitled Appendices, it contains 25 separate articles in small print, filling one half of the entire work. The reader might well ask, since Eissler spent nearly four hundred pages of small print (the equivalent of 800 pages, the two volumes containing a little over 1,500 pages) on appendices, why weren't they incorporated in the text? I find several possible reasons: as every psychologist knows from his daily practice, the "random" notes are often as important as the facts gathered from a patient during testing or therapy. They serve for later browsing, thinking, analyzing. Eissler has done the reader a service with these appendices. The reader can, if he so desires, start with the appendices (ranging in topics from the problems of relationship between psychosis and artistic creativity to chronologies and genealogies of Goethe, Karl August, et al.) and work his way back to the earlier parts of the book. In fact, that's the way I tried to read the set.

A FINAL word about Eissler's verdict: was Goethe mentally ill? In the text Eissler diagnoses one of Goethe's experiences (his breakdown while a student in Leipzig) as "paranoid schizophrenia." Some of us who have been close to Goethe's works may be shocked; or, if we are dyed-in-the-wool analysts, we may take this as a matter of course. But few will disagree with Eissler when he states in his Introduction: "Goethe cannot justifiably be classified under any of the headings of our textbooks of psychiatry or psychopathology; he is rather the representative of a special group that occupies a position parallel to the known classificatory groups of psychopathology" (p.xxxiv). Also: Goethe being a "representative of a special group," I find myself emotionally close to Eissler, when he states that "it was not only Goethe's own irresistible attraction that drew me, but the pleasure of living for a while in the world of the past . . ." Although there are many places in this work where I would disagree with Eissler, yet I find myself wishing I could have written this study myself. What more could a reviewer say about a work which he thinks will grow on him the more he reads it?

## Cooking up Conferences

Norman R. F. Maier and John J. Hayes

*Creative Management*. New York: John Wiley, 1962. Pp. v + 226.

Reviewed by JOHN MACMILLAN

The first author, Norman R. F. Maier, is the well known University of Michigan Maier, author of *Principles of Human Relations* (1952) and *The Appraisal Interview* (1958). The second author, John J. Hayes, is *Management Training Superintendent of the Education Training Center of United Airlines, Chicago*. John Macmillan, BA University of Toronto, PhD Cornell, is the reviewer. He taught at the University of Maryland before the war, spent the war years as Aviation Psychologist in Jack Jenkin's Navy, in 1946 went to the Office of Naval Research and was responsible there for administering vast funds in support of research in human relations, group performance, etc. In 1953 he returned home to Canada as a consultant to industry on human problems and in 1954 he took his present position as Director of Personnel and Staff Development, Canada Packers, Limited (which is not a football team) where he is responsible for personnel and for management development procedures in an organization with about 14,000 employees.

THE TITLE of this book sounds as though it might have been chosen to compete with the popular emanations of Packard, Whyte, Gross et al. This is mildly unfortunate because in reality it is a very worthwhile cookbook about conferences and was written by people who have tried the recipe.

The first four chapters describe the ingredients. Democratic principles, authoritarianism, creativity, conformity, communications, initiative and frustration are basic components and are ably treated. An aura of the Argvian stables





Coming, January 1964...

## **EDUCATIONAL PSYCHOLOGY**

An Application of Social and Behavioral Theory

by **LOUIS M. SMITH** and **BRYCE B. HUDGINS**

*both of Washington University at St. Louis*

This distinguished new textbook skillfully blends material from recent research and development in education, psychology, the behavioral sciences, and the humanities, with traditional subject matter. By discussing problems encountered by teachers in their classrooms, the authors face up squarely to one of the great dilemmas of the field—the practical application in the classroom of educational theory and the results of research.

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*both of the University of California, Berkeley*

This is without a doubt a superior text in every way. It is probably the most coherently written and theoretically consistent book in the field today. It presents some innovations, e.g. the "box" presentation of actual experiments and the integrated treatment of physiology, which should have been thought of years ago.

—Lee Sechrest, *Northwestern University*

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## **PSYCHOLOGY IN THE MAKING**

Histories of Selected Research Problems

Edited by **LEO POSTMAN**, *University of California, Berkeley*

The truly ingenious idea of presenting a series of "case studies" of problems produces a dramatic suspense inevitably absent in the usual histories of psychology. In each of the chapters, selected for their topical interest, a research question runs the gauntlet of evidence and counter-evidence...

—Rudolf Arnheim, *Sarah Lawrence College*

823 pages; \$9.00 text

## **INTRODUCTION TO THE STATISTICAL METHOD**

Foundations and Use in  
the Behavioral Sciences

by **KENNETH R. HAMMOND**, *University of Colorado*  
and **JAMES E. HOUSEHOLDER**, *Humboldt State College*

431 pages; \$7.00 text

**Alfred A. Knopf**, *Publisher*

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pervades the discussions and McGregorian chants can be heard faintly from the pantry, but a number of these concepts are more clearly related to the practical problem of conducting effective conferences than they have been in other publications. The authors' defence of management as a profession and the need for stimulation of non-conformity are well stated, while the descriptions of "playing it safe" behavior and the power factor in management will appeal to anyone who has ever been a member of a management group. The Conferencemanship ploy is a tidy vignette and the instructions on how to conduct a pseudo-democratic non-conference will strike a responsive chord in any experienced conferee.

CHAPTERS five to eight describe the kitchen and the testing of the recipe. Three conferences were held in which the same leader employed a different method of conducting each meeting. With the first group he used a problem-solving technique, the second was exposed to a pseudo-democratic type of leadership and the third to a "tell and sell" approach. Transcripts of the conferences are provided, with explanatory and analytical comments by the authors. At the conclusion of the meetings each participant was asked to complete a questionnaire about the leadership of the conference, the influence each person had on the decision making process, how well the group functioned as a whole, and how upward communications were handled.

The answers indicate that the members of the first group felt that they had really participated in the decision making process but that the second and third groups were aware of the manipulative nature of the meetings and resented it. Objective data on the frequency of speech by leader and participants seem to confirm these findings since they show that there was a progressive decrease in participation from the first to the third meeting.

All members were told about the experimental nature of the conferences at the end of the meetings and the design was fully explained. However, a follow-up one year later showed that members of group two and three re-

membered primarily the manipulative aspects of the sessions and had not forgiven the leader for his attempted deception. The authors' rather frightening conclusion is that "the price of every non-conference is a mortgage on the future."

THE NEXT two chapters contain additional cooking instructions and methods of testing the recipe. A five step procedure for conducting problem-solving conferences is outlined and the difficulties of upgrading the quality and acceptance of decisions are explained in some detail. When E stands for effectiveness, Q for quality and A for acceptance,  $E = Q + A$ . Thus when either Q or A becomes zero the effectiveness of the decision is also zero. Several applications of this formula are given, including the overworked Harwood Manufacturing study. However, the importance of the acceptance of a decision in getting something done about it is most properly heavily emphasized. The lack of factual data only

persuades one that further testing of the formula is indicated.

The final chapter, on introducing creative management, is an appeal to managers to try the recipe. A functional organization chart similar to Likert's linking-pin structure is illustrated and recommended as ideal for promoting participative management. The democratic ethic is described as an expanding moral force in our society and the necessity for the application of democratic principles in industry is propounded.

The authors recognize individual differences and the need for flexibility in dealing with them in management as elsewhere. They stress the problems and difficulties in promoting change and do not offer their formula as a panacea for management's troubles but as one which has had some practical value and is worth trying in other settings.

The experiments described should be replicated using similar ingredients in different kitchens, but the recipe as it stands is well worth tasting both by managers and psychologists in industry.

## Injured Young Czech Brains

Otakar Kučera and coworkers

*Psychopatologické Projevy při Lehkých Dětských Encefalopatiích* (Psychopathologic Manifestations in Children of Mild Encephalopathies). Prague: State Health Publishing House, 1961. Pp. 260, 22 pictures. 34,60 kcs.

Reviewed by L. HOSKOVCOVÁ and J. HOSKOVEC

The first author here, Otakar Kučera, is psychiatrist and chief physician with Prague's Psychiatric Polyclinic for Children and Adolescents, an out-patient department of the Regional Health Institute of Central Bohemia. His coworkers are listed and described briefly in the review. The reviewers are a husband and wife team, L. Hoskovcová and J. Hoskovec. L. Hoskovcová, an MD, has specialized in child psychiatry. At present she is on the staff of the Psychiatric Hospital in Bohnice near

Prague. J. Hoskovec, PhD, is in charge of archives of diagnostic material at the Institute of Psychology, Charles University, Prague. He served for some time as psychological diagnostician at the Special Home for Children at Dobřichovice.

THIS publication is a fruit of many years of experience with children who suffer from mild encephalopathies. It is based on observations made at



the Children's Psychiatric Sanatorium in Dolní Počernice near Prague and at the Psychiatric Clinic for Children and Adolescents in Prague.

The monograph is a result of the cooperation of an eight-member team headed by two child psychiatrists, O. Kučera and E. Stěrbáková, and consisting of psychologists (J. Jirásek, Z. Matějček), a pediatrician (K. Macek), a neurologist (J. Dittrich), a speech therapist (Z. Žlab), and an educator (J. Poledne).

The authors' original aim was practical: to present, in a systematic manner, their observations concerning therapy of children with mild encephalopathies. But in trying to find more objective diagnostic criteria and to increase therapeutic effectiveness, the depth-dimension of their explorations increased and forced them to attempt to grasp the basic nature of the problem from the anatomical, physiological and psychiatric points of view.

"Mild encephalopathies" refer to small diffuse lesions of the cerebral tissue, of a permanent nature, acquired pre-, peri-, or post-natally. The disorder is "mild" in the sense that the motor activity of the child is not obviously affected and the child's intelligence is not grossly impaired. By contrast, there are marked disturbances in perception, thought processes, and outward behavior. These primary disorders are the basis for a whole series of diverse psychopathological manifestations which form the actual subject of the work.

In view of the fact that the terminology of organic brain disorders lacks uniformity, the reader will welcome the introductory chapter devoted to a critical analysis of terms and concepts.

The diagnosis of a mild encephalopathy in children is based on three sets of criteria: 1) the psychiatric clinical data, supplemented by case history, 2) somatic findings (pediatric, neurological, speech examinations, and tests of motor function), and 3) psychological findings. With respect to localization of underlying brain lesions, the behavioral disturbances—hyperkinesia, distractibility, impulsiveness—point to impairment of the ascending reticular activation system.

Out of a total of 901 children treated during the years 1954-1958, a selection was made of 186 cases with a diagnosis of mild encephalopathy. In 100 children all the necessary investigations were completed. These 100 cases were carefully classified according to diagnosis and according to etiology. It is unfortunate that electroencephalographic investigations could be carried out only in 22 children.

**T**HE DIAGNOSIS of mild encephalopathies is greatly aided by a thorough psychological examination, including analysis and evaluation of behavior in free life situations, examination of performance in standard test situations, and appraisal of basic mental functions by means of specially designed laboratory tests.

Results obtained by a series of tests (the Terman-Merrill test, the Grace Arthur test, the Wechsler Intelligence Scale for Children, Raven's Progressive Matrices, the Kohs Block test, the Goodenough Draw-a-Man test, Strauss' Marble Board test, the Figure-Background test, Matějček's visuo-motor test of "drawing a progressive series of designs," and others) are discussed, indicating the signs which are characteristic of children with mild encephalopathy. Particular attention is devoted to a qualitative analysis of the results of the Kohs Block test. Children with mild encephalopathy are characterized by a primitive impulsive manner of work rather than by a low numerical score. Characteristic also are the primitive drawings in the Goodenough Draw-a-Man test and in Matějček's visuo-motor test. On the other hand, Jirásek's test of practical knowledge and general information shows no impairment. The difference between the results of this test and the Goodenough test are of great practical value for the diagnosis of mild encephalopathy.

The authors deal extensively with the psychotherapeutic guidance of children and parents and with the role of the teacher of the encephalopathic child. This part of the book is of definite practical importance.

The supplement contains the forms for observing deviations in the behav-

ior, items from Jirásek's test of practical knowledge and general information, and instructions for investigations of laterality and Matějček's visuo-motor test.

The bibliography deserves critical comment. It is divided into four parts each of which was supplied by different authors. A single unified bibliography would have been more useful. Some references are incomplete. A monograph of this kind should contain a subject and author index. A welcome feature is the detailed summary in English and Russian.

In conclusion it may be said, with regard to the psychological aspects of the monograph, that it will be extremely valuable not only to psychologists who study children with mild encephalopathy but also to others who are interested in using psychological methods in psychopathology. The principal author, Dr. O. Kučera, is an excellent teacher and organizer. This organizational talent was invaluable in the planning of this publication. In collaboration with a group of competent specialists he has succeeded in producing a book which treats the theme comprehensively and in depth.

*(This review was prepared in the frame of activities supported by the National Science Foundation grant G19469, awarded to Dr. Josef Brozek.)*



*I do not wish to hide the fact that I can only look with repugnance . . . upon the puffed-up pretentiousness of all these volumes filled with wisdom, such as are fashionable nowadays. For I am fully satisfied that . . . the accepted methods must endlessly increase these follies and blunders, and that even the complete annihilation of all these fanciful achievements could not possibly be as harmful as this fictitious (psychological) science with its accursed fertility. —IMMANUEL KANT*





# ON THE OTHER HAND



## TELEVISION, CHILDREN, ERRORS

The contretemps over the review (*CP*, June 1963, 8, 248f) of the pamphlet publication of four chapters of *Television and the Child*, about which Dr. Himmelweit protests in her letter to your journal is a result of a series of confusions between the ethics and manners of scientific publishing and wider writing for the general public. The first error was I believe made by the authors of the complete study, which Dr. Himmelweit documents in her letter, in letting chapters of their book be published and circulated in a form which she had not fully investigated. But they were published, the omissions against which she protests were made, and the pamphlet was receiving wide circulation. I reviewed it as a unit as I would review any publication that stood on its own, with a complete careful reference to the longer study.

The second error was made by my office. *Contemporary Psychology* did not ask me to review this pamphlet at all. Your editorial office, in a letter from Professor Lumsdaine, dated June 26, 1963, asked me to review a pamphlet called *Television for Children*, Foundation for Character Education, Boston, 1962. This pamphlet arrived in my office in an unlabeled envelope, simultaneously with the pamphlet edition of part of Dr. Himmelweit's book, and both pamphlets were filed in the folder marked for the *Contemporary Psychology* review, and only one—*Television and the Child*—was given to me to take with me on a trip on which I planned to write the review. After the review had been written and sent off to *Contemporary Psychology*, my secretary discovered the error and I immediately telephoned your office, saying that I had reviewed the wrong pamphlet, that I felt sure that you had reviewed the whole book, (as indeed you had, E. Macoby, *CP*, 1959, 4, 357) and that I thought you should check the matter, and I, myself, would of course be glad to withdraw the review. I had enjoyed do-

ing it and thinking over the problems that it raised. You said, with, I am afraid, more gallantry than caution, my dear editor, never mind, you were delighted to have it. And there the matter ended. Incidentally, a second injustice was done because *Television for Children* was never reviewed by *CP*.

The episode suggests the dangers and pitfalls which attend any mixture of scientific and popular writing, publication and reputation, a mixture with which neither the United States nor the United Kingdom are very able to deal. I recently had my *Growing up in New Guinea*, reissued by Pelican Press in the United Kingdom with a new jacket, picture, a phoney photograph of a child from another area, caressing a carving done by quite different people; the whole thing a travesty on responsible scientific work. I had never been consulted at all.

The episode has given Dr. Himmelweit an opportunity to set the record straight on the pamphlet, which I assume she had not done before, and to express any other objections which she had to my attempt to do justice to an edition of her work which I assumed she had authorized.

MARGARET MEAD  
*American Museum of Natural History*

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# CONTEMPORARY PSYCHOLOGY

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Also how excellent, that it is so, and that the deepest depths of our soul are hidden in the night! Our poor thinking organ would certainly not be able to seize every stimulus, the seed of every sensation, in its ultimate elements, or to hear aloud such a soaring ocean of dark waves, without shuddering with anxiety and in fear and cowardice letting the rudder go from its hands. So mother nature took away from it whatever could not be faced by its clear consciousness, weighed every impression that it might receive, and carefully organized every channel leading to it. Now it does not analyze the roots, but enjoys the blossoms; scents float to it from the dark bushes that it neither planned nor nourished; it stands on an abyss of infinity, and knows not that it is there; thus, through this happy ignorance, it stands firm and steady.

—J. G. UHERDER



## SWEET MADNESS

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